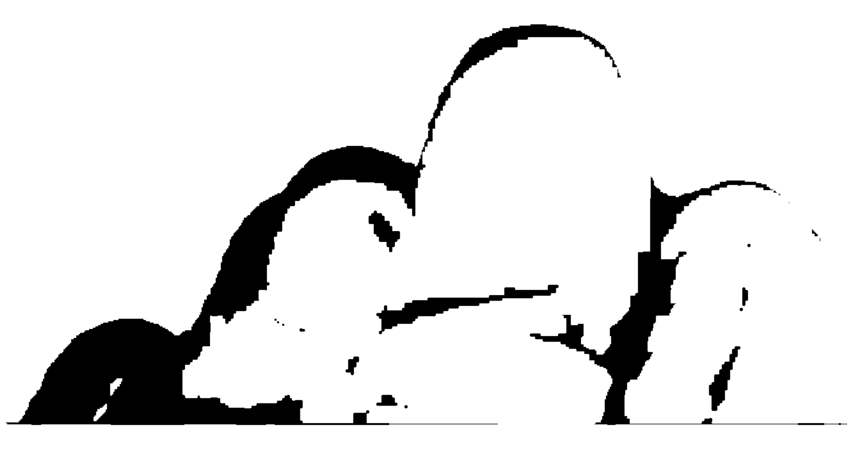

ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION
OF THE INSTITUTION

FOR

THE YEAR ENDING JUNE 30, 1899.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1901.

LETTER

FROM THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

ACCOMPANYING

The Annual Report of the Board of Regents of the Institution for the year ending June 30, 1899.

SMITHSONIAN INSTITUTION,
Washington, D. C., June 1, 1900.

To the Congress of the United States:

In accordance with section 5593 of the Revised Statutes of the United States, I have the honor, in behalf of the Board of Regents, to submit to Congress the Annual Report of the operations, expenditures, and condition of the Smithsonian Institution for the year ending June 30, 1899.

I have the honor to be, very respectfully, your obedient servant,

S. P. LANGLEY,
Secretary of Smithsonian Institution.

Hon. WILLIAM P. FRYE,
President pro tempore of the Senate.

ANNUAL REPORT OF THE SMITHSONIAN INSTITUTION FOR THE YEAR ENDING JUNE 30, 1899.

SUBJECTS.

1. Proceedings of the Board of Regents for the session of January 25, 1899.

2. Report of the executive committee, exhibiting the financial affairs of the Institution, including a statement of the Smithson fund, and receipts and expenditures for the year ending June 30, 1899.

3. Annual report of the Secretary, giving an account of the operations and condition of the Institution for the year ending June 30, 1899, with statistics of exchanges, etc.

4. General appendix, comprising a selection of miscellaneous memoirs of interest to collaborators and correspondents of the Institution, teachers, and others engaged in the promotion of knowledge. These memoirs relate chiefly to the calendar year 1899.

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REPORT OF THE SECRETARY.

GENERAL APPENDIX.

LIST OF PLATES.

THE SMITHSONIAN INSTITUTION.

MEMBERS EX OFFICIO OF THE "ESTABLISHMENT."

WILLIAM MCKINLEY, President of the United States.
GARRET A. HOBART, Vice-President of the United States.
MELVILLE W. FULLER, Chief Justice of the United States.
JOHN HAY, Secretary of State.
LYMAN J. GAGE, Secretary of the Treasury.
RUSSELL A. ALGER, Secretary of War.
JOHN W. GRIGGS, Attorney-General.
CHARLES EMORY SMITH, Postmaster-General.
JOHN D. LONG, Secretary of the Navy.
E. A. HITCHCOCK, Secretary of the Interior.
JAMES WILSON, Secretary of Agriculture.

REGENTS OF THE INSTITUTION.

(List given on the following page.)

OFFICERS OF THE INSTITUTION.

SAMUEL P. LANGLEY, *Secretary,*

Director of the Institution and of the U. S. National Museum.

RICHARD RATHBUN, *Assistant Secretary.*

REGENTS OF THE SMITHSONIAN INSTITUTION.

By the organizing act approved August 10, 1846 (Revised Statutes, Title LXXIII, section 5580), and amended March 12, 1894, "The business of the institution shall be conducted at the city of Washington by a Board of Regents, named the Regents of the Smithsonian Institution, to be composed of the Vice-President, the Chief Justice of the United States, three members of the Senate, and three members of the House of Representatives, together with six other persons, other than Members of Congress, two of whom shall be resident in the city of Washington and the other four shall be inhabitants of some State, but no two of the same State."

REGENTS FOR THE YEAR ENDING JUNE 30, 1899.

The Chief Justice of the United States:
MELVILLE W. FULLER, elected Chancellor and President of the Board January 9, 1889.

The Vice-President of the United States:
GARRET A. HOBART (March 4, 1897).

	Term expires.
United States Senators:	
JUSTIN S. MORRILL (appointed Feb. 26, 1883, Mar. 24, 1885, Dec. 15, 1891, and Mar. 15, 1897). Died Dec. 28, 1898.	
SHELBY M. CULLOM (appointed Mar. 24, 1885, Mar. 28, 1889, and Dec. 18, 1895)	Mar. 3, 1901
GEORGE GRAY (appointed Dec. 20, 1892, and Mar. 20, 1893). Term as Senator expired Mar. 4, 1899.	
ORVILLE H. PLATT (appointed Jan. 18, 1899)	Jan. 18, 1905
WILLIAM LINDSAY (appointed Mar. 3, 1899)	Mar. 3, 1905
Members of the House of Representatives:	
JOSEPH WHEELER (appointed Jan. 10, 1888, Jan. 6, 1890, Jan. 15, 1892, Jan. 4, 1894, Dec. 20, 1895, and Dec. 22, 1897)	Dec. 22, 1899
ROBERT R. HITT (appointed Aug. 11, 1893, Jan. 4, 1894, Dec. 20, 1895, and Dec. 22, 1897)	Dec. 22, 1899
ROBERT ADAMS, JR. (appointed Dec. 20, 1895, and Dec. 22, 1897)	Dec. 22, 1899
Citizens of a State:	
JAMES B. ANGELL, of Michigan (appointed Jan. 19, 1887, Jan. 9, 1893, and Jan. 24, 1899)	Jan. 24, 1905
ANDREW D. WHITE, of New York (appointed Feb. 15, 1888, and Mar. 19, 1894)	Mar. 19, 1900
WILLIAM PRESTON JOHNSTON, of Louisiana (appointed Jan. 26, 1892, and Jan. 24, 1898)	Jan. 24, 1904
Citizens of Washington:	
JOHN B. HENDERSON (appointed Jan. 26, 1892, and Jan. 24, 1898)	Jan. 24, 1904
WILLIAM L. WILSON (appointed Jan. 14, 1896)	Jan. 14, 1902
ALEXANDER GRAHAM BELL (appointed Jan. 24, 1898)	Jan. 24, 1904

Executive Committee of the Board of Regents.

J. B. HENDERSON, *Chairman.* WILLIAM L. WILSON.
ALEXANDER GRAHAM BELL.

PROCEEDINGS OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION,

AT THE ANNUAL MEETING HELD JANUARY 25, 1899.

In accordance with a resolution of the Board of Regents, adopted January 8, 1890, by which its stated annual meeting occurs on the fourth Wednesday of January, the Board met to-day at 10 o'clock a. m.

Present—The Chancellor (Mr. Chief Justice Fuller) in the chair; the Hon. S. M. Cullom, the Hon. George Gray, the Hon. O. H. Platt, the Hon. R. R. Hitt, the Hon. John B. Henderson, Dr. William L. Wilson, Dr. James B. Angell, Dr. A. Graham Bell, and the Secretary, Mr. S. P. Langley.

Excuses for nonattendance on account of illness were received from the Vice-President and Mr. Adams.

At the Chancellor's suggestion the Secretary read the minutes of the last meeting in abstract (explaining, in reference to the Board's authority to use a portion of the Hodgkins fund in his aerodynamic experiments, that he had not used such permission).

There being no objection, the Chancellor declared the minutes approved.

The Secretary announced the death of Senator Justin Smith Morrill, and Dr. Wilson offered the following resolutions:

Whereas the Board of Regents of the Smithsonian Institution are called upon to mourn the death, on December 28, 1898, of Justin Smith Morrill, for fifteen years a member of the Board, and to some members of it a still older colleague in the Senate of the United States:

Resolved, That the Board desire to place on record the expression of their sense of the exceptional loss they have sustained in the death of their venerable colleague; and that they unite with their fellow-citizens throughout the land in recognizing the great services of Senator Morrill to the whole country during a public career of forty-three years in Congress, where, amid other great national services, his public life in the special domain of education alone was the most important of that of any single American. With all these duties his time, his ripened knowledge of practical affairs, and his counsel were always at the service of the Smithsonian Institution, where no member of this Board represented its interests in Congress more persistently or more effectually than he.

Resolved, That by his personal character, no less than by his mental endowments, he endeared himself to all his associates on this Board, who feel that they have lost in him, not only a counselor and adviser, but a dear and honored friend; and that,

without desiring to intrude upon the private grief of his family, they wish to express to them their share in their sorrow.

Resolved, That this minute be entered as a part of the journal of the Board and a copy be transmitted to the family of Senator Morrill.

Ex-Senator Henderson said:

I feel a personal loss in the death of Senator Morrill, whom I had known somewhat intimately from 1862, and with whom I had been associated, more or less, in public life, when I was a young man.

He continued:

Mr. Chancellor, the deceased statesman, Mr. Morrill, is now perfectly secure in his well-earned fame. It was said by one of the Latin poets that no man should be esteemed happy before his funeral. In this case the sad rites have been performed, and to the end of a most useful life he kept the faith.

In his career we have an illustration of the beauty and excellency of our republican institutions. His education was limited, but his honesty and patriotism had no bounds. He did what conscience dictated to be done and put his trust in those he served. He loved, not license, but liberty as defined by Cicero, "the power to do what the law permits." He was true to his constituents, and in return they gave him those priceless gifts of freemen—their gratitude and fidelity.

He represented a State small in population and wealth, but rich in the character of its people and rich in the long line of able, pure, and distinguished statesmen she has given to the national councils. The horizon of his usefulness, like that of Collamer, Foote, and Edmunds, extended beyond the State of Vermont. He was broad as the Union itself.

From 1855 to 1867 he was a member of the House of Representatives. In 1861 private industries had become prostrated and bankruptcy threatened the National Treasury. Secession had already commenced the work of dissolution when he prepared and pressed to enactment the tariff laws of that year. Waiving the question of protection, the necessity for revenue alone demanded its passage, and the beneficent results gave national reputation to its author. He came to the Senate in 1867, and continued a member of that body until the date of his death, having received six successive elections by the legislature of his State. In the Senate he stood at all times for a sound currency. He had deplored the original issue of United States notes in 1862, and, true to his convictions of right, in after years he consistently demanded the performance of the nation's pledge that they be redeemed and canceled. He believed with all his heart that the gold dollar should measure values throughout the commercial world, and, unmoved by the clamors of hard times, he persevered in his faith until the fulfillment of his prophecies has broken the stubborn unbelief of millions.

Through his efforts the statues of distinguished Americans now adorn the old representative hall of the Capitol, where his own so well deserves to be placed.

Largely through his exertions came the building constructed for the State, War, and Navy Departments, and the last legislative act of his life was to provide a building for the use of the Supreme Court of the United States. To us who knew him so well it brings profound pleasure that he lived to see the Library building completed, which not only in its purposes but in the splendor of its architecture does honor to the nation. Somewhere within its walls a modest tablet should at least connect his name with this magnificent structure. It is no less a monument to his memory than is St. Paul's Cathedral to the memory of Sir Christopher Wren.

For the last eighteen years he was a Regent of this Institution, ever watchful of its interests and prompt to increase its usefulness. In the grandeur of his country he felt the patriot's pride. He sought to make its capital city worthy of the people

to whom it belonged. And the Smithsonian Institution was regarded by him as a chief factor in its future greatness and renown.

My acquaintance with Mr. Morrill began in 1862. In my earlier days I enjoyed his counsel and instruction in public affairs. He was, in my judgment, the true American nobleman. Here as elsewhere distinction imposes increased obligations—noblesse oblige. No books of heraldry and no blazoned emblems are necessary to evidence the rank of Senator Morrill. His patent of nobility is recorded in the hearts of a grateful people.

Dr. Angell said:

I was for some years president of the University of Vermont, of which Mr. Morrill was a trustee, and it gives me pleasure to say that during the time of my connection there, his duties were just as conscientiously and studiously performed for the university, small as it was, as they have been for this Institution. I think I can safely say that no man in the State of Vermont had a stronger hold on the hearts of its people than he.

The Secretary felt it difficult to speak of one who was to him so dear and personal a friend, and declined to make any remarks.

Senator Cullom then moved the adoption of the resolutions by a rising vote, which motion was carried.

The Secretary announced the appointment on January 18, 1899, by the President of the Senate, of the Hon. O. H. Platt, of Connecticut, to succeed Senator Morrill, and the election of Dr. J. B. Angell, to succeed himself, by joint resolution, approved January 24, 1899.

The Secretary presented his annual report of June 30, 1898, remarking that it had already been sent to the Regents, and that he would only ask them to look at one or two matters; for instance, at the map opposite page 26, showing the extent of the correspondence which the Institution had in connection with its exchanges; and he also asked their further attention to the illustrations in the report showing some views in the National Zoological Park.

On motion, the report was accepted.

Mr. Henderson then submitted the annual report of the Executive Committee to June 30, 1898, which, on motion was adopted.

Mr. Henderson offered the following customary resolution relative to income and expenditure, which was adopted:

Resolved, That the income of the Institution for the fiscal year ending June 30, 1900, be appropriated for the service of the Institution, to be expended by the Secretary, with the advice of the Executive Committee, with full discretion on the part of the Secretary as to items.

Mr. Henderson, as chairman of the Permanent Committee, made the following statements concerning the Hodgkins and the Avery bequests:

CONDITION OF THE HODGKINS FUND AT THE CLOSE OF THE YEAR 1898.

There have been no transactions during the year affecting the status of the Hodgkins fund, except as shown in the statement of the condi-

tion of the fund, and the receipts and disbursements during the year, contained in the report of the executive committee of the Regents.

CONDITION OF AVERY FUND AT CLOSE OF THE CALENDAR YEAR 1898.

By a decision of the United States Supreme Court the title to certain real estate on Capitol Hill, owned by Robert Stanton Avery at the time of his decease, and adversely claimed by the heirs of Mrs. Avery, has been adjudged to be rightfully in the Smithsonian Institution. This completes all litigation in connection with the Avery bequest, the status of which is now entirely settled.

The assessed valuation of the real estate now vesting in the Institution is $21,086. The Commissioners of the District of Columbia have during the past year directed the cancellation of certain arrearages of taxes charged against the property, and have relieved it from future taxation.

The personal estate is estimated to be worth $2,915.87. It consists of fifteen shares of stock, one gold bond, and a balance of $85.87 delivered over by Miss Avery in the settlement of her account as executrix to the National Safe Deposit, Savings and Trust Company, of Washington City, to be held in accordance with the provisions of the will of the testator, in trust for Miss Avery during her lifetime, and upon her demise to become the property of the Institution. The trust company has been designated as trustee by a decree of the equity court.

The revenue now derived from this bequest consists solely of the receipts for rent of real estate, amounting in gross to $606 annually. From this is to be deducted the cost of repairs, commissions, etc., which has lately been at the rate of about $300 annually, leaving a net revenue of about $300 a year.

Mr. Henderson concluded the report with the remark that the showing illustrated the necessity for the sale of the real estate, as provided for in the resolution adopted at the last meeting.

On motion, the reports were accepted.

The Secretary then made a statement as to the current affairs of the Institution under separate heads, a form which he explained he adopted with the hope that the Regents would express in each case any opinions they might have for his better instruction.

SECRETARY'S STATEMENT.

ADMINISTRATION OF THE NATIONAL MUSEUM.

The resignation of Mr. Charles D. Walcott as Acting Assistant Secretary of the Smithsonian Institution in charge of the National Museum, which was announced at the last meeting of the Regents, took effect on June 30, 1898. In the report of the Secretary, now before the Regents, a statement is furnished as to the administrative changes made in the Museum, by gathering the different departments under three

heads, an arrangement which has rendered the administration of the Museum possible without making its oversight an impossible burden upon the Secretary.

This plan of head curatorships is [he said] for the present working well, but I should prefer to give it another year's trial before saying that it can be considered a suitable plan for the permanent administration of the Museum. In accordance with the resolution modifying the terms of the appointment of Mr. Richard Rathbun as Assistant Secretary, he has been enabled to aid me in the administration of Museum affairs.

NEW BUILDING FOR THE NATIONAL MUSEUM.

The question of an additional building for the National Museum is one which has been frequently referred to, and which, I may remind the Regents, had the very special sympathy of their departed colleague, Senator Morrill.

The present building was erected at an entire cost of $315,000. It has an exhibition space of 96,000 square feet, and is the cheapest building of its size ever erected for museum purposes.

The growth of the Museum is extraordinary, when the mere number of specimens is considered without reference to their arrangement or value. Before the present Museum building was fairly completed, it was found inadequate to holding all the collections, then numbering between one and two millions, and a considerable part has remained in the Smithsonian building, which was built entirely at the private cost of the Smithson fund, and which the Government has continued to use up to the present time, rent free, by the permission of the Regents, for this purpose.

There are now between four and five million specimens, occupying a floor space which is only adequate for one-third of that number, and the rate of increase is itself increasing by bounds, more than 400,000 specimens having been added during the past year.

This great increase is not, however, cause for unmixed congratulation, for while some of the recent additions represent collections complete in themselves, as a whole they are not readily assimilable. They were not as a rule selected by the donors on any deliberate system, but are separate and unrelated gifts, too valuable in themselves for the Government to refuse, but which must be joined under some system, and can not be effectively used unless means are provided to fill the gaps between them by purchase, and make them continuous.

With some notable exceptions, then, the increase of collections is in the direction of a miscellaneous and fragmentary mass, which can only be fully developed and coordinated by purchases to fill the gaps, and there are almost no means for such purchases.

Although the Museum is now so congested that the contents are rather packed than displayed, it must not be supposed that the lack of space is the only imperative demand. Unless some additional means are provided by Congress for unifying these collections, I feel it my duty to state to the Regents that, even without reference to the building question, the Museum under their charge, which is still in the front of such American institutions, can not longer remain so.

The need of a new building is one quite apart from any enlargement in scope or future development, though questions of that nature are also presenting themselves for solution. Among these I may mention the very earnest recommendation of Mr. Walcott for a Museum of Practical Geology, the demand for which, he tells me, is every day becoming stronger.

I have laid these needs of the Museum before the Board thus briefly, to invite an expression of opinion as to whether the time is opportune for presenting them to Congress, and if so, to especially invite the help of the Regents in their accomplishment.

A discussion here arose, participated in by the Chancellor, Senators Platt and Quay, Mr. Hitt, and Mr. Henderson. In answer to questions, the Secretary explained the apparent anomaly of asking money to purchase collections at a time when the Museum had not space to provide for those it possessed, and he asked the Senatorial Regents to consider the desirability of interesting the Senate Appropriations Committee in some matters which the House committee had not apparently adequately considered. He named $25,000 as the lowest sum which it was desirable to have in addition to the $165,000 on the appropriation for preservation of collections.

The Secretary spoke of the attitude of Congress with regard to salaries in the Institution, and Mr. Hitt explained that the attitude of the Appropriations Committee with regard to all requests for salaries was due to the fact that experience had shown in the immense majority of cases that these requests had a personal motive. He thought it was to be regretted that those of the Institution were not discriminated from such a class, as they seemed to deserve.

Remarks to the same effect were made by other Regents, and the result of the discussion seemed to be a general expression of feeling among the Regents that the appointments by the Institution had been so markedly free from political or personal influence, that it was much to be desired that Congress should appreciate the fact and recognize it in reposing a larger confidence in the management of the Institution where the salaries were in question.

EXPLORATIONS OF NEW TERRITORY.

The Secretary proceeded:

Ever since there seemed to be a possibility of acquiring new territory as a result of the late Spanish-American war, I have had in mind the advisability and propriety of the sending out by the Institution of scientific parties under its various bureaus to conduct inquiries as to the natural history, geology, ethnology, archæology, and scientific activities of these countries. I understand that the political results of the war are not so thoroughly determined as to make this an opportune time to take the question up, but I hope that by next year Congress will be prepared to consider the matter and to listen to definite proposals concerning it.

COOPERATION OF NAVY AND ARMY IN COLLECTING ANIMALS FOR THE ZOOLOGICAL PARK.

The law establishing the Zoological Park states: "That the heads of the Executive Departments of the Government are hereby authorized and directed to cause to be rendered the necessary and practicable aid to the said Regents in the acquisition of collections in the Zoological Park."

I have this year addressed letters to the Secretary of War and the Secretary of the Navy, asking if it would meet their approval if letters were addressed to officers of the Army and Navy asking them to cooperate in this work. I have received in response to this a cordial reply from the Secretary of the Navy assenting to this proposition.

The Secretary gave an account of the experiments in aërodromics which are being conducted for the Board of Ordnance and Fortification, War Department.

Some general remarks were made by the Regents, among others by Mr. Bell, who said that he had been a witness of these experiments, which the Secretary had raised to a scientific plane. He was gratified by the action of the War Department, and by the prospect of the useful application of the principles established.

The Chancellor remarked that the Board was now fully informed as to what the Secretary wished, and he presumed their assent would be understood to be given without the need of a formal resolution.

HODGKINS MEDAL.

The Secretary continued:

While it does not seem advisable that the Hodgkins fund should be chiefly employed in giving medals, it was unquestionably the wish of its founder that it should be thus used to some extent, at least in the early years of its administration, and I have, after consulting scientific counsel, concluded to, with the sanction of the Regents, bestow the gold medal of the Institution on Prof. James Dewar, of the Royal Institution, for his researches on the solidification of atmospheric air, and useful discoveries in that connection.

The Secretary added his regret that this gold medal, which was being struck at the Paris mint, had not arrived in time to be shown to the Regents. He exhibited some other gold medals, one of them by the same medalist, and answered the questions of the Regents about them.

LAW PROTECTING ARCHÆOLOGICAL SITES.

The Secretary stated that as time was pressing, he would not read at length the form of proposed laws for protecting archæological sites and for preventing the forgery of antiquities, but that since the resolution he had to present in no way committed the Regents to anything further than an expression of opinion that some such legislation was desirable, he would simply submit the resolution, as follows, which, on motion, was adopted:

Resolved, That the Board approves in principle the passage of laws protecting archæological sites and making the uttering of forged objects a misdemeanor, and that the Regents in Congress be requested to endeavor to secure the passage of suitable laws covering these matters.

CIVIL SERVICE.

The Secretary went on to say—

At their last meeting the Board of Regents resolved:

That the Secretary be instructed to request of the President such a modification of the civil service regulations relating to appointments as will permit an exemption of such scientific positions under the Smithsonian Institution as the Secretary may deem best for the interests of the Institution.

The Chancellor said that the President had the matter under advisement. He knew the general tenor of what the Board desired, and he (the Chancellor) assumed that the matter would be taken up in order.

Senator Platt here remarked that he had reason to believe that the President would not issue such an order until after the adjournment of the present Congress.

NATIONAL UNIVERSITY.

After some further explanations to the Board, the Secretary went on to say that he had reserved until the close a subject on which it would seem necessary for the Regents to adjudicate with full deliberation.

He referred to the projects for a National University, and speaking in particular of those which sought directly to enlist the aid of the Smithsonian Institution in their plans, he said that while all the classes seemed desirous to obtain this aid, it was specially sought by those connected with the agricultural and State colleges, directly brought into existence by the Government.

He then read a letter from Mr. George E. MacLean, the secretary of the committee on graduate study at Washington of the American Association of Agricultural Colleges and Experiment Stations, and presented with it a report describing clearly the wishes of the applicants and requesting that Congress should be asked to make an annual appropriation of at least $25,000 for their purposes.

The Secretary said he would not read his reply, which was in substance that he could not act on such a matter without asking the instructions of the Regents. He had promised to lay the whole matter before them, which he now did.

This led to a very full discussion among the Regents, and after remarks by the Chancellor, and other members of the Board, Senator Henderson offered the following resolution, whose form he understood to have been arranged by his colleagues on the executive committee, Dr. Bell and Dr. Wilson, together with the Secretary, the latter having, at his own request, been excused from service with the proposed committee:

Resolved, That the communication from the committee representing the agricultural colleges of the United States be referred to a committee of Regents, to be appointed by the Chancellor to consider the same and all kindred questions, and to make a report thereon at the next meeting of the Board.

The Chancellor announced the committee as follows: Mr. Henderson, Dr. Wilson, Mr. Bell, Dr. Angell, and Mr. Hitt.

There being no further business to come before the Board, on motion it adjourned.

REPORT OF THE EXECUTIVE COMMITTEE OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION

FOR THE YEAR ENDING JUNE 30, 1899.

To the Board of Regents of the Smithsonian Institution:

Your Executive Committee respectfully submits the following report in relation to the funds of the Institution, the appropriations by Congress, and the receipts and expenditures for the Smithsonian Institution, the U. S. National Museum, the International Exchanges, the Bureau of Ethnology, the National Zoological Park, and the Astrophysical Observatory for the year ending June 30, 1899, and balances of former years:

SMITHSONIAN INSTITUTION.

Condition of the fund July 1, 1899.

The amount of the bequest of James Smithson deposited in the Treasury of the United States, according to act of Congress of August 10, 1846, was $515,169. To this was added by authority of Congress, February 8, 1867, the residuary legacy of Smithson, savings from income and other sources, to the amount of $134,831.

To this also have been added a bequest from James Hamilton, of Pennsylvania, of $1,000; a bequest of Dr. Simeon Habel, of New York, of $500; the proceeds of the sale of Virginia bonds, $51,500; a gift from Thomas G. Hodgkins, of New York, of $200,000 and $8,000, being a portion of the residuary legacy of Thomas G. Hodgkins, and $1,000, the accumulated interest on the Hamilton bequest, making in all, as the permanent fund, $912,000.

The Institution also holds the additional sum of $42,000, received upon the death of Thomas G. Hodgkins, in registered West Shore Railroad 4 per cent bonds, which were, by order of this committee, under date of May 18, 1894, placed in the hands of the Secretary of the Institution, to be held by him subject to the conditions of said order.

Statement of the receipts and expenditures from July 1, 1898, to June 30, 1899.

RECEIPTS.

Cash on hand July 1, 1898		$65, 803. 02	
Interest on fund July 1, 1898	$27, 360. 00		
Interest on fund January 1, 1899	27, 360. 00		
		54, 720. 00	
Interest to January 1, 1898, on West Shore bonds		1, 680, 00	
			$122, 203. 02
Cash from sales of publications		396. 18	
Cash from repayments, freight, etc		9, 227. 42	
			9, 623. 60
Total receipts			131, 826. 62

EXPENDITURES.

Building:			
Repairs, care, and improvements	$3, 717. 44		
Furniture and fixtures	688. 11		
		$4, 405. 55	
General expenses:			
Postage and telegraph	648. 50		
Stationery	1, 220. 66		
General printing	157. 80		
Incidentals (fuel, gas, etc.)	6, 807. 37		
Library (books, periodicals, etc.)	2, 632. 57		
Salaries[1]	21, 812. 12		
Gallery of art	191. 80		
Meetings	232. 03		
		33, 702. 85	
Publications and researches:			
Smithsonian contributions	1, 331. 20		
Miscellaneous collections	3, 772. 81		
Reports	853. 33		
Special publications	343. 60		
Researches	5, 962. 59		
Apparatus	200. 02		
Hodgkins fund	1, 910. 11		
Explorations	1, 300. 00		
		15, 673. 66	
Literary and scientific exchanges		3, 341. 14	
			57, 123. 20
Balance unexpended June 30, 1899			74, 703. 42

The cash received from the sale of publications, from repayments for freights, etc., is to be credited to the items of expenditure as follows:

Smithsonian contributions	$61. 68
Miscellaneous collections	323. 94
Reports	10. 56
	396. 18

[1] In addition to the above $21,812.12, paid for salaries under general expenses, $5,712.33 were paid for services, viz, $2,285.21 charged to building account, $925.11 to Hodgkins fund account, $1,079.28 to library account, and $1,422.73 to researches account.

Exchanges	$4,598.43
Incidentals	1,677.01
Explorations	400.00
Researches	2,550.92
Postage and telegraph	1.06
	9,623.60

The next expenditures of the Institution for the year ending June 30, 1899, were therefore $47,499.60, or $9,623.60 less than the gross expenditures, $57,123.20, as above stated.

All moneys received by the Smithsonian Institution from interests, sales, refunding of moneys temporarily advanced, or otherwise, are deposited with the Treasurer of the United States to the credit of the Secretary of the Institution, and all payments are made by his checks on the Treasurer of the United States.

Your committee also presents the following statements in regard to appropriations and expenditures for objects intrusted by Congress to the care of the Smithsonian Institution:

Detailed statement of disbursements from appropriations committed by Congress to the care of the Smithsonian Institution for the fiscal year ending June 30, 1899, and from balances of former years.

INTERNATIONAL EXCHANGES.

Receipts.

Appropriated by Congress for the fiscal year ending June 30, 1899, "for expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees" (sundry civil act, July 1, 1898) $21,000.00

Disbursements from July 1, 1898, to June 30, 1899.

Salaries or compensation:

1 curator, 12 months, at $225	$2,700.00
1 chief clerk, 12 months, at $175	2,100.00
1 clerk, 12 months, at $150	1,800.00
1 clerk, 12 months, at $116.67	1,400.04
1 clerk, 2 months, at $100	200.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $80	960.00
1 stenographer, 12 months, at $75	900.00
1 packer, 11½ months, at $55	632.50
1 workman, 12 months, at $50	600.00
1 copyist, 12 months, at $45	540.00
1 copyist, 10 months, at $45	450.00
1 messenger, 7 months, at $25	175.00
1 messenger, 5 months, at $25	125.00
1 laborer, 313 days, at $1.50	469.50
1 cleaner, 159 days, at $1	159.00
1 agent, 6 months, at $91.66⅔	550.00
1 agent, 6 months, at $50	300.00
1 agent, 6 months, at 13.58⅓	81.50
Total salaries or compensation	15,342.54

General expenses:			
Books	$29.55		
Freight	2,741.12		
Packing boxes	481.50		
Postage and telegraph	214.00		
Stationery and supplies	361.96		
		$3,828.13	
Total disbursements			$19,170.67
Balance July 1, 1899, to meet liabilities			1,829.33

INTERNATIONAL EXCHANGES, 1898.

Balance July 1, 1898, as per last report	$40.16

Disbursements.

Freight	34.05
Balance July 1, 1899	6.11

INTERNATIONAL EXCHANGES, 1897.

Balance July 1, 1898, as per last report	$1.08

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

AMERICAN ETHNOLOGY, 1899.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for continuing ethnological researches among the American Indians under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, and the purchase of necessary books and periodicals, $50,000, of which sum not exceeding $1,000 may be used for rent of building" (sundry civil act, July 1, 1898)	$50,000.00

The actual conduct of these investigations has been continued by the secretary in the hands of Maj. J. W. Powell, director of the Bureau of American Ethnology.

Disbursements.

Salaries or compensation:	
1 director, 12 months, at $375	$4,500.00
1 ethnologist in charge, 12 months, at $325	3,900.00
1 ethnologist, 12 months, at $200	2,400.00
1 ethnologist, 12 months, at $183.33	2,199.96
1 ethnologist, 12 months, at $166.67	2,000.04
1 ethnologist, 12 months, at $166.67	2,000.04
1 ethnologist, 9½ months and 5½ days, at $166.67	1,612.93
1 ethnologist, 12 months, at $125	1,500.00
1 ethnologist, 12 months, at $125	1,500.00
1 ethnologist, 12 months, at $125	1,500.00
1 ethnologic translator, 12 months, at $125	1,500.00
1 illustrator, 11 months, at $166.67	1,833.37
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 3 months, at $100	300.00
1 clerk, 12 months, at $75	900.00
1 clerk, 12 months, at $50	600.00
1 skilled laborer, 12 months, at $60	720.00

Salaries or compensation—Continued:			
1 skilled laborer, 12 months, at $45		$540.00	
1 laborer, 12 months, at $50		600.00	
1 messenger, 12 months, at $50		600.00	
Total salaries or compensation		34,306.34	
General expenses:			
Drawings and illustrations	$574.25		
Freight	377.35		
Postage, telegrams, etc	41.51		
Publications	453.33		
Office furniture	63.81		
Rental	916.63		
Special services	414.23		
Specimens	4,499.00		
Books	1,164.70		
Stationery, supplies, etc	1,692.92		
Traveling and field expenses	2,114.19		
Miscellaneous	271.74		
Translating	75.00		
		12,658.66	
Total disbursements			$46,965.00
Balance July 1, 1899, to meet liabilities			3,035.00

AMERICAN ETHNOLOGY, 1898.

Balance July 1, 1898, as per last report		$1,831.28

Disbursements.

General expenses:		
Freight	$120.48	
Miscellaneous	30.13	
Postage and telegraph	21.25	
Special services	7.00	
Specimens	1,587.00	
Traveling expenses	30.06	
Total		1,795.92
Balance July 1, 1899		35.36

NORTH AMERICAN ETHNOLOGY, 1897.

Balance July 1, 1898, as per last report	$5.58	
Appropriation by Congress "for payment of the outstanding accounts incurred during the fiscal year ended June 30, 1897, under the appropriation 'North American Ethnology, Smithsonian Institution,' and which are set forth on page 5 of House document No. 319, of this session, $466.50" (deficiency act, July 7, 1898)	466.50	
		$472.08

Disbursements.

General expenses:		
Freight and transportation	454.68	
Postage and telegraph	12.24	
		466.92
Balance		5.16

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

NATIONAL MUSEUM—PRESERVATION OF COLLECTIONS, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for continuing the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employees, $165,000, of which sum $5,500 may be used for necessary drawings and illustrations for publications of the National Museum" (sundry civil act, July 1, 1898)	$165,000.00

Expenditures.

Services	$148,368.54		
Special services	2,341.50		
Total services		$150,710.04	
Miscellaneous:			
Supplies	$2,766.01		
Stationery	770.17		
Specimens	3,781.44		
Travel	1,093.10		
Freight	1,217.30		
Total miscellaneous		9,628.02	
Total expenditures			160,338.06
Balance July, 1, 1899, to meet outstanding liabilities			4,661.94

Analysis of expenditures for salaries or compensation.

Scientific staff:	
1 executive curator, 12 months, at $291.66	$3,499.92
1 head curator, 12 months, at $291.66	3,499.92
1 head curator, 12 months, at $291.66	3,499.92
1 curator, 12 months, at $200	2,400.00
1 curator, 12 months, at $200	2,400.00
1 curator, 12 months, at $200	2,400.00
1 curator, 12 months, at $208.33	2,499.96
1 curator, 12 months, at $175	2,100.00
1 acting curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $130	1,560.00
1 assistant curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $125	1,500.00
1 assistant curator, 12 months, at $125	1,500.00
1 assistant curator, 12 months, at $116.66	1,399.92
1 assistant curator, 12 months, at $100	1,200.00
1 assistant curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $133.33	1,599.96
1 second assistant curator, 12 months, at $100	1,200.00
1 aid, 12 months, at $70	840.00

Scientific staff—Continued.		
1 aid, 12 months, at $60	$720.00	
1 aid, 12 months, at $75	900.00	
1 aid, 6 months, at $80	480.00	
1 aid, 10 months 17 days, at $50	527.42	
1 aid, 11 months, at $100	1,100.00	
1 aid, 12 months, at $100	1,200.00	
1 aid, 12 months, at $50	600.00	
1 aid, 12 months, at $100	1,200.00	
1 assistant, 12 months, at $75	900.00	
		$49,727.02
Preparators:		
1 photographer, 12 months, at $158.33	1,899.96	
1 modeler, 12 months, at $100	1,200.00	
1 osteologist, 12 months at $90	1,080.00	
1 preparator, 12 months, at $80	960.00	
1 preparator, 12 months, at $70	840.00	
1 preparator, 12 months, at $80	960.00	
1 preparator, 12 months, at $45	540.00	
1 preparator, 3 months, at $50, $150; 9 months, at $75, $675	825.00	
1 preparator, 12 months, at $80	960.00	
1 preparator, 12 months, at $45	540.00	
1 preparator, 12 months, at $60	720.00	
1 preparator, 12 months, at $75	900.00	
1 taxidermist, 12 months, at $60	720.00	
1 taxidermist, 12 months, at $90	1,080.00	
1 taxidermist, 10 months 27 days, at $100	1,088.17	
		14,313.13
Clerical staff:		
1 chief clerk, 5 months, at $208.34; 7 months, at $208.33	2,500.01	
1 acting chief clerk, 12 months, at $150	1,800.00	
1 editor, 12 months, at $167	2,004.00	
1 chief of division, 12 months, at $200	2,400.00	
1 registrar, 12 months, at $167	2,004.00	
1 disbursing clerk, 12 months, at $116.67	1,400.04	
1 assistant librarian, 12 months, at $117	1,404.00	
1 stenographer and typewriter, 5 months 11 days, at $50	267.74	
1 stenographer, 12 months, at $50	600.00	
1 stenographer and typewriter, 12 months, at $50	600.00	
1 stenographer and typewriter, 12 months, at $115	1,380.00	
1 typewriter, 5 months 31 days, at $55	331.42	
1 typewriter, 30 days, at $50	48.61	
1 typewriter, 3 months, at $50, $150; 9 months, at $65, $585	735.00	
1 typewriter, 12 months, at $50	600.00	
1 clerk, 12 months, at $83.34	1,000.08	
1 clerk, 12 months, at $85	1,020.00	
1 clerk, 12 months, at $90	1,080.00	
1 clerk, 12 months, at $50	600.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $60	720.00	
1 clerk, 10 months 7 days, at $50	511.29	

Clerical staff—Continued.

1 clerk, 12 months, at $115	$1,380.00	
1 clerk, 3 months, at $70, $210; 9 months, at $75, $675	885.00	
1 finance clerk, 12 months, at $110	1,320.00	
1 acting property clerk, 12 months, at $50	600.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $50	600.00	
1 clerk and preparator, 12 months, at $50	600.00	
1 clerk, 12 months, at $90	1,080.00	
1 clerk, 12 months, at $50	600.00	
1 clerk, 11 months 15 days, at $50	575.00	
1 clerk, 12 months, at $70	840.00	
1 clerk and preparator, 12 months, at $75	900.00	
1 clerk, 12 months, at $115	1,380.00	
1 clerk, 6 months, at $100	600.00	
1 document clerk, 12 months, at $50	600.00	
1 clerk, 12 months, at $90	1,080.00	
1 clerk, 12 months, at $60	720.00	
1 clerk and preparator, 4 months 28 days, at $45	222.00	
1 copyist, 12 months, at $35	420.00	
1 copyist, 10 months 25 days, at $40	432.26	
1 copyist, 3 months, at $50, $150; 3 days, at $50, $4.84	154.84	
1 copyist, 12 months, at $55	660.00	
1 copyist, 7 months, at $45, $315; 3 months, at $50, $150; 1 month, at $38.71; 1 month, at $30	533.71	
1 copyist, 12 months, at $50	600.00	
1 copyist, 12 months, at $40	480.00	
1 copyist, 12 months, at $35	420.00	
1 copyist, 12 months, at $35	420.00	
1 copyist, 12 months, at $35	420.00	
1 copyist, 12 months, at $50	600.00	
1 copyist, 12 months, at $30	360.00	
1 copyist, 11 months 16 days, at $40	460.65	
		$44,929.65

Buildings and labor:

1 general foreman, 10 months 25 days, at $115	1,242.74
1 foreman, 12 months, at $50	600.00
1 chief of watch, 12 months, at $65	780.00
1 chief of watch, 11 months 29 days, at $65	775.81
1 chief of watch, 12 months, at $65	780.00
1 skilled laborer, 12 months, at $40	480.00
1 skilled laborer, 12 months, at $50	600.00
1 skilled laborer, 6 months, at $55, $330; 1 month, at $56; 1 month, at $59	445.00
1 skilled laborer, 12 months, at $50	600.00
1 workman, 32 ½ days, at $1.50	489.75
1 workman, 358 days, at $1.50	537.00
1 workman, 313½ days, at $1.50	470.25
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $45	540.00
1 watchman, 11 months 25 days, at $45	532.50
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00

Buildings and labor—Continued.

1 watchman, 12 months, at $65	$780.00
1 watchman, 12 months, at $45	540.00
1 watchman, 1 month 15 days, at $45	66.77
1 watchman, 11 months 30 days, at $45	538.55
1 watchman, 12 months, at $60	720.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 11 months 28 days, at $50	595.16
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $45	540.00
1 watchman, 12 months, at $45	540.00
1 watchman, 12 months, at $45	540.00
1 watchman, 8 months 48 days, at $50	477.42
1 watchman, 7 months 100 days, at $50	514.74
1 watchman, 12 months, at $45	540.00
1 watchman, 12 months, at $30	360.00
1 watchman, 10 months 56 days, at $45	531.30
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $45	540.00
1 laborer, 124 days, at $1.50	186.00
1 laborer, 36 hours, at 20 cents	7.20
1 laborer, 2 months 31 days, at $20	60.35
1 laborer, 313 days, at $1.50	469.50
1 laborer, 27 hours, at 20 cents	5.40
1 laborer, 23 days, at $1.50	34.50
1 laborer, 5 months, at $30, $150; 2 months, at $31.50, $63; 1 month, at $39; 1 month, at $40.50; 2 months, at $33, $66; 1 month, at $36	394.50
1 laborer, 12 months, at $40	480.00
1 laborer, 12 months, at $45	540.00
1 laborer, 313 days, at $1.50	469.50
1 laborer, 313½ days, at $1.50	470.25
1 laborer, 313 days, at $1	313.00
1 laborer, 45 hours, at 20 cents	9.00
1 laborer, 304 days, at $1.50	456.00
1 laborer, 261 days, at $1.50	391.50
1 laborer, 256 days, at $1.50	384.00
1 laborer, 319¾ days, at $1.50	479.63
1 laborer, 78 days, at $1.50	117.00
1 laborer, 313 days, at $1.50	469.50
1 laborer, 340½ days, at $1.50	510.75
1 laborer, 314 days, at $1.50	471.00
1 laborer, 313 days, at $1	313.00
1 laborer, 153 days, at $1.50	229.50
1 laborer, 47 hours, at 20 cents	9.40
1 laborer, 22 days, at $1	22.00
1 laborer, 291½ days, at $1.50	437.25
1 laborer, 313 days, at $1	313.00
1 laborer, 1 month, at $40	40.00
1 laborer, 26 days, at $1.50	39.00
1 laborer, 313 days, at $1.50	469.50
1 laborer, 311 days, at $1	311.00
1 laborer, 32 hours, at 20 cents	6.40

Buildings and labor—Continued.

1 laborer, 313 days, at $1.50	$469.50	
1 laborer, 12 months, at $40	480.00	
1 laborer, 301½ days, at $1.50	452.25	
1 laborer, 313 days, at $1.50	469.50	
1 laborer, 313 days, at $1.50	469.50	
1 laborer, 324 days, at $1.50	486.00	
1 laborer, 320½ days, at $1.50	480.75	
1 laborer, 105 days, at $1.50	157.50	
1 laborer, 15 days, at $1.50	22.50	
1 laborer, 287 days, at $1.50	430.50	
1 messenger, 12 months, at $40	480.00	
1 messenger, 12 months, at $25	300.00	
1 messenger, 12 months, at $25	300.00	
1 messenger, 12 months, at $25	300.00	
1 messenger, 6 months, at $25, $150; 3 days, at $25, $3.23	153.23	
1 messenger, 12 months, at $50	600.00	
1 attendant, 14½ days, at $1	14.50	
1 attendant, 12 months, at $40	480.00	
1 cleaner, 2 months, 10 days, at $30	69.68	
1 cleaner, 11 months, at $30, $330; 1 month, at $31.50	361.50	
1 cleaner, 1 month 45 days, at $30	87.39	
1 cleaner, 8 months, at $30, $240; 1 month, at $34.50; 17 days, at $30, $17	291.50	
1 cleaner, 12 days, at $1	12.00	
1 cleaner, 360 days, at $1	360.00	
1 cleaner, 10 months 5½ days, at $30	305.32	
1 cleaner, 12 months, at $30	360.00	
		$39,398.74
Total services		148,368.54

PRESERVATION OF COLLECTIONS, 1898.

Balance as per report July 1, 1898			$2,363.51

Expenditures to June 30, 1899.

Services	$25.50		
Special services	298.59		
Total services		$324.09	
Miscellaneous:			
Supplies	256.58		
Stationery	24.52		
Specimens	753.21		
Books	425.91		
Travel	36.06		
Freight	445.86		
Total miscellaneous		1,942.14	
Total expenditures			2,266.23
Balance July 1, 1899			97.28

TOTAL STATEMENT OF RECEIPTS AND EXPENDITURES FOR PRESERVATION, 1898.

Receipts.

Appropriation June 4, 1897			$160,000.00

Expenditures.

Services	$139,614.18		
Special services	4,876.31		
Total services		$144,490.49	
Miscellaneous:			
Supplies	4,164.93		
Stationery	878.75		
Specimens	4,620.63		
Books	1,259.71		
Travel	2,457.52		
Freight	2,030.67		
Total miscellaneous		15,412.23	
Total			159,902.72
Balance July 1, 1899			97.28

PRESERVATION OF COLLECTIONS, 1897.

Balance as per report July 1, 1898		$379.99

Expenditures to June 30, 1899.

Special services	$90.00	
Freight	2.98	
Specimens	25.10	
Books	257.95	
		376.03
Balance		3.96

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

TOTAL STATEMENT OF RECEIPTS AND EXPENDITURES FOR PRESERVATION OF COLLECTIONS, 1897.

Receipts.

Appropriation June 11, 1896			$153,225.00

Expenditures.

Salaries	$134,364.19		
Special services	5,761.23		
Total services		$140,125.42	
Miscellaneous:			
Supplies	3,343.64		
Stationery	1,373.17		
Specimens	4,127.54		
Books	1,940.65		

Miscellaneous—Continued.

Travel	$785.77		
Freight	1,524.85		
Total miscellaneous		$13,095.62	
Total			$153,221.04
Balance			3.96

FURNITURE AND FIXTURES, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the National Museum, including $20,000 for furnishing new galleries, and including salaries or compensation of all necessary employees" (sundry civil act, July 1, 1898).... $35,000.00

Expenditures.

	Regular.	Galleries.	Total.
Salaries or compensation	$7,299.37	$5,138.11	$12,437.48
Special service	52.40	4.80	57.20
Total services	7,351.77	5,142.91	12,494.68
Miscellaneous:			
Exhibition cases	2,211.98	8,983.70	11,195.68
Storage cases	175.20		175.20
Drawers, trays, etc	582.94	217.44	800.38
Frames and woodwork	634.62	197.82	832.44
Glass	504.21	3,295.12	3,799.33
Hardware	739.42	822.39	1,561.81
Tools	63.89		63.89
Cloth	120.83	20.20	141.03
Glass jars	213.61		213.61
Lumber	700.13	674.32	1,374.45
Paints, oils, etc	576.81		576.81
Office furniture	278.60	6.10	284.70
Leather and rubber	71.62		71.62
Iron brackets	47.41	34.65	82.06
Apparatus	300.53		300.53
Drawings for cases	36.50		36.50
Total miscellaneous	7,258.30	14,251.74	21,510.04
Total	14,610.07	19,394.65	34,004.72
Balance July 1, 1899, to meet liabilities			995.28

ANALYSIS OF EXPENDITURES FOR SALARIES.

General account.

1 superintendent of construction, 12 months, at $115	$1,380.00
1 cabinetmaker, 131 days, at $2.25	294.75
1 carpenter, 26 days, at $3	78.00
1 carpenter, 48 days, at $3	144.00
1 carpenter, 53 days, at $3	159.00
1 carpenter, 200 days, at $3	600.00
1 carpenter, 4½ days, at $3	13.50
1 carpenter, 138½ days, at $3	415.50
1 carpenter, 57 days, at $3	171.00
1 carpenter, 83 days, at $3	249.00
1 carpenter, 104 days, at $3	312.00

1 carpenter, 61 days, at $3	$183.00
1 carpenter, 34 days, at $3	102.00
1 carpenter, 6 days, at $3	18.00
1 carpenter, 13 days, at $3	39.00
1 carpenter, 261 days, at $3	783.00
1 carpenter, 26 days, at $3	78.00
1 carpenter, 79 days, at $3	237.00
1 painter, 3 months 37 days, at $65	273.28
1 skilled laborer, 284¾ days, at $2	569.50
1 skilled laborer, 5 months, at $60	300.00
1 skilled laborer, 110 days, at $2	220.00
1 skilled laborer, 5 days, at $2	10.00
1 skilled laborer, 111 days, at $2	222.00
1 skilled laborer, 26 days, at $2	52.00
1 workman, 214 days, at $1.50	321.00
1 laborer, 1 month 27 days, at $40	74.84
Total	7,299.37

ANALYSIS OF EXPENDITURES FOR SALARIES FOR FURNISHING GALLERIES, 1899.

1 cabinetmaker, 102 days, at $2.25	$229.50
1 carpenter, 53 days, at $3	159.00
1 carpenter, 13 days, at $3	39.00
1 carpenter, 26 days, at $3	78.00
1 carpenter, 52 days, at $3	156.00
1 carpenter, 46½ days, at $3	139.50
1 carpenter, 37 days, at $3	111.00
1 carpenter, 123 days, at $3	369.00
1 carpenter, 114½ days, at $3	343.50
1 carpenter, 180 days, at $3	540.00
1 carpenter, 182 days, at $3	546.00
1 carpenter, 44 days, at $3	132.00
1 carpenter, 37 days, at $3	111.00
1 carpenter, 89 days, at $3	267.00
1 carpenter, 52 days, at $3	156.00
1 carpenter, 26 days, at $3	78.00
1 carpenter, 79 days, at $3	237.00
1 painter, 7 months, at $65	455.00
1 skilled laborer, 79 days, at $2	158.00
1 skilled laborer, 2 months, at $60, $120; 1 month, at $64; 29 days, at $60, $57.61	241.61
1 skilled laborer, 26 days, at $2	52.00
1 skilled laborer, 37 days, at $2	74.00
1 skilled laborer, 182 days, at $2	364.00
1 skilled laborer, 51 days, at $2	102.00
Total	5,138.11

FURNITURE AND FIXTURES, 1898.

Balance, as per report July 1, 1898	$1,710.46

Appropriation June 4, 1897, $30,000, of which $15,000 could be expended for furnishing new galleries.

Expenditures July 1, 1898, to June 30, 1899.

	General.	Galleries.	Total.
Special services	$7.45	....	$7.45
Cotton, cloth, etc	22.50	$1.25	23.75
Cases	....	1,050.00	1,050.00
Frames	23.40	67.76	91.16
Glass jars	5.00	....	5.00
Rubber	10.58	3.60	14.18
Tools	5.86	....	5.86
Paints and oil	68.80	....	68.80
Hardware	40.72	104.25	144.97
Lumber	118.61	....	118.61
Office furniture	111.05	68.40	179.45
Total	413.97	1,295.26	1,709.23
Balance, July 1, 1899	....	....	1.23

TOTAL STATEMENT OF RECEIPTS AND EXPENDITURES FOR FURNITURE AND FIXTURES, 1898.

Receipts.

Appropriation June 4, 1897 (including $15,000, furnishing galleries) $30,000

Expenditures.

	Regular.	Galleries.	Total.
Salaries	$8,855.71	$4,800.75	....
Special services	400.06	9.50	....
Total services	9,255.77	4,810.25	$14,066.02
Miscellaneous:			
Exhibition cases	115.00	6,557.90	....
Storage cases	162.50	415.00	....
Drawers, trays, etc	521.85	207.36	....
Frames and woodwork	136.80	455.38	....
Glass	625.66	859.34	....
Hardware	751.06	547.15	....
Tools	73.22	....	....
Cloth	96.99	16.35	....
Glass jars	446.34	....	....
Lumber	1,125.52	520.04	....
Paints, oils, etc	615.58	....	....
Office furniture	692.40	76.40	....
Plumbing	12.92	....	....
Leather and rubber	39.92	3.60	....
Iron brackets	195.69	36.00	....
Travel	34.30	....	....
Apparatus	21.96	200.00	....
Brick, plaster, tiles	75.53	2.00	....
Drawings for cases	....	293.00	....
Total miscellaneous	5,743.23	10,189.52	....
Total	14,999.90	14,999.77	29,998.77
Balance July 1, 1899	....	....	1.23

FURNITURE AND FIXTURES, 1897.

Balance July 1, 1898, as per last annual report $8.30

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund June 30, 1899.

HEATING AND LIGHTING, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for expense of heating, lighting, electrical, telegraphic, and telephonic service for the National Museum, including salaries or compensation of necessary employees" (sundry civil act, July 1, 1898)................ $14,000.00

Expenditures.

Services	$6,914.80		
Special services	97.00		
Total services		$7,011.80	
Miscellaneous:			
Coal and wood	3,319.03		
Gas	1,149.60		
Rental of call boxes	90.00		
Electrical supplies	320.43		
Heating supplies	300.40		
Telegrams	16.17		
Travel	12.55		
Total miscellaneous		5,208.18	
Total expenditures			$12,219.98
Balance July 1, 1899, to meet liabilities			1,780.02

Analysis of expenditures for salaries, heating, and lighting, 1899.

1 engineer, 12 months, at $115	$1,380.00
1 telephone operator, 12 months, at $50	600.00
1 skilled laboror, 12 months, at $75	900.00
1 skilled laborer, 12 months, at $60	720.00
1 skilled laborer, 12 months, at $60	720.00
1 workman, 100 days, at $1.50	159.00
1 fireman, 12 months, at $50	600.00
1 fireman, 12 months, at $50	600.00
1 fireman, 10 months, at $50	500.00
1 fireman, 6 days, at $1.75	10.50
1 laborer, 143 days, at $1.50	214.50
1 laborer, 316 days, at $1.50	474.00
1 laborer, 7 days, at $1.50, $10.50; 59½ hours, at 20 cents, $11.90	22.40
1 laborer, 26 hours, at 20 cents	5.20
1 laborer, 46 hours, at 20 cents	9.20
Total salaries	6,914.80

HEATING AND LIGHTING, 1898.

Receipts.

Balance as per report July 1, 1898	$816.87

Expenditures, July 1, 1898, to June 30, 1899.

Special services	$102.00	
Coal and wood	4.53	
Gas	87.60	
Telephones	221.33	
Rental of call boxes	20.00	
Electrical supplies	188.04	
Telegrams	6.41	
Heating supplies	181.47	
Expenditures to June 30, 1899		811.38
Balance July 1, 1899		5.49

TOTAL STATEMENT, HEATING AND LIGHTING, 1898.

Receipts.

Appropriation June 4, 1897			$14,000.00

Expenditures.

Salaries	$6,542.04		
Special services	267.17		
Total services		$6,809.21	
Miscellaneous:			
Coal and wood	3,322.98		
Gas	1,267.70		
Telephones	832.83		
Rental of call boxes	120.00		
Electrical supplies	601.27		
Telegrams	25.92		
Heating supplies	1,014.60		
Total miscellaneous		7,185.30	
Total expenditures			13,994.51
Balance July 1, 1899			5.49

HEATING AND LIGHTING, 1897.

Receipts.

Balance as per report July 1, 1898	$2.84

Expenditures, July 1, 1898, to June 30, 1899.

Telephones	2.66
Balance	.18

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

TOTAL STATEMENT OF APPROPRIATION FOR HEATING AND LIGHTING, 1897.

Receipts.

Appropriation by Congress, June 11, 1896			$13,000.00

Expenditures.

Salaries or compensation	$6,269.05		
Special services	21.75		
Total services		$6,290.80	
General expenses:			
Coal and wood	3,676.82		
Gas	1,037.20		
Telephones	692.07		
Electrical supplies	505.98		
Rental of call boxes	120.00		
Heating supplies	665.81		
Telegrams	11.14		
Total general expenses		6,709.02	
Total expenditures			12,999.82
Balance			.18

POSTAGE, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for postage stamps and foreign postal cards for the National Museum" (sundry civil act, July 1, 1898)	$500.00

Expenditures.

For postage stamps and cards	500.00

NATIONAL MUSEUM—PRINTING AND BINDING, 1899.

Receipts.

Appropriation by Congress for the year ending June 30, 1899, for the Smithsonian Institution for printing labels and blanks for the "Bulletins" and annual volumes of the "Proceedings" of the National Museum, the editions of which shall not be less than 3,000 copies, and binding in half turkey, or material not more expensive, scientific books and pamphlets presented to and acquired by the National Museum library	$17,000.00

Expenditures.

Bulletins of the Museum	$6,926.53	
Proceedings of the Museum	8,366.93	
Labels	561.20	
Blanks	199.75	
Envelopes, pads, and circulars	78.23	
Cards	147.17	
Binding	701.82	
Congressional Records	16.00	
Total		16,997.63
Balance July 1, 1899		2.37

NATIONAL MUSEUM—RENT OF WORKSHOPS, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, for rent of workshops and temporary storage quarters for the National Museum (sundry civil act, July 1, 1898)	$4,500.00

Expenditures.

For rent of workshops:		
431 Ninth street SW	$1,999.92	
217 Seventh street SW	1,290.00	
915 Virginia avenue (rear)	550.00	
313 Tenth street SW	550.00	
Total		4,389.92
Balance July 1, 1899		110.08

NATIONAL MUSEUM—RENT OF WORKSHOPS, 1898.

Balance July 1, 1898, as per last annual report	$0.08
Balance July 1, 1899	.08

NATIONAL MUSEUM—RENT OF WORKSHOPS, 1897.

Balance July 1, 1898 .... $0.08

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

NATIONAL MUSEUM—BUILDING REPAIRS, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for repairs to buildings, shops, and sheds, National Museum, including all necessary labor and material" $4,000.00

Expenditures.

Salaries or compensation:			
1 acting foreman, 30 days, at $2.40	$72.00		
1 carpenter, 53 days, at $3	159.00		
1 carpenter, 27 days, at $3	81.00		
1 carpenter, 53 days, at $3	159.00		
1 carpenter, 26 days, at $3	78.00		
1 skilled laborer, 12½ days, at $2	25.00		
1 laborer, 8 days, at $1.50	12.00		
1 laborer, 12 days, at $1.50	18.00		
1 laborer, 19 days, at $1.50	28.50		
1 laborer, 15 days, at $1.50	22.50		
1 laborer, 19 days, at $1.50	28.50		
1 laborer, 27 days, at $1.50	40.50		
1 laborer, 27 days, at $1.50	40.50		
1 laborer, 27 days, at $1.50	40.50		
1 laborer, 19 days, at $1.50	28.50		
1 laborer, 27 days, at $1.50	40.50		
1 laborer, 40¾ days, at $1.50	61.13		
1 laborer, 4 days, at $1.50	6.00		
1 laborer, 21¼ days, at $1.50	31.88		
		$973.01	
Miscellaneous:			
Terrazzo and marble floors	1,420.56		
Lumber	539.16		
Woodwork	264.00		
Cement	188.45		
Hardware and tools	164.40		
Mortar, sand, gravel, lime, slate	134.61		
Paints and oils	89.89		
Iron grills	29.60		
Brick	28.35		
Tiles	31.80		
Removing dirt	25.00		
Brushes	10.50		
Blue prints	2.59		
Glass	12.80		
Travel	4.20		
		2,945.91	
Total			3,918.92
Balance July 1, 1899			81.08

BUILDING REPAIRS, 1898.

Balance as per report July 1, 1898		$31.98

Expenditures to June 30, 1899.

Cement, brick, lime, sand, plaster	$14.95	
Hardware	12.50	
Expenditures to June 30, 1899		27.45
Balance July 1, 1899		4.53

TOTAL STATEMENT OF BUILDING REPAIRS, 1898.

Receipts.

Appropriation June 4, 1897			$4,000.00

Expenditures July 1, 1897, to June 30, 1899.

Salaries or compensation	$2,124.74		
Special services	232.80		
Total services		$2,357.54	
Miscellaneous:			
Granolithic pavement	803.15		
Arches and terrazzo pavement	265.60		
Iron columns	260.86		
Hardware	87.20		
Glass	6.25		
Lumber	13.50		
Brick, sand, marble, cement, etc	191.37		
Frames, etc	10.00		
Total miscellaneous		1,637.93	
Total			3,995.47
Balance July 1, 1898			4.53

NATIONAL MUSEUM—BUILDING REPAIRS, 1897.

Balance July 1, 1898, as per last annual report	$0.58

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

NATIONAL MUSEUM—GALLERIES, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, "for the continuation of the construction of galleries in the National Museum building, under the direction of the superintendent of the Congressional Library building and grounds, in accordance with the approval of the Secretary of the Smithsonian Institution, and for the building of skylights above galleries in the four courts, and the erection of a ventilator upon the roof of the lecture hall" (sundry civil act, July 1, 1898)	$10,000.00

Expenditures.

Salaries or compensation:	
1 acting foreman, 52 days, at $2.40, $124.80; 1 month 6 days, at $70, $85	$209.80
1 carpenter, 26 days, at $3	78.00

Salaries or compensation—Continued:			
1 carpenter, 7½ days, at $3	$22.50		
1 carpenter, 3½ days, at $3	10.50		
1 skilled laborer, 185 hours, at 25 cents	46.38		
1 skilled laborer, 58 days, at $2	116.00		
1 laborer, 52 days, at $1.50	78.00		
1 laborer, 29 days, at $1.50	43.50		
1 laborer, 26 days, at $1.50	39.00		
1 laborer, 59 days, at $1.50	88.50		
1 laborer, 24 days, at $1.50	36.00		
1 laborer, 14½ days, at $1.50	21.75		
1 laborer, 15¾ days, at $1.50	23.63		
1 laborer, 33 days, at $2	66.00		
1 laborer, 58 days, at $1	58.00		
1 laborer, 2 days, at $1.50	3.00		
Total salaries		$940.56	
Miscellaneous:			
Ransome arches	1,609.38		
Ironwork	1,338.35		
Terrazo floor and marble	1,295.09		
Lumber	103.34		
Blue prints and drawings	81.00		
Advertising	51.20		
Hardware and tools	40.99		
Sand, slate, cement, mortar	95.60		
Brick	38.50		
Canvas	29.21		
Paint	25.65		
Travel	23.10		
Sheeting	21.12		
Paper	5.25		
		4,757.78	
Total expenditures			$5,698.34
Balance July 1, 1899			4,301.66

GALLERIES, 1898.

Balance, as per report July 1, 1898		$551.87

Expenditures to June 30, 1899.

Iron columns and beams, erection	$225.00	
Rubber	150.00	
Four iron stairways	168.00	
Total expenditures to June 30, 1899		543.00
Balance July 1, 1899		8.87

NATIONAL MUSEUM—GALLERIES, 1898—TOTAL STATEMENT.

Receipts.

Appropriation by Congress, June 4, 1897	$8,000.00

Expenditures July 1, 1897, to June 30, 1898.

Salaries	$280.50		
Special services	471.03		
Total services		$751.53	
Miscellaneous:			
Drawings and blue prints	58.65		
Arches and pavements	4,979.50		
Iron columns and steel beams, erection	300.00		
Iron columns and steel beams	1,410.00		
Iron stairways	168.00		
Lumber	7.10		
Advertising	21.75		
Hardware	6.60		
Brick, lime, sand, etc	138.00		
Rubber	150.00		
Total miscellaneous		7,239.60	
Total expenditures			$7,991.13
Balance July 1, 1899			8.87

GALLERIES, NATIONAL MUSEUM, 1897.

Balance as per report July 1, 1898	$0.05

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department, to the credit of the surplus fund, June 30, 1899.

REBUILDING SHEDS, 1898.

Balance as per report July 1, 1898	$28.90

Expenditures to June 30, 1899.

Lumber	28.12
Balance July 1, 1899	.78

LIBRARY OF G. BROWN GOODE.

Receipts.

Appropriation by Congress for purchase of 2,900 volumes, 18,000 pamphlets, 1,800 portraits, autographs, and engravings relating to museums, exhibitions, and natural history, the library of the late G. Brown Goode (sundry civil act, July 1, 1898)	$5,000.00

Expenditure.

Sarah F. J. Goode, executrix	5,000.00

NATIONAL MUSEUM—BOOKS, 1899.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1899, for purchase of books, pamphlets, and periodicals for reference in the National Museum (sundry civil act, July 1, 1898)	$2,000.00

Expenditures.

Paid for purchase of books, pamphlets, and periodicals July 1, 1898, to June 30, 1899	1,300.43
Balance July 1, 1899	699.57

ASTROPHYSICAL OBSERVATORY, SMITHSONIAN INSTITUTION, 1899.

Receipts.

Appropriation by Congress for maintenance of Astrophysical Observatory, under the direction of the Smithsonian Institution, including salaries of assistants, the purchase of necessary books and periodicals, apparatus, printing and publishing results of researches, not exceeding one thousand five hundred copies, and miscellaneous expenses, ten thousand dollars (sundry civil act, July 1, 1898) $10,000.00

Disbursements from July 1, 1898, to June 30, 1899.

Salaries or compensation:		
1 aid, 12 months, at $166.67	$2,000.04	
1 junior assistant, 12 months, at $110	1,320.00	
1 junior assistant, 2 months, at $100	200.00	
1 instrument maker, 6½ months and 22½ days, at $80	577.85	
1 stenographer, 12 months, at $75	900.00	
1 fireman, 12 months, at $45	540.00	
1 clerk, 1 month, at $100	100.00	
1 carpenter, 14½ days, at $3	43.50	
1 carpenter, 37⅜ days, at $3	112.12	
1 carpenter, 6½ days, at $3	19.50	
1 carpenter, 53 days, at $3	159.00	
1 steam fitter, 6½ days, at $3	19.50	
1 steam fitter, 1 day, at $3	3.00	
1 bricklayer, 3 days, at $3	9.00	
1 skilled laborer, 12¼ days, at $2, and tinner, 1 day, at $3	27.50	
1 painter, 12½ days, at $2	25.00	
1 painter, 2 days, at $2.50	5.00	
1 laborer, 1½ days, at $1.50	2.25	
1 laborer, 12½ days, at $1.50	18.75	
1 laborer, 4 days, at $1.50	6.00	
1 laborer, 9½ days, at $1.50	14.25	
1 laborer, ½ day, at $1.50	.75	
1 laborer, 8 days, at $1.50	12.00	
1 laborer, 1 day, at $1.50	1.50	
1 laborer, ½ day, at $1.50	.75	
1 laborer, 3 days, at $1.50	4.50	
1 laborer, ¾ day, at $1.50	1.12	
1 laborer, ½ day, at $1.25	.62	
1 cleaner, 115 days, at $1	115.00	
Total salaries or compensation		$6,238.50
General expenses:		
Apparatus	2,134.84	
Books	161.73	
Illustrations	35.87	
Freight	35.10	
Fuel	116.87	
Furniture	74.45	
Lumber	112.58	
Postage and telegrams	2.92	

General expenses—Continued.

Stationery	$2.25		
Supplies, etc	332.53		
		$3,009.14	
Total disbursements			$9,247.64
Balance July 1, 1899, to meet liabilities			752.36

ASTROPHYSICAL OBSERVATORY, 1898.

Balance July 1, 1898, as per last report	$2,701.78

Appropriation by Congress "that the Secretary of the Smithsonian Institution is hereby authorized to apply any unexpended balance of the appropriation for the Astrophysical Observatory, Smithsonian Institution, for the fiscal year ending June thirtieth, eighteen hundred and ninety-eight, to the improvement of the building used for the purposes of the said observatory, and the same is hereby reappropriated and made available for expenditure during the fiscal year eighteen hundred and ninety-nine for the object set forth." (Deficiency act July 7, 1898.)

Disbursements July 1, 1898, to June 30, 1899.

General expenses:

Apparatus	$390.43	
Books	53.83	
Freight	5.20	
Lumber	1.62	
Improvement of building	2,235.00	
Supplies, etc	15.53	
		2,701.61
Balance July 1, 1899		.17

APPROPRIATION, ASTROPHYSICAL OBSERVATORY, SMITHSONIAN INSTITUTION, 1897.

Balance July 1, 1898, as per last report	$23.42
Apparatus	23.41
Balance	.01

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

NATIONAL ZOOLOGICAL PARK, 1899.

Receipts.

Appropriation by Congress "for continuing the construction of roads, walks, bridges, water supply, sewerage, and drainage; and for grading, planting, and otherwise improving the grounds; erecting and repairing buildings and inclosures; care, subsistence, purchase, and transportation of animals, including salaries or compensation of all necessary employees, the purchase of necessary books and periodicals, and general incidental expenses not otherwise provided for, sixty-five thousand dollars; one half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States; and of the sum hereby appropriated five thousand dollars shall be used for continuing the entrance into the Zoological Park from Woodley lane and opening driveway into Zoological Park from said entrance along the bank of Rock Creek" (sundry civil act, July 1, 1898) $65,000.00

Disbursements July 1, 1898, to June 30, 1899.

Salaries or compensation:		
1 superintendent, 12 months, at $225	$2,700.00	
1 property clerk, 12 months, at $125	1,500.00	
1 clerk, 12 months, at $75	900.00	
1 stenographer, 12 months, at $62.50	750.00	
1 copyist {1½ months, at $50	75.00	
1 copyist {8½ months, at $75	637.50	
1 copyist, 2½ months, at $50	125.00	
1 copyist, 8 days, at $50	12.90	
1 head keeper, 12 months, at $100	1,200.00	
1 keeper, 12 months, at $60	720.00	
1 keeper, 12 months, at $60	720.00	
1 keeper, 12 months, at $60	720.00	
1 keeper, 12 months, at $60	720.00	
1 {keeper, 10 months, at $60	600.00	
1 {laborer, 2 months, at $50	100.00	
1 keeper of aquarium, 9 months, at $75	675.00	
1 landscape gardener, 11 months 28½ days, at $75	893.95	
1 assistant foreman, 12 months, at $60	720.00	
1 watchman, 9 months 26 days, at $60	592.00	
1 watchman, 12 months, at $50	600.00	
1 watchman, 12 months, at $50	600.00	
1 blacksmith, 12 months, at $75	900.00	
1 assistant blacksmith, 12 months, at $60	720.00	
1 workman, 12 months, at $60	720.00	
1 workman, 12 months, at $50	600.00	
1 laborer, 11 months 14 days, at $50	572.50	
1 laborer, 12 months, at $50	600.00	
1 laborer, 12 months, at $50	600.00	
1 laborer, 12 months, at $45	540.00	
1 laborer, 11 months 21½ days, at $20	233.87	
1 modeler, 34 days, at $100	111.83	
Total salaries or compensation		$21,159.55
Miscellaneous:		
Buildings	770.95	
Building material	777.78	
Fencing, cage material, etc	621.70	
Food	6,367.93	
Freight	355.96	
Fuel	476.68	
Lumber	706.00	
Machinery, tools, etc	738.05	
Miscellaneous	742.52	
Paints, oils, glass, etc	66.32	
Postage and telegraph, etc	28.30	
Purchase of animals	2,270.02	
Road material, grading, etc	7,674.30	
Stationery, books, etc.	696.45	
Traveling expenses	373.40	

Miscellaneous—Continued.		
Trees, plants, etc	$571.89	
Water supply, sewerage, etc	732.13	
Total miscellaneous		$23,970.38
Wages of mechanics and laborers and hire of teams in constructing buildings and inclosures, laying water pipes, building roads, gutters, and walks, planting trees, and otherwise improving the grounds:		
1 laborer, 88 days, at $2.50	$220.00	
1 laborer, 210 days, at $2.50	525.00	
1 blacksmith, 32 days, at $2.50	80.00	
1 laborer, 1 day, at $2	2.00	
1 laborer, 5 days, at $2	10.00	
1 laborer, 12 days, at $2	24.00	
1 laborer, 365 days, at $2	730.00	
1 laborer, 277 days, at $2	554.00	
1 laborer, 5 days, at $2	10.00	
1 laborer, 2¼ days, at $1.75	3.94	
1 laborer, 331½ days, at $1.50	497.25	
1 laborer, 101¾ days, at $1.50	152.63	
1 laborer, 117¾ days, at $1.50	176.62	
1 laborer, 29 days, at $1.50	43.50	
1 laborer, 138¼ days, at $1.50	207.38	
1 laborer, 365 days, at $1.50	547.50	
1 laborer, 330½ days, at $1.50	495.75	
1 laborer, 51½ days, at $1.50	77.25	
1 laborer, 125½ days, at $1.50	188.26	
1 laborer, 123½ days, at $1.50	185.25	
1 laborer, 46 days, at $1.50	69.00	
1 laborer, 80½ days, at $1.50	120.74	
1 laborer, 78½ days, at $1.50	117.75	
1 laborer, 328 days, at $1.50	491.95	
1 laborer, 342½ days, at $1.50	513.75	
1 laborer, 207 days, at $1.50	310.51	
1 laborer, 365½ days, at $1.50	548.25	
1 laborer, 221 days, at $1.50	331.50	
1 laborer, 7¼ days, at $1.50	10.87	
1 laborer, 84¼ days, at $1.50	126.37	
1 laborer, 62 days, at $1.50	93.00	
1 laborer, 236¼ days, at $1.50	354.38	
1 laborer, 171½ days, at $1.50	257.25	
1 laborer, 39½ days, at $1.50	59.25	
1 laborer, 193½ days, at $1.50	290.27	
1 laborer, 35½ days, at $1.50	53.25	
1 laborer, 234½ days, at $1.50	351.76	
1 laborer, 84 days, at $1.50	126.00	
1 laborer, { 34¼ days, at $1.50	51.38	
32¼ days, at $1.25 }	40.32	
1 { laborer, 110¼ days, at $1.50	165.38	
stonebreaker, 2 cubic yards, at 60 cents }	1.20	
1 laborer, 103¾ days, at $1.25	129.70	

Wages of mechanics and laborers, etc.—Continued.

1 laborer, 2 days, at $1.25	$2.50
1 laborer, 11¼ days, at $1.25	14.06
1 laborer, 11¼ days, at $1.25	14.06
1 laborer, 40½ days, at $1.25	50.63
1 laborer, 46 days, at $1.25	57.50
1 laborer, 9 days, at $1.25	11.25
1 laborer, 58¼ days, at $1.25	72.82
1 laborer, 63¼ days, at $1.25	79.07
1 laborer, 1 day, at $1.25	1.25
1 laborer, 48¾ days, at $1.25	60.94
1 laborer, 12 days, at $1.25	15.00
1 laborer, 240½ days, at $1.25	300.61
1 { laborer, 77½ days, at $1.25	96.87
stonebreaker, ½ cubic yard, at 60 cents	.30
1 laborer, 67½ days, at $1.25	84.38
1 laborer, 61¼ days, at $1.25	76.56
1 laborer, 198¾ days, at $1.25	248.42
1 laborer, 73 days, at $1.25	91.26
1 { laborer, 55 days, at $1.25	68.75
stonebreaker, 68 11/30 cubic yards, at 60 cents	41.02
1 laborer, 94¾ days, at $1.25	118.44
1 laborer, 81½ days, at $1.25	101.87
1 laborer, 26 days, at $1.25	32.50
1 laborer, 33½ days, at $1.25	41.87
1 laborer, 29¼ days, at $1.25	36.56
1 laborer, 48¼ days, at $1.25	60.31
1 laborer, 1 day, at $1.25	1.25
1 laborer, 45 days, at $1.25	56.25
1 laborer, 43½ days, at $1.25	54.37
1 laborer, { 34 days, at $1.25	42.50
34 days, at $1.50	51.00
1 laborer, { 32 days, at $1.25	40.00
23 days, at $1.50	34.50
1 laborer, 6 days, at $1	6.00
1 laborer, 212 days, at $1	212.00
1 laborer, 20 days, at $1	20.00
1 laborer, 11 days, at $1	11.00
1 laborer, 35 days, at 75 cents	26.25
1 laborer, 34 days, at 75 cents	25.50
1 laborer, 15 days, at 50 cents	7.50
1 water boy, { 139¼ days, at 75 cents	104.44
210 days, at $1	210.00
1 water boy, 43 days, at 50 cents	21.50
1 water boy, 61½ days, at 50 cents	30.75
1 { water boy, 158 days, at 75 cents	118.50
attendant, 118 days, at 75 cents	88.50
1 attendant, 61 days, at 75 cents	45.75
1 { attendant, 146½ days, at 75 cents	109.88
water boy, 88 days, at 50 cents	44.00
1 { weeder, 53 days, at 50 cents	26.50
weeder, 100¾ days, at 75 cents	75.55
attendant, 155¾ days, at 75 cents	116.80
1 stonebreaker, 15¼ cubic yards, at 60 cents	9.15

Wages of mechanics and laborers, etc.—Continued:

1 stonebreaker, 28¾ cubic yards, at 60 cents	$17.25		
1 stonebreaker, 43 cubic yards, at 60 cents	25.80		
1 draftsman, 40¼ days, at $2	80.50		
1 bricklayer, 21 days, at $4	84.00		
1 bricklayer, 33 days, at $4	132.00		
1 bricklayer, 21½ days, at $4	86.00		
1 bricklayer, 13¾ days, at $4	55.00		
1 bricklayer, 29½ days, at $4	118.00		
1 carpenter, 4½ days, at $3	13.50		
1 carpenter, 296 days, at $3	888.00		
1 carpenter, 8 days, at $3	24.00		
1 hodcarrier, 35¾ days, at $2	71.50		
1 workman, 365 days, at $1.75	638.75		
1 wagon and team, 22¾ days, at $3	68.25		
1 wagon and team, 94¼ days, at $3	282.75		
1 wagon and team, 32 days, at $3	96.00		
1 horse and cart, 8¾ days, at $1.50	13.13		
1 horse and cart, 4¾ days, at $1.50	7.12		
1 horse and cart, 51¾ days, at $1.50	77.63		
1 horse and cart, 9 days, at $1.50	13.50		
1 horse and cart, 154½ days, at $1.50	231.76		
1 horse and cart, 124½ days, at $1.50	186.75		
1 horse and cart, 5½ days, at $1.50	8.25		
1 horse and cart, 16¼ days, at 1.50	24.37		
1 horse, 2 days, at $1	2.00		
1 horse, 22 days, at $1	22.00		
1 horse, 179½ days, at 50 cents	89.74		
Total wages, mechanics, etc		$16,069.97	
Total disbursements			$61,199.90
Balance July 1, 1899, to meet liabilities			3,800.10

NATIONAL ZOOLOGICAL PARK, 1898.

Balance July 1, 1898, as per last report		$1,752.34

Disbursements.

General expenses:

Building material	$184.20	
Food	380.99	
Freight	176.80	
Lumber	178.60	
Machinery, tools, etc	77.33	
Fencing, etc	17.71	
Miscellaneous	150.51	
Postage, telegraph, etc	74.22	
Paints, oils, etc	12.08	
Roads, etc	21.00	
Surveying, plans, etc	292.40	
Trees, plants, etc	95.30	
Stationery, books, etc	46.59	
Water supply, etc	37.46	
		1,745.19
Balance July 1, 1899		7.15

NATIONAL ZOOLOGICAL PARK 1897.

Balance July 1, 1898, as per last report $12.52

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1899.

RECAPITULATION.

The total amount of funds administered by the institution during the year ending June 30, 1899, appears from the foregoing statements and the account books to have been as follows:

Smithsonian Institution.

From balance of last year, July 1, 1898		$65,803.02	
(Including cash from executors of Dr. J. H. Kidder)	$5,000.00		
(Including cash from gift of Alex. Graham Bell)	5,000.00		
	10,000.00		
From interest on Smithsonian fund for the year		54,720.00	
From interest on West Shore bonds		1,680.00	
From sales of publications		396.18	
From repayments of freight, etc		9,227.42	
			$131,826.62

Appropriations committed by Congress to the care of the Institution.

International exchanges—Smithsonian Institution:		
From balance of 1896–97	$1.08	
From balance of 1897–98	40.16	
From appropriation for 1898–99	21,000.00	
		21,041.24
American ethnology—Smithsonian Institution:		
From balance of 1896–97	472.08	
From balance of 1897–98	1,831.28	
From appropriation for 1898–99	50,000.00	
		52,303.36
Preservation of collections—National Museum:		
From balance of 1896–97	379.99	
From balance of 1897–98	2,363.51	
From appropriation for 1898–99	165,000.00	
		167,743.50
Furniture and fixtures—National Museum:		
From balance of 1896–97	8.30	
From balance of 1897–98	1,710.46	
From appropriation for 1898–99	35,000.00	
		36,718.76
Heating and lighting, etc.—National Museum:		
From balance of 1896–97	2.84	
From balance of 1897–98	816.87	
From appropriation for 1898–99	14,000.00	
		14,819.71
Printing—National Museum:		
From appropriation for 1898–99		17,000.00
Rent of workshops, etc.—National Museum:		
From balance of 1896–97	.08	
From balance of 1897–98	.08	
From appropriation for 1898–99	4,500.00	
		4,500.16

Postage—National Museum:		
From appropriation for 1898–99		$500.00
Books—National Museum:		
From appropriation for 1898–99		2,000.00
Building repairs—National Museum:		
From balance of 1896–97	$0.58	
From balance of 1897–98	31.98	
From appropriation for 1898–99	4,000.00	
		4,032.56
Galleries—National Museum:		
From balance of 1896–97	.05	
From balance of 1897–98	551.87	
From appropriation for 1897–98	10,000.00	
		10,551.92
Rebuilding sheds, etc.—National Museum:		
From appropriation for 1897–98		28.90
Library of G. Brown Goode—National Museum:		
From appropriation for 1898–99		5,000.00
Astrophysical Observatory—Smithsonian Institution:		
From balance of 1896–97	23.42	
From balance of 1897–98	2,701.78	
From appropriation for 1898–99	10,000.00	
		12,725.20
National Zoological Park:		
From balance of 1896–97	12.52	
From balance of 1897–98	1,752.34	
From appropriation for 1898–99	65,000.00	
		66,764.86

SUMMARY.

Smithsonian Institution	131,826.62	
Exchanges	21,041.24	
Ethnology	52,303.36	
Preservation of collections	167,743.50	
Printing	17,000.00	
Furniture and fixtures	36,718.76	
Heating and lighting	14,819.71	
Rent of workshop	4,500.16	
Postage	500.00	
Books	2,000.00	
Library of G. Brown Goode	5,000.00	
Building repairs	4,032.56	
Galleries	10,551.92	
Rebuilding sheds	28.90	
Astrophysical Observatory	12,725.20	
National Zoological Park	66,764.86	
		547,556.79

The committee has examined the vouchers for payment from the Smithsonian income during the year ending June 30, 1899, each of which bears the approval of the Secretary or, in his absence, of the Acting Secretary, and a certificate that the materials and services charged were applied to the purposes of the Institution.

The committee has also examined the accounts of the several appropriations committed by Congress to the Institution, and finds that the

balances hereinbefore given correspond with the certificates of the disbursing clerk of the Smithsonian Institution, whose appointment as such disbursing officer has been accepted and his bond approved by the Secretary of the Treasury.

The quarterly accounts current, the vouchers, and journals have been examined and found correct.

Statement of regular income from the Smithsonian fund available for use in the year ending June 30, 1900.

Balance on hand June 30, 1899		$74,703.42
(Including cash from executors of J. H. Kidder)	$5,000.00	
(Including cash from Dr. Alex. Graham Bell)	5,000.00	
	10,000.00	
Interest due and receivable July 1, 1899	27,360.00	
Interest due and receivable January 1, 1900	27,360.00	
Interest, West Shore Railroad bonds, due July 1, 1899	840.00	
Interest, West Shore Railroad bonds, due January 1, 1900.	840.00	
		56,400.00
Total available for year ending June 30, 1900		131,103.42

Respectfully submitted.

J. B. HENDERSON,
WILLIAM L. WILSON,
ALEXANDER GRAHAM BELL,
Executive Committee.

WASHINGTON, D. C., *January 11, 1900.*

ACTS AND RESOLUTIONS OF CONGRESS RELATIVE TO THE SMITHSONIAN INSTITUTION, NATIONAL MUSEUM, ETC.

(Continued from previous reports.)

[Fifty-fifth Congress, Third Session.]

SMITHSONIAN INSTITUTION.

Resolved by the Senate and House of Representatives of the United States of America in Congress assembled, That the vacancy in the Board of Regents of the Smithsonian Institution, of the class other than members of Congress, shall be filled by the reappointment of James B. Angell, a resident of Michigan, whose term of office expires on January nineteenth, eighteen hundred and ninety-nine. (Joint resolution No. 10, approved January 24, 1899. Statutes, XXX, 1387.)

Report on Salaries.—For the fiscal year nineteen hundred and one, and annually thereafter, a report in detail, shall be made to Congress of the salaries of all officers and employees paid from appropriations under the Smithsonian Institution. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1085.)

Library of Congress.—One assistant (librarian) (in charge of Smithsonian deposit), one thousand five hundred dollars. (Legislative, executive, and judicial act, approved February 24, 1899. Statutes, XXX, 853.)

INTERNATIONAL EXCHANGES.

International exchanges.—For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, and the purchase of necessary books and periodicals, twenty-four thousand dollars. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1085.)

NATIONAL MUSEUM.

National Museum.—For cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the

National Museum, including ten thousand dollars for furnishing new galleries, and including salaries or compensation of all necessary employees, twenty-five thousand dollars.

For expense of heating, lighting, electrical, telegraphic, and telephonic service for the National Museum, fourteen thousand dollars.

For continuing the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employees, one hundred and seventy thousand dollars, of which sum five thousand dollars may be used for necessary drawings and illustrations for publications of the National Museum.

For purchase of books, pamphlets, and periodicals for reference in the National Museum, two thousand dollars.

For repairs to buildings, shops, and sheds, National Museum, including all necessary labor and material, six thousand dollars.

For rent of workshops and temporary storage quarters for the National Museum, four thousand and forty dollars.

For postage stamps and foreign postal cards for the National Museum, five hundred dollars. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1086.)

PUBLIC PRINTING AND BINDING ALLOTMENT.

For the Smithsonian Institution, for printing labels and blanks, and for the "Bulletins" and "Proceedings" of the National Museum, the editions of which shall not be less than three thousand copies, and binding in half Turkey or material not more expensive, scientific books and pamphlets presented to and acquired by the National Museum Library, seventeen thousand dollars. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1119.)

BUREAU OF AMERICAN ETHNOLOGY.

American ethnology.—For continuing ethnological researches among the American Indians, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees and the purchase of necessary books and periodicals, fifty thousand dollars, of which sum not exceeding one thousand dollars may be used for rent of building. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1086.)

ASTROPHYSICAL OBSERVATORY.

Astrophysical Observatory.—For maintenance of Astrophysical Observatory, under the direction of the Smithsonian Institution, including salaries of assistants, the purchase of necessary books and periodicals, apparatus, printing and publishing results of researches,

not exceeding one thousand five hundred copies, repairs and alteration of buildings, and miscellaneous expenses, ten thousand dollars. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1086.)

NATIONAL ZOOLOGICAL PARK.

National Zoological Park.—For continuing the construction of roads, walks, bridges, water supply, sewerage and drainage; and for grading, planting, and otherwise improving the grounds; erecting and repairing buildings and inclosures; care, subsistence, purchase, and transportation of animals, including salaries or compensation of all necessary employees; the purchase of necessary books and periodicals, and general incidental expenses not otherwise provided for, seventy-five thousand dollars; one-half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States; and of the sum hereby appropriated five thousand dollars shall be used for continuing the entrance into the Zoological Park from Woodley Lane, and opening driveway into Zoological Park, from said entrance along the bank of Rock Creek, and five thousand dollars shall be expended in widening the Adams Mill road entrance to the Zoological Park from the corner of Eighteenth street and Columbia road, by acquiring by purchase or condemnation of land sufficient to widen the same to a width of one hundred feet, and such road, so widened, shall form a parkway under the control of the Zoological Park. (Sundry civil act for 1900, approved March 3, 1899. Statutes, XXX, 1086.)

PHILADELPHIA COMMERCIAL EXPOSITION.

AN ACT providing for a national exposition of American products and manufactures at the city of Philadelphia, for the encouragement of the export trade.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That there shall be held a national exposition of American products and manufactures, suitable for export, at the city of Philadelphia, in the State of Pennsylvania, in the year eighteen hundred and ninety nine, under the auspices of the Philadelphia Exposition Association; and that there may be admitted to said exposition such articles not of American manufacture and such other objects as may conduce to the interest of the exposition and may be useful for comparison with American products and manufactures: *Provided*, That the United States shall not be liable for any of the expenses attending or incident to such an exposition, nor by reason of the same, further than hereinafter provided for.

SEC. 2. That all articles which shall be imported from foreign countries for the sole purpose of exhibition at said exposition, upon which there shall be a tariff or customs duty, shall be admitted free of pay-

ment of duty, customs fees, or charges, under such regulations as the Secretary of the Treasury shall prescribe; but it shall be lawful at any time during the exhibition to sell, for delivery at the close thereof, any goods or property imported for and actually on exhibition in the exhibition building, or on the grounds, subject to such regulation for the security of the revenue and for the collection of import duties as the Secretary of the Treasury shall prescribe: *Provided*, That all such articles when sold or withdrawn for consumption in the United States shall be subject to the duty, if any, imposed upon such articles by the revenue laws in force at the date of importation; and all penalties prescribed by the law shall be applied and enforced against the persons who may be guilty of any illegal sale or withdrawal.

SEC. 3. That for the purpose of enabling the collection in foreign markets of samples of merchandise of the character in favor and demand therein, of illustrating the manner in which merchandise for such markets should be prepared and packed, together with necessary business data concerning said samples to be displayed at the said exposition for the instruction and benefit of American manufacturers and merchants, and thereby laying the foundation of a great system of national commercial education, there is hereby appropriated, out of any money in the Treasury not otherwise appropriated, to the board of trustees of the Philadelphia Museums the sum of fifty thousand dollars: *Provided*, That this sum shall be expended only for the purposes set forth in this section, and the samples of merchandise so collected shall become the property of said Philadelphia Museums. The Department of State is hereby directed to cooperate in this work, through the consular service of the United States, in such a manner as may be agreed upon by conference between the Secretary of State and the trustees and officers of the exposition association.

SEC. 4. That to aid in providing buildings necessary for the purposes of the exposition (said buildings to be erected on lands set aside by the city of Philadelphia for the board of trustees of the Philadelphia Museums, and after the close of the exposition to be available for one or more of the various purposes of the Philadelphia Museums corporation, as set forth in its charter), and for the purpose of collecting, installing, and caring for such an exhibit by the United States Government as may be found expedient and desirable, there is hereby appropriated, out of any money not otherwise appropriated, to the said Philadelphia Exposition Association the sum of three hundred thousand dollars: *Provided*, That no liability against the Government shall be incurred, and no payments of money under this section shall be made, until the officers of said exposition shall have furnished the Secretary of the Treasury proofs to his satisfaction that there have been obtained by said board of trustees of the Philadelphia Museums and said Philadelphia Exposition Association, in good faith, subscriptions, contribu-

tions, donations, or appropriations, from all sources, for the purpose of said exposition and the buildings to be used therefor, a sum aggregating not less than an amount equal to the sum appropriated in this section.

SEC. 5. That the United States shall not in any manner, nor under any circumstances, be liable for any of the acts, doings, proceedings, or representations of said board of trustees of the Philadelphia Museums or the Philadelphia Exposition Association, its officers, agents, servants, or employees, or any of them, or for service, salaries, labor, or wages of said officers, agents, servants, or employees, or any of them, or for any subscriptions to the capital stock, or for any certificates of stock, bonds, mortgages, or obligations of any kind issued by said corporation, or for any debts, liabilities, or expenses of any kind whatever attending such corporation or accruing by reason of the same, other than are in this Act provided.

SEC. 6. That nothing in this Act shall be so construed as to create any liability of the United States, direct or indirect, for any debts or obligations incurred, nor for any claim for aid or pecuniary assistance from Congress or the Treasury of the United States in support or liquidation of any debts or obligations created by said board of trustees of the Philadelphia Museums or the Philadelphia Exposition Association in excess of the sums herein appropriated.

(Approved, December 21, 1898. Statutes, XXX, 768.)

JOINT RESOLUTION authorizing foreign exhibitors at the commercial exposition to be held in Philadelphia, Pennsylvania, in eighteen hundred and ninety-nine, to bring to this country foreign laborers from their respective countries for the purpose of preparing for and making their exhibits under regulations prescribed by the Secretary of the Treasury.

Resolved by the Senate and House of Representatives of the United States of America in Congress assembled, That the Act of Congress approved February twenty-sixth, eighteen hundred and eighty-five, prohibiting the importation of foreigners under contract to perform labor, and the Acts of Congress prohibiting the coming of Chinese persons into the United States, and the Acts amendatory of these Acts, shall not be construed, nor shall anything therein operate to prevent, hinder, or in any wise restrict any foreign exhibitor, representative, or citizen of a foreign nation, or the holder who is a citizen of a foreign nation of any concession or privilege from the Philadelphia Exposition Association of Pennsylvania from bringing into the United States, under contract, such mechanics, artisans, agents, or other employees, natives of their respective foreign countries, as they or any of them may deem necessary for the purpose of making preparations for installing or conducting their exhibits or of preparing or installing or conducting any business authorized or permitted under or by virtue of or pertaining to any concession or privilege which may have been

granted by the Philadelphia Exposition Association of Pennsylvania in connection with such exposition: *Provided, however,* That no alien shall by virtue of this Act enter the United States under contract to perform labor except by express permission, naming such alien, of the Secretary of the Treasury; and any such alien who may remain in the United States for more than three months after the close of the exposition shall thereafter be subject to all the processes and penalties applicable to aliens coming in violation of the alien contract-labor law aforesaid.

(Approved, March 1, 1899. Statutes, XXX, 1390.)

PAN-AMERICAN EXPOSITION.

AN ACT to encourage the holding of a Pan-American Exposition on the Niagara frontier, within the county of Erie or Niagara, in the State of New York, in the year nineteen hundred and one.

Whereas it is desirable to encourage the holding of a Pan-American Exposition on the Niagara frontier, within the county of Erie or Niagara, in the State of New York, in the year nineteen hundred and one, to fittingly illustrate the marvelous development of the Western Hemisphere during the nineteenth century, by a display of the arts, industries, manufactures, and products of the soil, mines, and sea; and

Whereas the proposed Pan-American Exposition, being confined to the Western Hemisphere, and being held in the near vicinity of the great Niagara cataract, within a day's journey of which reside forty million people, would unquestionably be of vast benefit to the commercial interests, not only of this country, but of the entire hemisphere, and should therefore have the sanction of the Congress of the United States; and

Whereas satisfactory assurances have already been given by the diplomatic representatives of Canada, Mexico, the Central and South American Republics, and most of the States of the United States that these countries and States will make unique, interesting, and instructive exhibits peculiarly illustrative of their material progress during the century about to close; and

Whereas no exposition of a similar character as that proposed has ever been held in the great State of New York; and

Whereas the Pan-American Exposition Company has undertaken to hold such exposition, beginning on the first day of May, nineteen hundred and one, and closing on the first day of November, nineteen hundred and one: Therefore,

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That all articles that shall be imported from foreign countries for the sole purpose of exhibition at said exposition upon which there shall be a tariff or customs duty shall be admitted free of payment of duty, customs fees, or charges, under

such regulations as the Secretary of the Treasury shall prescribe; but it shall be lawful at any time during the exposition to sell for delivery at the close thereof any goods or property imported for or actually on exhibition in the exposition buildings, or on the grounds, subject to such regulation for the security of the revenue and for the collection of import duties as the Secretary of the Treasury shall prescribe: *Provided*, That all such articles when sold or withdrawn for consumption in the United States shall be subject to the duty, if any, imposed upon such articles by the revenue laws in force at the date of importation, and all penalties prescribed by law shall be applied and enforced against the persons who may be guilty of any illegal sale or withdrawal: *And provided further*, That all necessary expenses incurred in carrying out the provisions of this section, including salaries of customs officials in charge of imported articles, shall be paid to the Treasury of the United States by the Pan-American Exposition Company, under regulations to be prescribed by the Secretary of the Treasury.

SEC. 2. That there shall be exhibited at said exposition by the Government of the United States, from its Executive Departments, the Smithsonian Institution and National Museum, the United States Commission of Fish and Fisheries, the Department of Labor, and the Bureau of the American Republics, such articles and material as illustrate the function and administrative faculty of the Government in time of peace, and its resources as a war power, and its relations to other American Republics, tending to demonstrate the nature of our institutions and their adaption to the wants of the people. And to secure a complete and harmonious arrangement of such Government exhibit, a board of management shall be created, to be charged with the selection, purchase, preparation, transportation, arrangement, safe-keeping, exhibition, and return of such articles and materials as the heads of the several Departments and the secretary of the Smithsonian Institution, the Commissioner of Fish and Fisheries, the Commissioner of Labor, and the Director of the Bureau of the American Republics may respectively decide shall be embraced in said Government exhibit. The President may also designate additional articles for exhibition. Such board shall be composed of one person to be named by the head of each Executive Department, one by the head of the Smithsonian Institution and National Museum, one by the head of the United States Commission of Fish and Fisheries, one by the Commissioner of Labor, and one by the Director of the Bureau of the American Republics. The President shall name one of said persons so detailed as chairman, and the board itself shall appoint its secretary, disbursing officer, and such other officers as it may deem necessary. The members of said board of management, with other officers and employees of the Government who may be detailed to assist them, including officers of the Army and Navy, shall receive no compensation in addition to their regular sala-

ries, but they shall be allowed their actual and necessary traveling expenses, together with a per diem in lieu of subsistence, to be fixed by the Secretary of the Treasury, while necessarily absent from their homes engaged upon the business of the board. Officers of the Army and Navy shall receive this allowance in lieu of the transportation and mileage now allowed by law. Any provision of law which may prohibit the detail of persons in the employ of the United States to other service than that which they customarily perform shall not apply to persons detailed for duty in connection with the Pan-American Exposition. Employees of the board not otherwise employed by the Government shall be entitled to such com ensation as the board may determine. The disbursing officer shall give bond in the sum of twenty thousand dollars for the faithful performance of his duties, said bond to be approved by the Secretary of the Treasury. The Secretary of the Treasury shall advance to said officer, from time to time, under such regulations as the Secretary of the Treasury may prescribe, a sum of money from the appropriation for the Government exhibit, not exceeding at any one time three-fourths of the penalty of his bond, to enable him to pay the expenses of said exhibit as authorized by the board of management herein created.

SEC. 3. That the Secretary of the Treasury shall cause a suitable building or buildings to be erected on the site selected for the Pan-American Exposition for the Government exhibits from plans to be approved by the board, and he is hereby authorized and directed to contract therefor in the same manner and under the same regulations as for other public buildings of the United States; but the contract for said building or buildings shall not exceed the sum of two hundred thousand dollars, said sum being hereby appropriated for said purpose. out of any money in the Treasury not otherwise appropriated. The Secretary of the Treasury is authorized and required to dispose of such building or buildings, or the material composing the same, at the close of the exposition, giving preference to the city of Buffalo or to the said Pan-American Exposition Company to purchase the same at an appraised value, to be ascertained in such manner as may be determined by the Secretary of the Treasury.

SEC. 4. That the United States shall not be liable on account of said exposition for any expense incident to or growing out of same, except for the construction of the building or buildings hereinbefore provided for, and for the purpose of paying the expense of selection, preparation, purchase, installation, transportation, care, custody, and safe return of exhibits by the Government, for the employment of proper persons as officers and assistants by the board of management created by this Act and for their expenses, and for the maintenance of the said building or buildings and other contingent expenses, to be approved by the

chairman of the board of management, or, in the event of his absence or disability, by such other officer as the board may designate and the Secretary of the Treasury upon itemized accounts and vouchers; and the total cost of said building or buildings shall not exceed the sum of two hundred thousand dollars; nor shall the expenses of said Government exhibit for each and every purpose connected therewith, including transportation, exceed the sum of three hundred thousand dollars, amounting in all to not exceeding the sum of five hundred thousand dollars, which sum is hereby appropriated, out of any money in the Treasury not otherwise appropriated, the sum of five hundred thousand dollars, or so much thereof as may be necessary, to be disbursed by the board of management hereinbefore created, of which not exceeding the sum of ten thousand dollars shall be expended for clerical service: *Provided*, That no liability against the Government shall be incurred, and no expenditure of money under this Act shall be made, until the officers of said exposition shall have furnished the Secretary of the Treasury proofs to his satisfaction that there has been obtained by said exposition corporation subscriptions of stock in good faith, contributions, donations, or appropriations from all sources for the purposes of said exposition a sum aggregating not less than five hundred thousand dollars.

SEC. 5. That medals, with appropriate devices, emblems, and inscriptions commemorative of said Pan-American Exposition, and of the awards to be made to the exhibitors thereat, shall be prepared at some mint of the United States for the board of directors thereof, subject to the provisions of the fifty-second section of the coinage Act of eighteen hundred and ninety-three, upon the payment of a sum not less than the cost thereof; and all the provisions, whether penal or otherwise, of said coinage Act against the counterfeiting or imitating of coins of the United States shall apply to the medals struck and issued under this Act.

SEC. 6. That the United States shall not in any manner nor under any circumstances be liable for any of the acts, doings, proceedings, or representations of said Pan-American Exposition Association, its officers, agents, servants, or employees, or any of them, or for service, salaries, labor, or wages of said officers, agents, servants, or employees, or any of them, or for any subscriptions to the capital stock, or for any certificates of stock, bonds, mortgages, or obligations of any kind issued by said corporation, or for any debts, liabilities, or expenses of any kind whatever attending such corporation, or accruing by reason of the same.

SEC. 7. That nothing in this Act shall be so construed as to create any liability of the United States, direct or indirect, for any debt or obligation incurred, nor for any claim for aid or pecuniary assistance

from Congress or the Treasury of the United States in support or liquidation of any debts or obligations created by said commission in excess of appropriations made by Congress therefor.

SEC. 8. That the appropriation herein made of five hundred thousand dollars in all shall take effect and become available immediately upon the passage of this Act.

(Approved, March 3, 1899, Statutes, XXX, 1022.)

PARIS EXPOSITION.

Paris Exposition.—For each and every purpose named in the paragraph in the sundry civil appropriation Act approved July first, eighteen hundred and ninety-eight, under the heading "Paris Exposition," seven hundred and fifty thousand dollars, of which amount not exceeding one hundred and twenty thousand dollars may be used for clerk hire in the United States and in Paris, and the limit of appropriations provided for in the provisions of said paragraph shall be extended three hundred thousand dollars, or to nine hundred and fifty thousand dollars in all, said appropriation to be available until expended: *Provided*, That of said latter sum one hundred and fifty thousand dollars shall be for the exhibits by the Secretary of Agriculture provided for in said paragraphs.

For the construction of necessary buildings in connection with said exposition, two hundred thousand dollars, to be immediately available.

For pay of jurors, sixty thousand dollars, or so much thereof as may be necessary, to be available until expended; and the sums herein and heretofore appropriated on account of the Paris Exposition shall be in full of all appropriations to be made on account of said Exposition by Congress, and no deficiency shall be created therein. (Sundry civil Act for 1900, approved March 3, 1899. Statutes XXX, 1117.)

OHIO CENTENNIAL AND NORTHWEST TERRITORY EXPOSITION.

AN ACT to encourage the holding of the Ohio Centennial and Northwest Territory Exposition at the city of Toledo, Ohio.

Whereas it is desirable to encourage the holding of the Ohio Centennial and Northwest Territory Exposition at the city of Toledo, in the State of Ohio, in the year nineteen hundred and two or nineteen hundred and three, as the Ohio general assembly may hereafter determine, for the exhibition of the resources of the United States of America, Hawaii, Cuba, Porto Rico, and the Philippines, and the progress and civilization of the American countries, and for a display of the arts, industries, manufactures, and products of the soil, mine, and sea; and

Whereas it is desirable to commemorate by an appropriate naval display the important victory of Commodore Perry in the western

waters of Lake Erie, beside which waters said exposition is located; and

Whereas it is desirable for its historical and educational effect that there be given an exhibition of the Indians of North America, and especially the tribes of the old Northwest Territory; and

Whereas it is desirable that an exhibition shall be made of the great staples of the original Northwest Territory and Ohio Valley region, which contributes so largely to domestic and international commerce; and

Whereas encouragement should be given to an exhibit of the arts, industries, manufactures, and products illustrative of the progress and development of that and other sections of the country; and

Whereas such exhibition should be international as well as national in its character, in which the people of this country, of Mexico, the Central and South American Governments, and other States of the world should participate, and should, therefore, have the sanction of the Congress of the United States; and

Whereas it is desirable and will be highly beneficial to bring together at such an exposition the people of the United States and other States of this continent; and

Whereas the Ohio Centennial Company, a corporation, has undertaken to hold such exposition, beginning on the first day of May, nineteen hundred and two or nineteen hundred and three, and closing on the first day of November, nineteen hundred and two or nineteen hundred and three: Therefore,

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That there shall be exhibited by the Government of the United States at said Ohio Centennial and Northwest Territory Exposition from the Executive Departments, the Smithsonian Institution and National Museum, the Commission of Fish and Fisheries, the Department of Labor, and the Bureau of American Republics such articles and materials as illustrate the function and administrative faculty of the Government, its resources as a war power, and its relations to other American Republics; and, to secure a complete and harmonious arrangement of said Government exhibit, a board of management shall be created, to be charged with the selection, purchase, preparation, transportation, arrangement, safe-keeping, exhibition, and return of such articles and materials as the heads of said departments and institutions of the Government may respectively decide shall be embraced in said Government exhibit. The President may also designate additional articles for exhibition. Such board shall be composed of one member to be detailed by the head of each Executive Department, one by the head of the Smithsonian Institution and National Museum, one by the head of the United States Fish Commission, one by the Commissioner of Labor, and one by the

Director of the Bureau of American Republics. The President shall name one of said persons so detailed as chairman, and the board itself shall appoint its secretary, disbursing officer, and such other officers as it may deem necessary. The members of said board of management, with other officers and employees of the Government who may be detailed to assist them, including officers of the Army and Navy, shall receive no compensation in addition to their regular salaries, but they shall be allowed their actual and necessary traveling expenses, together with a per diem in lieu of subsistence, to be fixed by the Secretary of the Treasury, while necessarily absent from their homes engaged upon the business of the board. Officers of the Army and Navy shall receive this allowance in lieu of the transportation and mileage now allowed by law. Any provision of law which may prohibit the detail of persons in the employ of the United States to other service than that which they customarily perform shall not apply to persons detailed for duty in connection with the Ohio Centennial and Northwest Territory Exposition. Employees of the board not otherwise employed by the Government shall be entitled to such compensation as the board may determine. The disbursing officer shall give bond in the sum of twenty thousand dollars for the faithful performance of his duties, said bond to be approved by the Secretary of the Treasury. The Secretary of the Treasury shall advance to said officer from time to time, under such regulations as the Secretary of the Treasury may prescribe, a sum of money from the appropriation for the Government exhibit, not exceeding at any one time three-fourths of the penalty of his bond, to enable him to pay the expenses of said exhibit as authorized by the board of management herein created.

SEC. 2. That the Secretary of the Treasury shall cause a suitable building or buildings, from plans to be approved by the board of management, to be erected on the site selected at the Ohio Centennial and Northwest Territory Exposition for the Government exhibit; and he is hereby authorized and directed to contract therefor in the same manner and under the same regulations as for other public buildings of the United States; but the contract for said building or buildings shall not exceed the sum of two hundred thousand dollars, said sum being hereby appropriated for said purpose out of any money in the Treasury not otherwise appropriated. The Secretary of the Treasury shall dispose of such building or buildings or the material composing the same after the close of the exposition, giving preference to the city of Toledo or the Ohio Centennial Company to purchase the same at an appraised value, to be ascertained in such manner as the President and Secretary of the Treasury may determine; and whatever sum may be so realized shall be covered into the Treasury of the United States.

SEC. 3. That for the purpose of paying the expenses of the selection, purchase, preparation, transportation, installation, care, and return of

said Government exhibit, and for the employment of proper persons as officers and assistants by the board of management created by this Act and for their expenses, and for the maintenance of the building hereinbefore provided for, and for other contingent expenses incidental to the Government exhibit, to be approved by the chairman of the board of management, or in the event of his absence or disability by such other officer as the board may designate, upon itemized accounts and vouchers, there is hereby appropriated, out of any money in the Treasury not otherwise appropriated, the sum of three hundred thousand dollars, or so much thereof as may be necessary, to be disbursed by the board of management hereinbefore created, of which not exceeding the sum of ten thousand dollars shall be expended for clerical service: *Provided*, That no liability against the Government shall be incurred and no expenditure of money under this Act shall be made until the officers of said exposition shall have furnished the Secretary of the Treasury proofs to his satisfaction that there has been obtained by said exposition corporation subscriptions of stock in good faith, contributions, donations, or appropriations from all sources for the purpose of said exposition, a sum aggregating not less than five hundred thousand dollars, nor until the State of Ohio shall by legislative enactment have appropriated a sum of money equal to that herein appropriated.

SEC. 4. That all articles which shall be imported from foreign countries for the sole purpose of exhibition at said exposition upon which there shall be a tariff or customs duty shall be admitted free of payment of duty, customs fees, or charges, under such regulations as the Secretary of the Treasury shall prescribe; but it shall be lawful at any time during the exhibition to sell, for delivery at the close of the exposition, any goods or property imported for and actually on exhibition in the exposition buildings or on its grounds, subject to such regulations for the security of the revenue and for the collection of import duties as the Secretary of the Treasury shall prescribe: *Provided*, That all such articles, when sold or withdrawn for consumption in the United States, shall be subject to the duty, if any, imposed upon such articles by the revenue laws in force at the date of importation, and all penalties prescribed by law shall be applied and enforced against such articles and against the persons who may be guilty of any illegal sale or withdrawal: *And provided further*, That all necessary expenses incurred in carrying out the provisions of this section, including salaries of customs officials in charge of imported articles, shall be paid to the Treasury of the United States by the Ohio Centennial Company, under regulations to be prescribed by the Secretary of the Treasury.

SEC. 5. That medals with appropriate devices, emblems, and inscriptions commemorative of said Ohio Centennial and Northwest Territory Exposition and of the awards to be made to exhibitors thereat be pre-

pared at some mint in the United States for the board of directors thereof, subject to the provisions of the fifty-second section of the coinage Act of eighteen hundred and ninety-three, upon the payment by the Ohio Centennial Company of a sum not less than the cost thereof; and all the provisions, whether penal or otherwise, of said coinage Act against the counterfeiting or imitating of coins of the United States shall apply to the medal struck and issued under this Act.

SEC. 6. That the United States shall in no manner and under no circumstances be liable for any bond, debt, contract, expenditure, expense, or liability of any kind whatever of the said Ohio Centennial Company, its officers, agents, servants, or employees, or incident to or growing out of said exposition, nor for any amount whatever in excess of the five hundred thousand dollars herein authorized; and the heads of the Executive Departments, the Smithsonian Institution and National Museum, the Commission of Fish and Fisheries, the Department of Labor, and the Bureau of American Republics, and the board of management herein authorized, their officers, agents, servants, or employees, shall in no manner and under no circumstances expend or create any liability of any kind for any sum in excess of the appropriations herein made or create any deficiency.

SEC. 7. That at the close of the Ohio Centennial and Northwest Territory Exposition the exhibits of the United States Government shall be returned to the several departments or bureaus from which they were received; and such collections as may be acquired by the board by purchase, preparation, gift, or otherwise, illustrating the natural resources, industries, customs, and commerce of the other American Republics, shall be placed for permanent preservation in the United States National Museum.

SEC. 8. That the appropriation herein made, of five hundred thousand dollars in all, shall take effect and become available immediately upon the proof being made to the satisfaction of the Secretary of the Treasury that the conditions prescribed in section three of this Act have been complied with.

(Approved, March 3, 1899. Statutes, XXX, 1346.)

LAFAYETTE MONUMENT.

Lafayette Monument.—For the purpose of aiding in defraying the cost of a pedestal, and completing in a suitable manner the work of erecting a monument in the city of Paris to General Lafayette, designed by the Lafayette Memorial Commission, as a feature of the participation of the United States in the Paris Exposition of nineteen hundred the Secretary of the Treasury shall be, and is hereby authorized to purchase in the market twenty-five thousand dollars worth of silver bullion, or so much thereof as may be necessary for the purpose herein

provided for, from which there shall be coined at the mints of the United States silver dollars of the legal weight and fineness to the number of fifty thousand pieces, to be known as the Lafayette dollar, struck in commemoration of the erection of a monument to General Lafayette, in the city of Paris, France, by the youth of the United States, the devices and designs upon which coins shall be prescribed by the Director of the Mint, with the approval of the Secretary of the Treasury, and all provisions of law, relative to the coinage, and legal tender quality, of the present silver dollars shall be applicable to the coins issued under this Act, and when so coined, there is hereby appropriated from the Treasury the said fifty thousand of souvenir dollars, and the Secretary of the Treasury is authorized to place the same at the disposal of the Lafayette Memorial Commission, a commission organized under the direction and authority of the Commissioner-General for the United States to the Paris Exposition of nineteen hundred. (Sundry Civil Act for 1900, approved March 3, 1899. Statutes XXX, 1117).

REPORT

OF

S. P. LANGLEY,

SECRETARY OF THE SMITHSONIAN INSTITUTION,

FOR THE YEAR ENDING JUNE 30, 1899.

To the Board of Regents of the Smithsonian Institution.

GENTLEMEN: I have the honor to present herewith my customary report, showing the operations of the Institution during the year ending June 30, 1899, including the work placed under its direction by Congress in the U. S. National Museum, the Bureau of American Ethnology, the International Exchanges, the National Zoological Park, and the Astrophysical Observatory.

Following the precedent of several years, I have in the body of this report given a general account of the affairs of the Institution and its bureaus, while the appendix presents more detailed statements by the persons in direct charge of the different branches of the work. Independently of this, the operations of the National Museum are fully treated in a separate volume of the Smithsonian Report, and the report of the work of the Bureau of American Ethnology constitutes a volume prepared under the supervision of the Director of that Bureau.

THE SMITHSONIAN INSTITUTION.

THE ESTABLISHMENT.

I have to record two changes in the membership of the Establishment during the year, caused by the resignation of the Secretary of State, William R. Day, and the Secretary of the Interior, Cornelius N. Bliss, who were succeeded by the Hon. John Hay and the Hon.

E. A. Hitchcock. As organized at the end of the fiscal year, the Establishment consisted of the following ex officio members.

WILLIAM MCKINLEY, *President of the United States.*
GARRET A. HOBART, *Vice-President of the United States.*
MELVILLE W. FULLER, *Chief Justice of the United States.*
JOHN HAY, *Secretary of State.*
LYMAN J. GAGE, *Secretary of the Treasury.*
RUSSELL A. ALGER, *Secretary of War.*
JOHN W. GRIGGS, *Attorney-General.*
CHARLES EMORY SMITH, *Postmaster-General.*
JOHN D. LONG, *Secretary of the Navy.*
E. A. HITCHCOCK, *Secretary of the Interior.*
JAMES WILSON, *Secretary of Agriculture.*

The Establishment, which formerly held occasional meetings, has not been assembled for some time.

THE BOARD OF REGENTS.

In accordance with a resolution of the Board of Regents adopted January 8, 1890, by which its annual meeting occurs on the fourth Wednesday of each year, the Board met on January 25, 1899, at 10 o'clock a. m. The journal of its proceedings will be found, as hitherto, in the annual report of the Board to Congress, though reference is made later on in this report to several matters upon which action was taken at that meeting.

The Secretary announced the death of the Hon. Justin S. Morrill, and after appropriate remarks by several of the Regents, the following resolutions were adopted by a rising vote.

Whereas the Board of Regents of the Smithsonian Institution are called upon to mourn the death, on December 28, 1898, of Justin Smith Morrill, for fifteen years a member of the Board, and to some members of it a still older colleague in the Senate of the United States:

Resolved, That the Board desire to place on record the expression of their sense of the exceptional loss they have sustained in the death of their venerable colleague; and that they unite with their fellow-citizens throughout the land in recognizing the great services of Senator Morrill to the whole country during a public career of forty-three years in Congress, where, amid other great national services, his public life in the special domain of education alone was the most important of that of any single American. With all these duties, his time, his ripened knowledge of practical affairs, and his counsel, were always at the service of the Smithsonian Institution, where no member of this Board represented its interests in Congress more persistently or more effectually than he.

Resolved, That by his personal character, no less than by his mental endowments, he endeared himself to all his associates on this Board,

who feel that they have lost in him, not only a counselor and adviser, but a dear and honored friend; and that without desiring to intrude upon the private grief of his family, they wish to express to them their share in their sorrow.

Resolved, That this minute be entered as a part of the journal of the Board and a copy be transmitted to the family of Senator Morrill.

Further mention of Senator Morrill will be found in the necrology.

On January 18, 1899, Senator O. H. Platt, of Connecticut, was appointed a Regent by the President of the Senate to succeed Senator Morrill. Dr. J. B. Angell, of Michigan, was reappointed a Regent by a joint resolution of Congress, approved by the President January 24, 1899.

The Secretary's report and the reports of various committees were submitted, the Secretary mentioning among other matters of interest the need of a systematic exploration of new territories, the cooperation of the Army and Navy in securing animals for the National Zoological Park, the aerodromic work in cooperation with the War Department, and the necessity of a law protecting sites on the public domain containing aboriginal ruins.

The question of the relation of the Smithsonian Institution to the proposed National University, was referred to a committee composed of the Hon. J. B. Henderson, Dr. W. L. Wilson, Dr. A. Graham Bell, Dr. J. B. Angell, and the Hon. R. R. Hitt.

Since the meeting of the Board, Senator William Lindsay, of Kentucky, was, on March 3, 1899, appointed by the President of the Senate a Regent to succeed Senator Gray, whose term as Senator had expired.

GENERAL CONSIDERATIONS.

The operations of the Institution are constantly on the increase, but while it is impossible to give an idea in brief of their extent and variety, it may not be superfluous to at least indicate them in a few words. The parent institution devotes itself to its primary functions of "the increase and diffusion of knowledge" along the lines of its original programme, adopted by the Board of Regents in 1847 and reenacted by them in 1851, interpreting the establishing act.

The "increase" is advanced by researches now being actively promoted in nearly every department of knowledge by its own officials, by their preparation of results for publication, and by grants to special investigators and explorers, by administering legacies for scientific ends, furnishing information and advice, and in like ways. These, the original functions of the Institution, are performed primarily out of the income of the Smithsonian fund and other private funds, with a comparatively small force, which is yet independently busied in the supervision of researches carried on in the Institution's bureaus, under governmental appropriations directed by it.

Through the National Museum, the Bureau of Ethnology, the National Zoological Park, and the Astrophysical Observatory, researches are promoted in all the branches of natural history, zoology, anthropology, and astro-physics, both by the opportunities afforded in Washington and by aid given students elsewhere.

In furtherance of its second function in the "diffusion" of knowledge the Institution issues and freely distributes three classes of publications: Original memoirs, useful scientific publications, and popular expositions of scientific work and thought, in untechnical language, prepared by eminent scholars and thinkers.

Nor is the instruction of the people forgotten, since, through the museum and park, great object lessons are placed before the hundreds of thousands of visitors to the national capital from all parts of the United States. The pupils in the public schools of Washington benefit greatly by these opportunities. It has become a part of their routine to visit, under the care of a teacher, the Smithsonian Institution and National Museum buildings, as well as the park; while those outside the city benefit indirectly through the numerous excursions of teachers and the stimulus and suggestion they may thus receive.

Perhaps unique as an agency in promotion of scientific and literary relationships abroad is the exchange service, which facilitates intercourse between governmental and learned societies and institutions and scientific men, and which, as is stated later, now has 31,000 correspondents, of whom more than 20,000 are in Europe and Asia.

Standing near the threshold of the second half century of the Institution's life, it is plain to see how scientific conditions have changed, and easier to estimate the relative position which the Institution holds toward American and foreign scientific endeavor. The most noteworthy fact is the much greater esteem in which American science is held abroad, the better knowledge had of its representatives, and the more friendly and even intimate relations between American workers and their foreign colleagues. To this end it may unquestionably be said that the Institution has contributed a very large share, and that its influence abroad has so notably increased within the past ten years, as to constitute a very gratifying fact.

Its relations with Government and with the leading academies are close and friendly, and in accordance with its motto "PER ORBEM" its encouragement of research is as freely extended to students in England and on the Continent of Europe, or anywhere else, as to those within the United States. The great part which scientific thought and activity are playing in the world to-day has partially contributed to this condition, and the recognition of the Institution by our own Department of State as the adviser in matters relating to international science has greatly contributed to this end.

It is clear that American science is almost incomparably stronger than it was fifty years ago. With rich endowments for learned societies, museums, and universities, and vastly augmented avenues for scientific publication, American investigators no longer have the need of the Institution for purposes of publication, as in times past, and its most friendly relations to our great colleges and universities are more those of an intermediary for their communications abroad than in any other capacity.

I wish that these relations may grow still more intimate and be still more helpful in promoting the common cause. When the changed conditions alluded to are fully appreciated, and the differentiation which those conditions make necessary fully understood, new methods for promoting the highest research by friendly arrangement will undoubtedly arise in this country.

ADMINISTRATION.

The writer continues to be chiefly occupied with purely administrative duties, though certain important ones connected more particularly with the interests of science consume a minor part of his time.

In the administration of the various bureaus placed by the Government under the direction of the Institution there continues to be a greatly increasing amount of business that must be transacted in the Secretary's office, and it has been found necessary during the year to provide additional assistance to attend to the increasing correspondence pertaining directly to these and to legislative and civil service matters; but though the Secretary has already been authorized by the Board to call upon Congress for a special appropriation for the organization of a force for the general administrative work of the bureaus, he can only refer to what has been said on this point in previous reports, and especially in that for 1897, and add an expression of his regret that Congress has as yet done nothing to relieve the Institution's fund of the burdens thus laid upon it for the support of purely Government interests.

FINANCES.

The permanent funds of the Institution are as follows:

Bequest of Smithson, 1846		$515,169.00
Residuary legacy of Smithson, 1867		26,210.63
Deposits from savings of income, 1867		108,620.37
Bequest of James Hamilton, 1875	$1,000.00	
Accumulated interest on Hamilton fund, 1895	1,000.00	
		2,000.00
Bequest of Simeon Habel, 1880		500.00
Deposits from proceeds of sale of bonds, 1881		51,500.00
Gift of Thomas G. Hodgkins, 1891		200,000.00
Portion of residuary legacy, T. G. Hodgkins, 1894		8,000.00
Total permanent fund		912,000.00

The Regents also hold certain approved railroad bonds, forming part of the fund established by Mr. Hodgkins for investigations of the properties of atmospheric air.

By act of Congress approved by the President March 12, 1894, an amendment was made to section 5591 of the Revised Statutes, the fundamental act organizing the Institution, as follows:

> The Secretary of the Treasury is authorized and directed to receive into the Treasury, on the same terms as the original bequest of James Smithson, such sums as the Regents may, from time to time, see fit to deposit, not exceeding with the original bequest the sum of $1,000,000: *Provided*, That this shall not operate as a limitation on the power of the Smithsonian Institution to receive money or other property by gift, bequest, or devise, and to hold and dispose of the same in promotion of the purposes thereof.

Under this provision the above fund of $912,000 is deposited in the Treasury of the United States, bearing interest at 6 per cent per annum, the interest alone being used in carrying out the aims of the Institution.

The unexpended balance at the beginning of the fiscal year, July 1, 1898, as stated in my last annual report, was $65,803.02. The total receipts for the year were $66,023.60, being $56,400 derived from the interest on the permanent fund in the Treasury and elsewhere, and $9,623.60, received from miscellaneous sources.

The disbursements for the year amounted to $57,123.20, the details of which are given in the report of the executive committee. The balance remaining to the credit of the Secretary on June 30, 1899, for the expenses of the Institution was $74,703.42, which, it will be remembered, includes $10,000 referred to in previous reports, $5,000 of which was received from the estate of Dr. J. H. Kidder, and a like sum from Dr. Alexander Graham Bell, the latter a gift made personally to the Secretary to promote certain physical researches. This latter sum was, with the donor's consent, deposited by the Secretary to the credit of the current funds of the Institution.

This balance also includes the interest accumulated on the Hodgkins and other funds, which is held against certain contingent obligations, besides relatively considerable sums held to meet obligations which may be expected to mature as a result of various scientific investigations and publications in progress.

During the fiscal year 1898–99 Congress charged the Institution with the disbursement of the following appropriations:

International Exchanges, Smithsonian Institution	$21,000
American Ethnology, Smithsonian Institution	50,000
United States National Museum:	
Preservation of Collections	165,000
Furniture and fixtures	35,000
Heating and lighting	14,000

United States National Museum—Continued.

Postage	$500
Repairs to buildings	4,000
Rent of workshops	4,500
Galleries	10,000
Books	2,000
Purchase of library of the late G. Brown Goode	5,000
Printing	17,000
National Zoological Park	65,000
Astrophysical Observatory	10,000

The executive committee has examined all the vouchers for disbursements made during the fiscal year, and a detailed statement of the receipts and expenditures will be found reported to Congress, in accordance with the provisions of the sundry civil acts of October 2, 1888, and August 5, 1892, in a letter addressed to the Speaker of the House of Representatives.

The vouchers for all the expenditures from the Smithsonian fund proper have been likewise examined and their correctness certified to by the executive committee, whose statement will be published, together with the accounts of the fund appropriated by Congress, in that committee's report.

The estimates for the fiscal year ending June 30, 1900, for carrying on the Government interests under the charge of the Smithsonian Institution and forwarded as usual to the Secretary of the Treasury were as follows:

International Exchanges	$24,000
American Ethnology	65,000
National Museum:	
Preservation of collections	180,000
Furniture and fixtures	25,000
Heating and lighting	15,000
Postage	500
Repairs to buildings	10,000
Rent of workshops	4,040
Books	2,000
Investigation of new territory	50,000
Printing	17,000
National Zoological Park	100,000
Astrophysical Observatory	15,000

The appropriations made by Congress for the fiscal year 1900 were as follows:

International Exchanges, Smithsonian Institution	$24,000
American Ethnology, Smithsonian Institution	50,000
Astrophysical Observatory, Smithsonian Institution	10,000
National Museum:	
Furniture and fixtures	25,000
Heating and lighting	14,000
Preservation of collections	170,000
Postage	500

National Museum—Continued.

Books	$2,000
Rent of workshops	4,040
Building repairs	6,000
National Zoological Park	75,000

BUILDINGS.

I have set aside a small room in the Institution's building, in the south tower; have had some alterations made, with a view to increasing its light and cheerfulness, and am having plans prepared for the purpose of bringing together there, in a simple and attractive manner, objects which may be of interest to children. This small collection, I hope, may serve a double purpose—that of interesting the child's mind to the point of inquisitiveness which finally results in study, and to the setting of an example which may cause such little collections, and, possibly, even larger ones, arranged especially for children, to grow up in other sections of the country.

The Regents' room on the second floor has been partially renovated, and a room in the half story below has been fitted for the care of the valuable collection of physical apparatus used by Professor Henry and others in various investigations.

The hall devoted to the bird collection has been rendered lighter by the rearrangement of exhibition cases and by replacing the wooden floor by terrazzo pavement. In the hallway of the east basement a cement pavement has been laid and the heating apparatus rearranged.

The electric plant installed in the building some years ago was at the time considered ample for the needs of the Institution, but during the past year it became necessary to secure a large increase of power, and this was finally secured by taking service from the local electric light and power company, and the operation of the plant in the Institution has been discontinued for the present.

Improvements in the Museum, Astrophysical Observatory, and Zoological Park buildings are mentioned elsewhere.

RESEARCH.

One of the most important functions of the Institution during the half century of its existence has been the promotion of original research in the various branches of science. It is so now, and investigations of great benefit have continued to be carried on by persons directly connected with the Institution or by others aided by special grants.

In this connection the parent Institution has found it possible to continue to render aid to investigations through the Hodgkins fund, as mentioned somewhat in detail below, and it has also continued the rental of a table at the Naples Zoological Station, where certain biological researches have been conducted.

The Secretary has pursued his studies in regard to aerodromic experiments, and an account of many years' investigation of this subject has been prepared and is nearly ready for the press, but publication is deferred foı a time.

The Institution subscribed for a considerable number of copies of the Astronomical Journal and the Astrophysical Journal for distribution in the general interest of the diffusion of knowledge.

The National Museum has been concerned in investigations relating to anthropology, biology, and geology, and the Bureau of American Ethnology has devoted its energies to inquiries into aboriginal customs and languages, while in the Astrophysical Observatory the Secretary himself has continued his researches in connection with the spectrum and solar radiation. The results of these several lines of investigation are mentioned in the paragraphs devoted to those bureaus or in the appendix to this report in detailed statements of those in charge of the work.

HODGKINS FUND.

Since the last Report the Hodgkins memoir, "La Vie sur les hauts Plateaux," submitted in collaboration by Prof. A. L. Herrera and Dr. D. Vergara Lope, of the City of Mexico, has been published in French by the authors, and a considerable number of copies was secured by the Institution for distribution to some of the principal libraries of the country. This memoir, which was designated by the committee for honorable mention, with silver medal, appears as a timely contribution to our knowledge of the atmosphere in connection with the welfare of man.

As mentioned in last year's Report, a memoir embodying the results of a research to determine the ratios of specific heats, prosecuted by Drs. Lummer and Pringsheim under a grant from the Hodgkins fund, has been published in the Smithsonian Contributions to Knowledge and distributed in accordance with the established custom of the Institution. This important investigation is to be continued under the direction of the Physikalisch-Technische Reichsanstalt, of Berlin, with which establishment Drs. Lummer and Pringsheim are connected.

Although necessarily limited in number by the conditions of the bequest, the investigations now in progress under grants from the Hodgkins fund are reported as making satisfactory advance along their special lines of research, among which the progress in the investigations of the higher regions of the atmosphere, made at the Blue Hill Meteorological Observatory by means of kites ascending vertically between 2 and 3 miles, are to be specifically mentioned.

An additional grant of $500 to Mr. A. Lawrence Rotch, the director, was approved May 4, 1899, for a series of experiments in wireless telegraphy by the use of kites at varying altitudes, a part of this sum being for the purchase of apparatus, which on the termination of the

research will remain the property of the Institution. Prof. E. C. Pickering, of Harvard University Observatory, while unable to give his personal attention to the details of the investigation, furthered its inception by valuable suggestions as to the electrical part of the experiments.

The meteorological experiments at Blue Hill, just alluded to, and which are circumstantially described by Mr. Rotch, the director, in his printed bulletins, have been steadily progressing, aided, as before stated, by a Hodgkins grant.

On February 12, 1899, one of the automatic kites sent up from the station reached the height of 12,507 feet, or nearly 2⅓ miles, making the highest ascension on record. As a proof that the increased height of this flight is due to improved apparatus and methods, rather than to some fortunate combination of air currents at the time, it may be added that the average height of five flights made at Blue Hill between February 23 and 28 of the same year was about 10,280 feet, this average being only 806 feet less than the highest single flight ever attained previous to that of February 12, 1899.

It has been found at the Blue Hill Observatory that the exposure of the instruments carried by kites is probably equal to that of instruments at the ground, and the results have so far proved more satisfactory than those recorded by instruments carried by balloons. The stationary position of the kites, as opposed to the motion of free balloons with the atmospheric currents, also renders possible a record of the progressive changes of the atmosphere, as well as a comparison of similar coincident phenomena at a fixed point on the surface of the earth. Kites can, moreover, reach higher altitudes than captive balloons have yet attained, and are better fitted to withstand the wind velocities to be encountered at great heights. Experiments are still in progress, and it is hoped that a union of the two methods of exploring the atmosphere—by means of kites and by captive balloons—may eventually give better results than any yet attained by either method alone.

It may be noted that Mr. William A. Eddy, who experimented, by the aid of a small grant from the Hodgkins fund, in 1894 at Bayonne, New Jersey, was the first to demonstrate the adaptability of a modern kite of his own device to the purposes of scientific experiment.

The investigation on the intensity of sound, conducted by Prof. A. G. Webster, of Clark University, Worcester, Mass., is reported as making satisfactory progress. It is expected that by means of the new instrument designed by Professor Webster an absolute measure of the intensity of a sound, even when rapidly varying, will be recorded with an accuracy not hitherto attained.

Extended researches upon the propagation, reflection, and diffraction of sound are in progress, and results of much practical value are

looked for from experiments in connection with the action of the megaphone and phonograph, and in verification of the general theory of resonators.

The research noted in my last report, by Prof. William Hallock, of Columbia University, New York, having for its object the complete analysis of a particle of air under the influence of articulate sounds, is still in progress and has been reported upon in detail, so far as completed. It is believed that this investigation will yield broader results than were, perhaps, anticipated, and that the experiments of Professor Hallock in connection with the synthesis of sound by means of magnetic induction and the telephone, which are largely conducted by means of a tone synthesizer of his own invention, will help to solve questions of the greatest importance, not only in reference to the motion of air in articulate speech, but also in phonetics in general.

It having been proposed by Prof. L. A. Bauer, the editor of the periodical entitled "Terrestial Magnetism," to extend its scope to include "Atmospheric Electricity," his request for a grant from the Hodgkins fund was considered and referred to specialists distinguished for their researches in connection with the subjects treated of in that publication. A grant having been heartily recommended, was approved for the sum of $200, and the title of the journal was changed to "Terrestrial Magnetism and Atmospheric Electricity." To further as effectively as possible in this instance the aim of the Institution in dispensing the fund, Professor Bauer offered, as a recognition of this grant, to send one hundred copies of the periodical of which he is the editor to a list of educational and scientific establishments approved by the Institution. This action is the more gratifying as it is known that efforts for the advancement of our knowledge of the subject of atmospheric electricity are entirely consonant with the wishes of the founder of the Hodgkins fund.

Coincidentally with the publication of the Hodgkins prize competition the establishment of the Hodgkins medal of the Smithsonian Institution was announced, to be awarded only for exceptionally important contributions to our knowledge of the nature and properties of atmospheric air, or for original and practical applications of existing knowledge to the welfare of mankind.

The advisory committee, after giving the subject special consideration, in December, 1898, recommended the award of the medal to Prof. James Dewar, of the Royal Institution of Great Britain, for his meritorious researches on the liquefaction and solidification of atmospheric air, for his investigations of the physical properties of substances in contact with liquid air, and for his discovery of the extraordinary magnetic properties of liquid oxygen.

In accordance with the recommendation of the committee, the first Hodgkins gold medal of the Smithsonian Institution was awarded to

Professor Dewar, to whom, by the courtesy of the Department of State, it was conveyed through the American ambassador in London, in April, 1899.

NAPLES TABLE.

The following applications for the seat at the Smithsonian table in the Naples Zoological Station were approved for the year ending with June, 1899:

Dr. J. H. Gerould, of Dartmouth College, whose appointment to the table dated from November 1, 1898, remained in occupation until the end of February, 1899.

Dr. F. W. Bancroft received an appointment for the period from March 1 to July 1, 1899, going to the station from the University of Berlin, where he had been studying under a traveling fellowship from Harvard University.

During the absence in Europe of Prof. E. B. Wilson, of Columbia University, New York, who represents the Society of American Naturalists on the Naples table advisory committee, his duties were courteously assumed by Dr. T. H. Morgan, of Bryn Mawr, who will be remembered as an early appointee to the station. Dr. Wilson has been a valued member of the advisory committee since its formation, and it was a pleasure to approve his application for the Smithsonian seat for three months during the summer of 1899.

The occupation of the Smithsonian table during the past year has been equivalent to the time of one student for ten months, or the full annual period for which it is practicable to study at Naples.

With the exception of the time covered by the absence of Dr. Wilson, the personnel of the advisory committee has remained unchanged since my last report. I have again to record my appreciation of the efficient aid afforded me by the committee in examining applications and recommending action in connection with appointments to the table.

The continued courtesy of Dr. Dohrn, the director of the station, in arranging for the accommodation of all the Smithsonian appointees, when two, or even three, have desired to be at Naples at the same time, has frequently been of advantage to students, who, being prevented by their engagements at other institutions from reaching Naples on the date fixed for their reception, would otherwise have been deprived of a part of the period allotted to them by the Institution.

The lease of the seat in the Naples station on behalf of the Institution for the second term of three years expired June 1, 1899. A contract for a third term has not yet been concluded, but the urgent desire of the leading biologists of the country that the Institution should continue to afford this undoubted advantage to our investigators will receive due consideration in reaching a decision.

EXPLORATIONS.

As in past years a large amount of exploration work has been accomplished by the Institution through the National Museum and the Bureau of Ethnology, particularly in anthropological and natural history lines, detailed accounts of which are given in the appendix.

It was expected that Congress would make provision for extended explorations in the new possessions acquired as a result of the war with Spain, but the time seemed inopportune for general work. It was found possible, however, to make limited investigations in Puerto Rico partly through the cooperation of the Commissioner of Fish and Fisheries.

It seems important that I should repeat my recommendation of last year in regard to the new regions, that not only for practical purposes, but as a contribution to the general intelligence of mankind, the Government should institute scientific inquiry as to the natural history, geology, geography, ethnology, archæology, and scientific utilities of any new possessions. These inquiries should be made coherently and harmoniously on the part of the various Government interests involved.

In April, 1899, the Institution communicated with the Secretary of the Navy in regard to the Island of Guam, a new possession of the United States, and the commander of the U. S. S. *Yosemite* was given instructions about collecting ethnological, zoological, and natural history specimens in that region.

The attention of the Institution having been called to the probability of obtaining valuable archæological information through the diplomatic and consular representatives of the United States in Canada, Central America, and South America, the Department of State issued the following circular:

ANTIQUARIAN DISCOVERIES.

DEPARTMENT OF STATE,
Washington, February 15, 1899.

To the Diplomatic and Consular Officers of the United States in Central and South America and Mexico, and the Consular Officers in Canada.

GENTLEMEN: In view of the interests of anthropological science, and the probability that, without systematic plans for gathering such information, important antiquarian discoveries may escape the notice of American investigators, you are requested to ascertain and to report to the Department any discoveries of this character that may be made in your district.

In making such reports you will forward copies of any original narrations or descriptions that may be available.

These reports are called for at the suggestion of the Smithsonian Institution in a letter of the 1st instant, which is appended hereto.

I am, gentlemen, your obedient servant,

THOMAS W. CRIDLER,
Third Assistant Secretary.

SMITHSONIAN INSTITUTION,
Washington, U. S. A., February 1, 1899.

SIR: In reply to the letter of the Department of the 12th ultimo, inclosing copies of letters from Mr. Whitelaw Reid and the Duke de Loubat, suggesting that the diplomatic and consular officers in Mexico and Central and South America be given instructions to report any antiquarian discoveries that may be made within their districts, and soliciting my view on the subject, I beg to say that our knowledge of the American aborigines could be materially advanced if such instruction were sent to diplomatic and consular representatives in Mexico, Central America, South America, the Dominion of Canada, and Newfoundland, and if the data thus obtained were communicated to the Smithsonian Institution it would be referred to the proper bureau there and would be brought to the attention of scientific men.

I am informed that in Mexico and certain of the Central American countries, where Government offices exist for the direction of antiquities, the securing of this information would be a comparatively simple matter.

Very respectfully yours,
S. P. LANGLEY,
Secretary.

The SECRETARY OF STATE.

PUBLICATIONS.

It has been chiefly through the medium of its publications that the Institution has aimed to fulfill the expressed wish of its founder for "the diffusion of knowledge."

The results of original researches have thus been made known by a wide and general distribution of scientific works to libraries and institutions of learning throughout the world, every branch of human knowledge being represented to a greater or less degree in the 250 or more volumes published by the Institution and its bureaus during the last fifty years, while as each year's additions are made to the several series of works they become more and more important as a library of valued reference. Many of the earlier volumes are out of print, so that it is impossible to comply with requests for the full series where new libraries are added to the lists. The editions of each series have from time to time been increased as far as the income of the Institution or the Government appropriation would permit; but the increase can not keep pace with the growing demand for the publications. By a judicious geographical distribution, the aim has been to place the books in libraries where they may be accessible to all who desire to consult them.

During the year there have been added to the miscellaneous collections three works—a supplement to the Bibliography of Chemistry, an index to the Literature of Thallium, and an index to the Literature of Zirconium.

The Annual Reports of the Institution for 1896 and 1897, which had been unavoidably delayed beyond the close of the last fiscal year by

necessary printing in connection with the war with Spain, were issued during the current year, and some progress was made on the Report for 1898.

The Museum volume of the 1896 Report of the Institution was likewise distributed, and although much progress was made in the 1897 and 1898 volumes, it was not possible to complete them before the close of the fiscal year.

Of publications of the National Museum there appeared volume 20 of the Proceedings and pamphlet copies of papers to comprise volume 21 of that series. Two additional volumes, parts 2 and 3 of Jordan and Evermann's Fishes of North and Middle America, were also published.

The Secretary, remembering that Congress in the fundamental act had authorized him to call on heads of departments in the interests of the Zoological Park, issued a pamphlet entitled "Animals desired for the National Zoological Park at Washington, D. C.," which was distributed chiefly to diplomatic and army and navy officers of the United States, through the courtesy of the Secretaries of State, War, and Navy. This, which will be spoken of more fully in another place, promises very good results through the interest already shown in the object by the officers in question.

The 1898 Report of the American Historical Association was transmitted to Congress in the spring of 1899, and much of it was in type before July 1.

In accordance with the act of incorporation of the National Society of the Daughters of the American Revolution, the Secretary transmitted to Congress the first report of that society, and the work was printed as a public document, no copies, however, being made available for distribution by the Institution.

The manuscript of the Annals of the Astrophysical Observatory, Volume I, was sent to the printer near the close of the year, and most of the illustrations have been engraved.

The Institution aided in the preparation of an exhaustive bibliography on the fossil vertebrates of North America by Mr. O. P. Hay, but the manner of its publication has not been determined.

LIBRARY.

The number of volumes, parts of volumes, pamphlets, and charts added to the library has aggregated 36,663. A considerable number of these were retained in the working libraries of the Institution and the Museum, but the great majority were transferred to the Smithsonian deposit in the Library of Congress. The improved facilities for reference and care of books in the new building make it possible to send to the Library of Congress a much greater proportion of books received than heretofore, and it is gratifying to report that most of

the vast mass of Smithsonian material that had accumulated in the old Library has now been arranged in a systematic manner, and is available for study.

The east stack, in which the transactions of learned societies and periodicals belonging to the Smithsonian deposit were placed, lacked the necessary conveniences to enable scholars to consult these series direct.

The Librarian of Congress has recognized the disadvantages of the present situation, and proposes asking Congress to provide furniture for several of the large halls, in order that these series may be made available to the numerous scientific students desiring to consult them. In this connection he also intends to ask provision for the proper custody of the collection. I sincerely trust that Congress may see its way clear to supporting these various measures of the Librarian for making the library worthy of its new home and of the nation.

A special room is being fitted up in the Institution for the care of engravings and works relating to the fine arts.

As in nearly every other line of Smithsonian activities, so in the library, lack of sufficient room prevents the introduction of desired improvements. The cataloguing and reading rooms, that seemed ample for the growth of many years, have in a brief time become greatly crowded, and additional quarters will soon be absolutely necessary.

CORRESPONDENCE.

In accordance with the general plan which has been in vogue in the Institution ever since its foundation, careful attention has been given to all communications received, and it has been endeavored so far as possible to furnish information called for. A considerable number of letters contained inquiries having no bearing on the activities of the Institution, or on science in general, but even in such cases, where data could be conveniently had, replies have been made. A large number of communications have also been referred to other bureaus or establishments of the Government having immediate supervision of the matters to which they related.

Letters pertaining to matters of a special nature, or which on account of their importance deserve special care in handling, with a view to insuring attention are entered in a register kept especially for the purpose, and 3,381 of these special entries have been made during the year covered by this report. In accordance with the rule observed with this correspondence, a record is made of each individual step in the treatment of the letter until it can be reported that it has been completely attended to and placed on file.

It is gratifying to note that, owing to the careful manner in which the routine has been established and systematized, there has been

little or no occasion during the year to add to or alter the rules governing the conduct of correspondence, and the wisdom of the plan of filing has been demonstrated by the ability to find almost invariably at a moment's notice any papers relating to a subject called up for attention. The card index, devised by my regretted friend William C. Winlock and begun on January 1, 1893, of all letters received and letters written within the parent Institution, as well as those pertaining to the several bureaus which require under the rules to pass through the Secretary's office, has been kept current and has fully demonstrated its efficiency. This index has, since it was first instituted, been extended back to cover letters as early as January 1, 1892.

INTERNATIONAL CONGRESSES.

International Congress of Zoology.—Prof. O. C. Marsh was, on July 14, 1898, nominated by the Institution and later was appointed by the Secretary of State as United States delegate to an International Congress of Zoology, held at Cambridge, England, August 23, 1898.

International Congress of Orientalists.—On December 9, 1898, the Secretary designated Dr. Paul Haupt and Prof. Charles R. Lanman as delegates of the Institution, and the Department of State accredited them as delegates of the United States, to attend the Twelfth International Congress of Orientalists at Rome, on October 2, 1899, and on February 3, 1899, Professor Jastrow was recognized as Government delegate to the same congress.

International Catalogue of Scientific Literature.—The Secretary and the Librarian were appointed delegates to a Conference on an International Catalogue of Scientific Literature to be held in London on July 12, 1898, and postponed to October 11. By reason of this postponement the Secretary was unable to attend the meeting, the United States Government being represented by Dr. Adler, who presented the following report to the Secretary of State:

WASHINGTON, *November 15, 1898.*

SIR: Having been appointed, together with Mr. S. P. Langley, Secretary of the Smithsonian Institution, a delegate on the part of the United States to the Conference on an International Catalogue of Scientific Literature, to be held at London on July 12, 1898, we proceeded abroad on July 2.

The British Government found it expedient to postpone the conference until October 11. At the request of the Department, and with the consent of the Secretary of the Smithsonian Institution, I continued abroad and attended the conference. Mr. Langley's official duties necessitated his return to the United States in September.

The deliberations were in continuation of those had at a previous conference in 1896, at which this Government was also represented. Satisfactory conclusions were reached, leaving only such questions as can be definitely determined by an international committee, on which the United States is represented by Mr. Langley.

I have the honor to transmit herewith the acta of the conference. The procés verbal will be issued later, and a copy forwarded to the Department.

I beg most respectfully to bring to your notice the report of the delegates of the United States to the first conference (Prof. Simon Newcomb and Dr. J. S. Billings), to repeat the recommendations made by them, and to further draw your attention to the recommendation of the Secretary of the Smithsonian Institution, all of which is contained in Senate Document No. 43, Fifty-fourth Congress, second session, a copy of which is herewith appended.

I have much pleasure in informing you that both in public and privately the delegates of the United Kingdom and of other powers expressed a very generous appreciation of the scientific activity of the United States, and I beg to be allowed to commend to the favorable consideration of the Department the recommendation of such legislation as will enable the United States to worthily take its share in this highly important international project.

I have the honor to be, sir, your most obedient servant,

CYRUS ADLER.

The SECRETARY OF STATE.

His reply is given herewith:

DEPARTMENT OF STATE,
Washington, November 25, 1898.

SIR: I have to acknowledge the receipt of your letter of the 15th instant in regard to the work of the Conference on an International Catalogue of Scientific Literature which met at London on the 11th ultimo and to which you were a delegate on the part of the United States.

With reference to your suggestion that such legislation be recommended to Congress as will enable the United States to worthily take its share in this highly useful and important international project, I have to state that I had already, in the estimates for this Department for the fiscal year ending June 30, 1900, submitted an item of $10,000, or so much thereof as may be necessary, for the purpose of carrying out on the part of the United States the recommendation of the International Conference on a Catalogue of Scientific Literature, and for the expense of clerk hire and for the other expenses of the work of cataloguing the scientific publications of the United States, the same to be expended under the direction of the Secretary of the Smithsonian Institution, and pointed out that as the preparation of the catalogue is to begin on January 1, 1900, it would be necessary for appropiate action to be taken by Congress at its forthcoming session, if this Government is to participate therein.

In support of this recommendation, I inclosed as appendices a copy of the Congressional document to which you refer and a copy of your report on the conference of 1896. The estimates are now in print and it is too late to have your present letter included therein; but I shall, upon the assembling of Congress, communicate it to that body in further support of the item.

I am, sir, your obedient servant,

[L. S.] JOHN HAY.

Prof. CYRUS ADLER,
Smithsonian Institution, Washington, D. C.

Since that date many difficulties have arisen over questions of classification and management of the catalogue, but I still hope that some way may be found of bringing to a successful issue this worthy project.

EXPOSITIONS.

The several bureaus of the Institution participated in the Trans-Mississippi Exposition at Omaha, which opened June 1, 1898, and continued for five months. An account of the exhibits will be printed elsewhere.

In an act of Congress approved March 3, 1899, the sum of $300,000 was appropriated for a Government exhibit in connection with the Pan-American Exposition to be held at Buffalo in 1901, with $200,000 additional for the erection of a building. Dr. F. W. True has been designated to represent the Smithsonian Institution and the National Museum on the Government Board of Management, and Mr. W. V. Cox as chief special agent.

On the date above mentioned an act was also approved allowing similar amounts for a Government exhibit and building at the Ohio Centennial Exposition to be held at Toledo in 1902 or 1903, as may be determined upon hereafter. This appropriation is, however, conditional upon a grant of $500,000 to the exposition by the legislature of the State of Ohio and the raising of an equal amount by subscription.

MISCELLANEOUS.

Documentary history of the Institution.—The legislative history of the Institution from 1877 to date, mentioned in the Secretary's last Report, has been extended and revised, but not yet published.

Gifts and bequests.—Among the important collections received by the Institution during the year may be mentioned a large series of medals gathered by Hon. Charles Francis Adams, United States minister to England, and deposited with the Smithsonian Institution by his son, Henry Adams; somė Japanese masks received through Dr. Alexander Graham Bell; a valuable early book on whales presented by the British Museum; and a large number of very interesting historical objects pertaining to the war with Spain, transmitted by the Secretary of the Navy and the Secretary of War.

Foreign institutions.—The relations of the Smithsonian Institution with other institutions of learning throughout the world continue to be most cordial. During the past year communications were received inviting participation in the Centennial of the Russian Imperial Military Academy of Medicine, the Gauss-Weber Memorial at Gottingen, the Stokes Celebration at Cambridge, and the Centenary of the Royal Institution of Great Britain.

Dr. W. L. Wilson, Regent, and the Secretary were appointed Smithsonian representatives at the Royal Society celebration and were made

honorary members of that important scientific body. Dr. Wilson was unavoidably prevented from attendance; the Institution, however, participated vicariously in the imposing ceremonies of the Centenary, and at a banquet presided over by the Duke of Northumberland, and at which the Prince of Wales and the Duke of Connaught were present, its representative responded to the toast on behalf of the foreign guests proposed by the Lord Chancellor.

The Secretary also attended the semicentennial celebration in honor of Prof. William Stokes, held at Cambridge, and paid a visit to Pembroke College, at Oxford, at which James Smithson was graduated. In a conspicuous place in the library of the college is the Smithson memorial tablet, presented by the Institution a few years ago, and in a special bookcase suitably marked is a full set of the publications of the Institution. The Secretary also visited Genoa and found the tablets sent by the Regents had been properly placed on the tomb of Smithson on the heights of San Benigno and in the English church in the city.

NATIONAL MUSEUM.

The Smithsonian Museum, or, as it is commonly called, the National Museum, has grown to be the largest interest of the Institution, and the future growth of this most important work is limited only by the willingness of Congress to provide means for its extension and maintenance. Each year seems to be a phenomenal one in the increase of the collections, and more and more imperative does it become that greater space be provided for the display of the valuable contributions constantly being received. The collections now comprise nearly four and a half million specimens, illustrating practically every branch of anthropology, biology, and geology. During the past year the accessions numbered more than 200,000 specimens.

The new galleries have been of great benefit in relieving some of the crowded halls, and additional storage quarters enable a better handling of objects than heretofore, but a proper display of all the collections will not be possible until a new building is erected, as the present one, even with the aid just referred to, is in parts in a state of continued congestion.

Among new accessions of special interest may be mentioned about 1,000 ethnological objects of exceptional value pertaining to the Indians of the Great Plains and Rocky Mountains, archæological collections made by Mr. Holmes in Mexico, collections by Mr. Beckwith in Puerto Rico, and some electrical apparatus from the Boston fire department, and from other sources, and also a large number of most valuable historical objects relating to the war with Spain, secured through the courtesy of the Navy Department, several of the bureau chiefs taking a lively interest in collecting articles of special importance. There were also received from the Society of Colonial Dames and the Society

of Daughters of the American Revolution some very interesting historical objects pertaining to colonial and Revolutionary war periods. The most extensive additions to the biological collections were from Puerto Rico. In the department of geology the Museum is enriched by the addition of several large accessions transferred by the U. S. Geological Survey. These objects and others of importance are mentioned more in detail in the appendix to this Report.

The Museum continues to benefit greatly by the exchange of duplicate specimens with foreign museums, some very valuable accessions having recently been made in this way.

It became possible several years ago to arrange for the distribution of duplicates of certain classes of objects to educational establishments, and the extent to which this very valuable work can be carried on is limited only by lack of adequate appropriations for assistance in the preparation of the specimens. The total distribution during the year included 25,000 specimens, consisting mainly of marine invertebrates, rocks and ores, and casts of prehistoric stone implements.

BUREAU OF AMERICAN ETHNOLOGY.

Field operations and office studies have been continued by the Bureau of American Ethnology under the direction of Maj. J. W. Powell. In Maine, Florida, Arizona, California, Alaska, Patagonia, and other regions explorations and researches were carried on which resulted in the addition of much valuable material to the National Museum collections, and in the partial solution of important problems concerning the aborigines of America. The details of the work of the bureau are fully set forth by Major Powell in the appendix to this Report.

Under special authorization of the Secretary, Mr. Holmes and Mr. McGee made somewhat exhaustive researches in California, gaining considerable new knowledge of the archæology of the regions visited and gathering a large number of very interesting prehistoric relics.

At the Omaha Exposition the Bureau of Ethnology cooperated in an Indian Congress that created considerable popular interest. The Indians lived in houses and lodges of their own construction. A special object of interest was a Wichita grass house brought from Indian Territory, and afterwards transferred to Washington City.

INTERNATIONAL EXCHANGES.

The Smithsonian Exchange Service was established to facilitate the interchange of scientific publications between domestic and foreign institutions and societies, and of public documents between the United States and foreign countries, resulting in most valuable additions to the Library of Congress either directly or by the deposit of exchanges received by the Smithsonian Institution.

It has constantly grown until it has carried the work of the Institution, and incidentally its own name, through every part of the world, and it would be well to recall the constantly growing number of its correspondents, which at the close of the year had reached 31,000, of whom 23,000 are outside the United States.

No portion of the work done by the Institution more justifies the motto of its seal, "PER ORBEM," and possibly even the general reader may care to look at the indications of the vast amount of correspondence that is carried on as given in the report of the assistant in charge.

The weight of packages handled by the service during the past year was 317,883 pounds and their total number was 97,835, representing 10,322 foreign societies, 13,378 foreign individuals, and 7,269 domestic societies and individuals.

There were received from abroad and distributed to domestic addresses 30,645 packages. The total number of correspondents at the close of the year was 30,969, an increase of 1,511 over the previous year. These are scattered over nearly every portion of the globe.

The new distributing agencies established in Vienna and Budapest have proved very beneficial and relieved the former severe pressure at the Leipzig agency.

The small increase in the appropriation granted by Congress has enabled the service to dispatch packages by more expeditious routes than heretofore, and greater improvement still is expected during the coming year.

The thanks of the Institution are again due to those forwarding agencies who for many years have granted special courtesies in facilitating the dispatch of exchanges.

The very complete record system practiced in the Service made it possible to determine the contents, source, and destination of several boxes lost by the foundering of two ocean steamers, and to arrange for their duplication in nearly every instance.

NATIONAL ZOOLOGICAL PARK.

As the result of special effort a considerable number of valuable animals have been added to the zoological park.

Through the courtesy of the Secretaries of State, War, and Navy, an illustrated circular prepared by the Secretary and describing the zoological park has been distributed to officers of the United States throughout the world, and in this way the special wants of the park have been made known and methods of caring for animals to be transmitted have been explained. As one of the results of this effort, a very interesting group of animals was forwarded by Commander Todd of the U. S. S. *Wilmington*, having been obtained while on a cruise up

the Amazon River, and other naval and army officers have called attention to the probability of securing further important accessions.

Among other additions have been several secured in Japan by Dr. Alexander Graham Bell. Though all of those who have thus aided in developing the national collections have their contributions acknowledged, the Secretary desires to take this opportunity of again expressing to them his thanks.

Improvements in the park buildings and roads have been made as far as available appropriations permitted.

The most important work in road construction has been grading and macadamizing the road to Klingle Ford entrance, at an annual expense of $5,000, in compliance with the terms of the appropriation act.

An expensive yet necessary improvement, now that roadways leading to the park have been completed at established grades, is the construction of a tight fence around the entire park, in order to prevent the entrance of dogs and other predatory animals, and thus to allow small game to run at large. There appears to be great need of grading the precipitous banks on the eastern boundary of the park, a work, however, which would require the expenditure of about $10,000.

The aquarium, of which I have already spoken to Congress, and which is a feature of the highest popular interest, deserves a building, and an estimate to cover the cost of commencement of one ample to accommodate the immediate needs of the park has been included in the general estimate for appropriations, though the item is not specifically given.

A primary object for which Congress established the zoological park was the preservation of fast vanishing species of American animals.

The same phenomenon which was noticeable in the western part of of the country some years ago is now occurring in Alaska. With the advent of the settler and the railroad in the West, the great herds of animals which ranged over our western territory were practically exterminated, though by strenuous efforts here and there small collections of the buffalo and other large interesting mammals, like those in the National Zoological Park, have been kept alive. Whether a race can be made to survive in this way is open to question, but the effort at least should be made, and the Institution is trying to promote this survival.

The United States still possesses at Kadiak Island, on the southeast coast of Alaska, a few living specimens of the largest carniverous animal now in the *world*—a monster bear—which has not, to my knowledge, at any time been brought into captivity. I have been trying for two years, through American companies on the island, to obtain live specimens of this and other great mammals of Alaska with the hope of preserving the species before the inevitable opening of all that distant

territory of the United States to civilization and settlement will have resulted in the extermination of its large fauna, but these efforts have hitherto been wholly unsuccessful.

A secondary purpose of Congress in establishing the park was the recreation of the people, and this purpose has certainly proved of great public benefit, while the advantage of the zoological park as a means of education has become interestingly manifest by the constantly increasing number of school children who, with their teachers, visit the park and there make a life study of the animals. Interesting illustrations of this are given in the superintendent's report.

ASTROPHYSICAL OBSERVATORY.

The operations of the Astrophysical Observatory during the last year have been of continued interest, although the work is done under difficulties, owing to the excessively confined quarters, which were originally intended only as temporary sheds for the instruments, and which are shown in the accompanying illustration.

In my last Report attention was called to the remarkable periodic changes in the absorption of water vapor in the air, as recently discovered by inspection of the records of the Astrophysical Observatory. This matter has been more carefully investigated this year, and it has been found that these changes occur to a marked degree in the spring and fall months each year, and occasionally at other times as well. It would be a matter of great interest if these remarkable variations in the atmospheric absorption could be traced simultaneously at different latitudes and longitudes, and such a research might lead to results of far-reaching importance.

Attention is again called to the announcement in the report of the Aid Acting in Charge of a technical result of interest to the effect that prisms can now be easily constructed in which indices of refraction and wave lengths are, so far as can be seen, in as "constant" a relation as exists between deviation and wave lengths in the grating.

The accuracy of the Observatory work, it will be remembered, depends, strange as it may appear, on our increased knowledge of the optical properties of rock salt. This year it has been shown that, so far as the accuracy of the most painstaking measurements can go, these indicate that rock-salt prisms, whether mined at one part or another of the earth's surface, have identical refractive indices, and thus the measures of 1897–98, which determined the exact positions of 700 Fraunhofer lines in the infra red spectrum of rock salt, may be regarded as fixing "constants of nature."

One of the pieces of work accomplished in this last year, however, has been to go further and redetermine the dispersion of rock salt in

terms of wave lengths. The excellent facilities of the Observatory have enabled this to be done with such great accuracy as to give the wave lengths of the lines discovered to an average accuracy of 3 parts in 10,000.

This brings up the analysis of the infra red solar spectrum to a point closely corresponding with that reached by eye observations in the visible spectrum prior to the use of photography and the concave grating, and it is certainly calculated to excite a feeling of wonder to know that it is possible to thus automatically obtain in the dark, by means of the bolometer, results comparable in accuracy with those reached by incomparably more pains through the eye itself.

Personal investigation into the phenomena of the Welsbach mantle had interested me in their purely scientific aspect, but the records of my early prolonged investigation being unfortunately inaccessible, I have asked Mr. Abbot, Aid Acting in Charge, to repeat these experiments with the aid of the completer bolometric apparatus, more recently acquired by the Observatory. He has done so with marked success, and pending still further investigation attention is called to the interesting curves reproduced in his detailed report. An interesting thing in connection with these curves is that they show, as I have elsewhere remarked, that all artificial lights, even the best, are extravagantly wasteful of energy, in that they lavish it in the infra-red and not in the visible spectrum. Nature here as elsewhere does what we can not, for the glowworm and the firefly are still able to confine their exertions to the production of light with comparatively little heat, and to set us an example which would add millions to the nation's wealth if we could imitate it successfully on a commercial scale.

I am gratified to say that the first extensive publication of the Astrophysical Observatory is now in the hands of the Public Printer.

NECROLOGY.

JUSTIN SMITH MORRILL.

Senator Justin Smith Morrill was born in Strafford, Vermont, April 14, 1810, and died in Washington City December 28, 1898. He was appointed regent February 21, 1883, and reappointed March 23, 1885, December 15, 1891, and March 15, 1897.

The following most true as well as deeply felt tribute to the memory of Senator Morrill was given by Mr. Henderson at the last meeting of the Regents of the Institution:

I feel a personal loss in the death of Senator Morrill, whom I had known somewhat intimately from 1862, and with whom I had been associated, more or less, in public life when I was a young man.

Mr. Chancellor, the deceased statesman, Mr. Morrill, is now per-

fectly secure in his well-earned fame. It was said by one of the Latin poets that no man should be esteemed happy before his funeral. In this case the sad rites have been performed, and to the end of a most useful life he kept the faith.

In his career we have an illustration of the beauty and excellency of our republican institutions. His education was limited, but his honesty and patriotism had no bounds. He did what conscience dictated to be done and put his trust in those he served. He loved not license, but liberty as defined by Cicero—"the power to do what the law permits." He was true to his constituents, and in return they gave him those priceless gifts of freemen, their gratitude and fidelity.

He represented a State small in population and wealth, but rich in the character of its people and rich in the long line of able, pure, and distinguished statesmen she has given to the National Councils. The horizon of his usefulness, like that of Collamer, Foote, and Edmunds, extended beyond the State of Vermont. He was broad as the Union itself.

From 1855 to 1867 he was a member of the House of Representatives. In 1861 private industries had become prostrated and bankruptcy threatened the National Treasury. Secession had already commenced the work of dissolution, when he prepared and pressed to enactment the tariff laws of that year. Waiving the question of protection, the necessity for revenue alone demanded its passage, and the beneficent results gave national reputation to its author. He came to the Senate in 1867, and continued a member of that body until the date of his death, having received six successive elections by the legislature of his State. In the Senate he stood at all times for a sound currency. He had deplored the original issue of United States notes in 1862, and, true to his convictions of right, in after years he consistently demanded the performance of the nation's pledge that they be redeemed and canceled. He believed with all his heart that the gold dollar should measure values throughout the commercial world, and, unmoved by the clamors of hard times, he persevered in his faith until the fulfillment of his prophecies has broken the stubborn unbelief of millions.

Through his efforts the statues of distinguished Americans now adorn the old Representative hall of the Capitol, where his own so well deserves to be placed.

Largely through his exertions came the building constructed for the State, War, and Navy Departments; and the last legislative act of his life was to provide a building for the use of the Supreme Court of the United States. To us who knew him so well it brings profound pleasure that he lived to see the Library building completed, which not only in its purposes but in the splendor of its architecture does honor to the nation. Somewhere within its walls a modest tablet should at least connect his name with this magnificent structure. It is no less a monument to his memory than is St. Paul's Cathedral to the memory of Sir Christopher Wren.

For the last eighteen years he was a regent of this Institution, ever watchful of its interests and prompt to increase its usefulness. In the grandeur of his country he felt the patriot's pride. He sought to make its capital city worthy of the people to whom it belonged; and the Smithsonian Institution was regarded by him as a chief factor in its future greatness and renown.

My acquaintance with Mr. Morrill began in 1862. In my earlier days I enjoyed his counsel and instruction in public affairs. He was, in my judgment, the true American nobleman. Here, as elsewhere, distinction imposes increased obligations—*noblesse oblige*. No books of heraldry and no blazoned emblems are necessary to evidence the rank of Senator Morrill. His patent of nobility is recorded in the hearts of a grateful people.

Little can be added to Senator Henderson's words, but the Secretary asks to be permitted to record here his own sense of the loss of a dear and honored friend.

Respectfully submitted.

S. P. LANGLEY,
Secretary of the Smithsonian Institution.

APPENDIX TO THE SECRETARY'S REPORT.

APPENDIX I.

THE NATIONAL MUSEUM.

SIR: I have the honor to submit the following condensed report upon the present condition of the National Museum and the progress made during the year ending June 30, 1899:

In addition to the building, heating plant, and other equipment, the fixed property of the Museum consists of collections, books, office furniture, and cases and other receptacles for exhibiting and storing specimens.

The collections comprise nearly 4,400,000 specimens, including objects in all branches of anthropology, biology, and geology.

Since July 1, 1898, the collections have been increased by the acquisition of about 211,000 specimens.

The cases used for exhibiting specimens number about 2,250, and those used for storage purposes about 1,500.

The office furniture, including tables, desks, chairs, file cases, typewriters, bookcases, and minor articles, comprises about 900 pieces. There are also some 500 chairs, formerly used in the lecture room.

New furniture and fixtures have been acquired to the value of nearly $35,000, more than half of this amount having been expended for cases for the new galleries.

The library contains about 12,000 volumes and 8,500 pamphlets, not including the library of the late Dr. G. Brown Goode, which comprises about 2,900 volumes, 18,000 pamphlets, and 1,800 portraits, engravings, etc., and which was purchased during the past year by special authorization of Congress. During the year covered by this report 640 books, 965 pamphlets, and 5,196 parts of periodicals were catalogued.

Additional galleries have been erected, and steps have been taken toward the construction of skylights, for which special provision was made by Congress. The appropriation for these purposes was $10,000. Two of the old wooden floors in the Museum building have been replaced by terrazzo pavements.

There have been no losses of property during the year.

The boilers connected with the heating plant in the Museum building have been in use for nearly twenty years and are now practically worn out. They should be replaced.

The Museum staff.—Dr. O. C. Marsh, professor of paleontology at Yale University, and for many years connected with the staff of the National Museum in the capacity of Honorary Curator of vertebrate fossils, died March 18, 1899.

Mr. E. A. Schwarz, who formerly had charge of the coleopterous larvæ, has now been placed in charge of the entire collection of coleoptera, and Mr. Nathan Banks has been made Custodian of the collection of arachnida.

Mr. O. F. Cook, who has been connected with the Museum in the capacity of Assistant Curator in the Division of Plants, has been on leave of absence for several months. Having recently accepted a position in the Department of Agriculture, he has now been made Honorary Assistant Curator in charge of the cryptogamic collections.

Dr. George H. Girty has been appointed as Custodian of the carboniferous collections in the Section of Invertebrate Fossils.

Mr. George C. Maynard, Custodian of the Electrical Collections, has also been designated Aid in the Division of Technology.

Dr. A. C. Peale was appointed Aid in the Section of Paleobotany on July 25, 1898.

Explorations.—The explorations which have been carried on during the year have been conducted for the most part by curators of the Museum and other members of its staff. So far very little has been done toward making systematic collections in the territory recently brought under the control of the United States. (Congress was asked for a specific appropriation which would enable the Institution to carry on work in this direction, but no funds were provided.)

The anthropological explorations for the year have been fruitful of results. In September, 1898, Mr. W. H. Holmes visited California in the interest of the Museum and secured valuable collections illustrating both the ethnology and archæology of that State. From the auriferous gravel region of Nevada, and from Calaveras and Tuolumne counties, California, many stone implements, supposed to have a bearing upon the occupation of those areas by Tertiary man, were secured. The ancient soapstone quarries on Santa Catalina Island were visited, and explorations were made in two prehistoric burial places. From the latter a number of relics were obtained. The most extensive collections made by Mr. Holmes represent the basketry, implements, etc., of the Pomo, Digger, and Tulare Indians.

In April, 1899, Mr. Holmes spent a month in Mexico, securing important collections from the ancient Aztec obsidian mines of the State of Hidalgo, besides various relics from the ancient cities of San Juan, Teotihuacan, and Xochicalco.

In September, 1898, Mr. Paul Beckwith, representing the Division of History, was sent to Cuba and Puerto Rico for the purpose of collecting for the Museum, especial attention being given to gathering relics illustrating the war with Spain. Many valuable objects were secured, and agencies were set at work, calculated to add greatly to this interesting series of exhibits.

Mr. J. B. Hatcher, who has been carrying on extensive explorations in Patagonia for Princeton University, has forwarded to the National Museum one valuable lot of ethnological specimens, and important additions are expected at an early date.

Early in May, 1899, Dr. Walter Hough was detailed to carry on ethno-botanical researches in Mexico, in connection with the explorations of the Division of Botany, in charge of Dr. J. N. Rose.

Among the explorations yielding important results to the Department of Biology, those conducted by the naturalists of the U. S. Fish Commission and by Mr. A. B. Baker, in Puerto Rico and vicinity, are worthy of particular mention. Of mollusks the Museum received some 5,000 specimens, representing about 400 species, many of them rare or undescribed. Large series of other marine and fresh-water invertebrates, about 180 birds, 200 specimens of reptiles and batrachians, and 200 bats were collected. A large and important series of mammals from Sweden, Germany, Switzerland, and Belgium was obtained for, and under the direction of, the Museum by Mr. J. A. Loring, of Owego, New York. Mr. Dall de Weese visited Alaska during the summer of 1898 and special arrangements were made with him for procuring large mammals for the Museum. He secured several specimens of the Alaskan moose, and a large number of the wild white sheep of that Territory. Dr. E. A. Mearns, U. S. A., made collections of birds' skins in Texas.

The Division of Marine Invertebrates has been enriched by material collected by Dr. T. H. Bean and Mr. B. A. Bean on Long Island; by Miss Mary J. Rathbun, at Grand Manan, New Brunswick; by Messrs. William Palmer and Paul Bartsch, in the Dismal Swamp, Virginia; by Mr. Harold Heath in Monterey Bay, California, and by the Biological Laboratory, Cold Spring Harbor, New York. As the result of explorations in the North Pacific, Alaska, Kamchatka, Lake Superior, and Florida, collections of fishes were transferred to the Museum by the Fish Commission.

Dr. J. N. Rose was engaged at the close of the fiscal year in explorations in Mexico, where, it is believed, he will secure much valuable botanical material. He is accompanied, as already stated, by Dr. Walter Hough, who is charged with the collection of ethno-botanical specimens. Important botanical collections have been made by Messrs. W. T. Swingle and D. G. Fairchild in Europe. Explorations conducted by members of the Biological Survey, Department of Agriculture, have resulted in the substantial enrichment of the herbarium. A series of Mexican plants, collected by Mr. E. W. Nelson, was purchased by the Museum.

The fossil plants from the lower coal measures of Henry County, Missouri, and the carboniferous fossils from Indian Territory, referred to elsewhere, were collected by Messrs. W. P. Jenney, Gilbert Van Ingen, S. A. Miller, and Dr. J. H. Britts, under the auspices of the U. S. Geological Survey. The explorations of Mr. H. E. Dickhaut, of the Survey, in the vicinity of Lockport, New York, and of Mr. Paul Bartsch, of the Museum staff, in Iowa, yielded important results. The former collected a valuable series of Medina and Niagara fossils, and the latter, Hamilton corals and Kinderhook fossils. Geological material obtained by Mr. F. W. Crosby in Europe, and by Mr. Edward Palmer, in Mexico, has been added to the collections.

Accessions.—There was an increase of 55 in the number of accessions for the year, the total being 1,497. Owing to the limitations of space, it is impossible to do more than refer briefly in this place to some of the most important of them, under the heads of anthropology, biology, and geology. The material obtained by special expeditions has already been referred to.

Anthropology.—The collection of ethnological specimens from the Indian tribes of the Great Plains and the Rocky Mountains, purchased from Mr. E. Granier, of Paris, France, contains about 1,000 specimens, many of which are of exceptional value, having been collected a number of years ago. They comprise articles of costume, implements of many kinds, ceremonial objects, etc. The specimens of beadwork and quill embroidery are especially notable.

A collection of great value illustrating Mexican ethnology and archæology was obtained from Mr. E. W. Nelson. The number of specimens is not large, but they have especial value to the Museum as coming from districts rarely visited by its representatives.

From Dr. E. Palmer some additional Mexican material has been received, a feature of especial interest being a native still of most primitive construction.

A second collection of stone implements and miscellaneous relics was purchased from Dr. Roland Steiner during the month of June, but the specimens have not yet been catalogued. The same may be said of a very important collection of Indian basketry obtained from Dr. W. J. Hudson, of Ukiah, California.

Among the more important collections of electrical objects received during the year is one deposited by Miss Sarah J. Farmer, of Eliot, Maine. It consists of various pieces of apparatus devised and used by the late Moses G. Farmer in his experimental work in the various branches of electrical science.

Other collections of interest, illustrating this branch, may be briefly mentioned: Original fire alarm telegraph apparatus, deposited by the fire department of Boston, Massachusetts, through correspondence with Col. H. S. Russell, fire commissioner; a collection of insulated and uninsulated conductors used for the transmission of currents of electricity for lighting and power purposes, besides some telegraph and telephone cables and trolley wires, lent by John A. Roebling's Sons Company, of Trenton, New Jersey; a pocket telegraph instrument made by J. D. Caton, Ottawa, Illinois, and a Morse telegraph relay and sounder made of silver by S. W. Chubbuck, Utica, New York, and deposited in the Museum by D. Wilmot Smith, of Breckinridge, Minnesota.

Biology.—Mr. W. B. Moss, of Ashton-under-Lyne, England, presented a collection of about 3,000 specimens of small shells, mostly marine gastropods, collected by the

Rev. James Hadfield and Mrs. Hadfield at Lifou Island, Loyalty Group, central Pacific Ocean. Most of these have but recently been described, and nearly all are new to the collection. A number of valuable river mussels from the Southern States, including a number of types of species described by Mr. B. H. Wright, of Penn Yan, New York, have been received from the latter.

Large and valuable collections of hemiptera, hymenoptera, siphonaptera, and mallophaga were presented by Prof. Carl F. Baker, of Auburn, Alabama. Mr. W. H. Ashmead has presented his private collection of insects, which is especially rich in type material and includes not less than 60,000 specimens. Prof. V. L. Kellogg, of the Leland Stanford Junior University, gave 60 microscopical preparations of mallophaga, mounted on glass slides.

Eighty species of brachyuran and anomuran crustaceans were received in exchange from the Museum of Natural History, Paris; and 47 species of decapod crustaceans were acquired from the Museum of Comparative Zoology, Cambridge, Massachusetts, in exchange.

Five skins of the California condor were obtained by purchase. Mr. Paul D. Bergen, of Chefoo, China, sent 48 skins of Chinese birds in exchange. From the Albany Museum, Grahamstown, South Africa, 29 bird skins were received, and Prof. M. F. Colunga, Lima, Peru, transmitted 24 Peruvian skins in exchange for other material. Skins from New Zealand, Colombia, and China were received, respectively, from Messrs. L. T. Ayson, Masterton, New Zealand; Mr. Outram Bangs, Boston, Massachusetts, and Mr. George D. Wilder, Pekin, China. One hundred and eighty-four bird skins from various localities in the United States were presented by Dr. E. A. Mearns, U. S. A. Dr. W. L. Ralph contributed 207 birds' eggs, and 127 eggs were received from Prof. C. F. Baker.

In the Division of Comparative Anatomy the most important accession was a small collection of mounted skeletons of cartilaginous fishes purchased for use in connection with the exhibit at the Trans-Mississippi Exposition, held in Omaha.

A series of fishes from northern and central Asia was received in exchange from the Museum of Natural History, Paris; a specimen of *Icosteus ænigmaticus* was presented by Mr. John Chapman, San Diego, California, and a specimen of *Rhamphocottus*, from the coast of Washington, by Mr. O. E. Shaffer, Port Townsend, Washington. Type specimens of several species of fishes were transmitted by the U. S. Fish Commission.

A series of 1,049 specimens of plants, collected in Florida in 1843–1849, was received from the British Museum (Natural History). Five hundred specimens of Samoan plants, 674 Colorado plants, and about 2,500 Mexican plants were obtained by purchase. Mr. J. G. Baker, London, England, presented a portion of his private herbarium. Seven hundred plants from the Gulf coast were purchased and 300 specimens from southern Florida were collected by Mr. C. L. Pollard.

A large number of reptiles and batrachians, collected in various parts of the world by field parties of the Fish Commission, were transferred to the Museum. This collection embraces much material valuable in connection with the study of geographical distribution and individual variation. One of the most interesting specimens was a discoglossoid toad, described by Dr. Stejneger under the name *Ascaphus truei*. An interesting collection of reptiles from Java, collected by Mr. D. G. Fairchild, was received from the Department of Agriculture.

A collection consisting of 80 specimens of European bats, and another consisting of 52 specimens of Norwegian mammals, were purchased. Baron E. de Selys-Longchamps, of Liege, Belgium, presented 24 European mammals. An Asiatic elephant, a lion, and numerous other mammals were received from the National Zoological Park.

Geology.—The largest accessions in the Division of Invertebrate Paleontology were those from the U. S. Geological Survey, comprising between six and seven thou-

sand specimens. Among the more valuable collections from this source is an extensive series of Cambrian brachiopods, determined by Mr. C. D. Walcott, and certain specimens of Lower Cretaceous fossils, figured in Bulletin No. 151 of the Survey. An important collection of fossils from the Cincinnati group, consisting of about 4,000 specimens and including many pelecypods, was obtained by purchase. Three valuable series of Post-Pliocene corals were received in exchange from the Geological Museum, Leyden; the University Museum of Natural History, Turin, and the British Museum of Natural History, respectively. Valuable specimens of Upper Carboniferous fossils from Texas were acquired by purchase. About 900 Carboniferous fossils, collected by Messrs. Taff and Richardson in Indian Territory, were acquired from the Geological Survey. These have been described by Mr. David White in the Nineteenth Annual Report of the Survey, which was in press at the close of the year. Mr. R. D. Lacoe has made substantial additions to the collections which bear his name, 30 boxes of Carboniferous plants having recently been received from him.

The Museum of Comparative Zoology, Cambridge, transmitted a small series of teeth of Paleozoic sharks. Mr. A. B. Baker collected some fossil fishes in Kansas. A collection of skulls of mammals of the White River Miocene formation, obtained by Mr. N. H. Darton, was received from the Geological Survey. Fossils of this formation had not previously been discovered in the locality from which these were obtained. A skull of a new species of bear (*Ursus procerus*) was purchased, and a fine skull of a species of *Hydracodon* was presented by Mr. A. W. Barber, Washington, District of Columbia.

Eighty-eight specimens from different localities were received from the Geological Survey by the Division of Minerals. This collection includes specimens of cesium, beryl, bixbyite (a new mineral), tysonite, a fine series of endlichite, specimens of minium, tourmaline, chrysoberyl, martite, and wolframite. A specimen of parisite from Missoula County, Montana, was presented by Mr. F. D. Smith, through the U. S. Geological Survey. Mr. George F. Kunz, of Tiffany & Co., New York City, presented a specimen of prosopite, a rare mineral. Six specimens of roscoelite on auriferous quartz from Eldorado, California, were presented by Mr. G. W. Kimble, through the U. S. Geological Survey. Five specimens of native arsenic (crystallized) and specimens of topaz and rhodochrosite from Japan were presented by K. Kato, of Tokyo. Two specimens of the new mineral, wellsite, and other material were transmitted by Mr. J. H. Pratt, of Chapelhill, North Carolina. Dr. L. T. Chamberlain presented to the Smithsonian Institution, for addition to the Lea collection, two cut sapphires from Yogo Gulch, Montana, and an opal; also 21 cut sapphires of assorted colors, 2 garnets (var. rhodolite), and 1 specimen of citrine quartz (cut). Four cut opals were purchased from Messrs. Tiffany & Co.

A series of rocks illustrating the Pre-Cambrian geology of the Lake Superior region was received from the Geological Survey. This material was obtained by Mr. C. R. Van Hise. Geological material from Italy and Sicily was acquired by purchase. Mr. Charles Burdett Hart, minister of the United States to Colombia, presented to the Smithsonian Institution specimens of ores from the Zancudo mines, near Medellin, Colombia. Specimens of jointed sandstones from the Black Hills of South Dakota, transmitted to the Geological Survey by Mr. N. H. Darton, Washington, District of Columbia, were transferred to the Museum. From Mr. James D. Husted, Kansas City, Kansas, 3 slabs of onyx marble were received. The late Prof. O. C. Marsh presented a series of 19 polished spheres of Japanese breccia.

Foreign exchanges.—Important exchanges were arranged with the following establishments and individuals: The British Museum, London, England; the Museum of Natural History, Paris, France; the Geological-Paleontological Institute, Munich, Germany; the Geological Survey of Canada, Ottawa; the Rijks Ethnographic Museum, Leyden, Holland; the Public Gardens and Plantations, Botanical Department, Kingston, Jamaica; the Museum Michoacana, Morelia, Mexico; the Albany

Museum, Grahamstown, South Africa; the Riksmuseum, Stockholm, Sweden; Mr. G. Van Roon, Rotterdam, Holland; Mr. Jean Miguel, Barrubio, Herault, France; Rev. Paul D. Bergen, Chefoo, China; Baron R. de Vriere, Zedelghem, Belgium; Mr. R. Ruscheweyh, Buenos Ayres, Argentine Republic; Mr. E. Y. Connell, St. Kitts and Nevis, British West Indies; Mr. C. F. Pavona, Museum of Natural History, University of Turin; Prof. M. F. Colunga, Lima, Peru; Mr. L. Y. Ayson, Masterton, Wellington, New Zealand.

Distribution of specimens.—Nearly 25,000 specimens were distributed during the year. A portion of these were sent in exchange for other material and the remainder were distributed to educational establishments as gifts. In addition nearly 10,000 specimens were lent for study. The gifts consisted mainly of collections of marine invertebrates, rocks, and ores, and casts of prehistoric stone implements.

Specimens received for determination.—As in past years, a very great deal of time has been devoted to the identification of material transmitted by correspondents for examination. Five hundred and eighteen lots of material have been reported upon by the various curators during the year just ended. A small number of specimens, desirable for addition to the Museum collections, have been retained, with the consent of the senders, in exchange for the work of identification.

Visitors.—During the year 192,471 visitors were registered in the Museum building and 116,912 in the Smithsonian building, making a total of 309,383, showing an increase over the record of the preceding year of about 33,000.

Publications.—The Annual Report of the National Museum for 1896 has been issued and the papers in the appendix have also been published in separate form. Volume 20 of the Proceedings was issued in August, 1898. Proceedings papers 1140 to 1178, inclusive, constituting volume 21, have been distributed since the beginning of the last fiscal year. The editions of parts 2 and 3 of Bulletin 47, entitled "The Fishes of North and Middle America," by Drs. Jordan and Evermann, were received from the Government Printing Office late in the fall of 1898, and have also been distributed. The complete work will include an atlas of plates.

Expositions.—The Trans-Mississippi and International Exposition, which opened at Omaha on June 1, 1898, continued for five months. The Report of the National Museum for 1898 contains a brief reference to the various series of exhibits prepared by the Museum. A more formal and extended account will probably appear elsewhere. Congress has made provision for participation by the Museum in expositions to be held at Buffalo in 1901 and at Toledo in 1902 or 1903.

Respectfully submitted.

F. W. TRUE, *Executive Curator.*

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

AUGUST 1, 1899.

APPENDIX II.

REPORT OF THE DIRECTOR OF THE BUREAU OF AMERICAN ETHNOLOGY FOR THE YEAR ENDING JUNE 30, 1899.

SIR: I have the honor to ask attention to the following report of operations of the Bureau of American Ethnology for the year ending June 30, 1899. The operations have been conducted in accordance with an act of Congress making provision "for continuing researches relating to the American Indians, under the direction of the Smithsonian Institution," approved July 1, 1898.

The work has been carried forward in accordance with a plan of operations submitted on June 18, 1898, and duly approved by the Secretary.

Field operations have been conducted in Arizona, California, Indian Territory, Maine, Nebraska, New Brunswick, New Mexico, New York, Oklahoma, and Ontario, while researches have been made by special agents in Alaska and Patagonia. The office work has included the collection and preparation of material from Indian tribes in Arizona, California, Colorado, Florida, Idaho, Indian Territory, Iowa, Nebraska, New Brunswick, New York, Oklahoma, Ontario, and in less quantity from the several States and Territories, as well as from neighboring countries.

As heretofore, the work has been conducted in accordance with a classification of ethnic science based largely on the special researches of the last two decades and developed largely in this Bureau. This classification has been set forth at length in previous reports and need not be repeated.

FIELD RESEARCH AND EXPLORATION.

Early in the fiscal year the Director resumed the study of shell mounds and earthworks in Maine, and continued the comparison of aboriginal handiwork contained in these accumulations with the handicraft of the partially accultured aborigines still living in the adjacent forests and among the less frequented inlets and islands of the coast. Some of the results were put in the form of a preliminary paper on "Technology, or the Science of Industries," designed for further elaboration and incorporation in the formal reports.

Under a special authorization from the Secretary, Mr. W J McGee and Mr. W. H. Holmes, of the U. S. National Museum, made an extended ethnologic and archeologic reconnoissance in California during October, November, and December. The districts examined comprised the western slopes and foothills of the Sierra Nevada, including the Table Mountain region from Yuba River southward to Tule River; a portion of the northern Coast Range region, centering about Ukiah; typical portions of the Sacramento Valley, centering about Stockton, and the coastwise areas and offshore islands of the southwestern region of the State. The primary purpose was the collection of typical artifacts representing the aboriginal culture of the peculiarly interesting Pacific coast province; a secondary purpose was the collection of prehistoric relics, the comparison of these with the early historical period, and the general study of the culture history of the region; and a satisfactory degree of progress was made in the attainment of both purposes. The operations resulted in substantial enrichment of the Museum through the acquisition of new and representative material; indirectly the opportunities for local work led to the acquisition

of a highly useful collection of basketry—the Hudson collection,—which throws much light on the aboriginal handicraft and motives of the California Indians.

In November Dr. J. Walter Fewkes repaired to Arizona for the purpose of continuing researches concerning the winter ceremonies of the Hopi Indians, but soon after his arrival an epidemic of smallpox manifested itself in such severity as completely to demoralize the Indians and prevent them from carrying out their ceremonial plans, and at the same time to place Dr. Fewkes in grave personal danger. It accordingly became necessary to abandon the work for the season.

Early in the fiscal year an arrangement was effected with the managers of the Trans-Mississippi and International Exposition, at Omaha, under which Mr. James Mooney cooperated, for the installation and conduct of an Indian congress. In carrying out the plan, Mr. Mooney visited Indian Territory and Oklahoma, and successfully enlisted the sympathy and aid of representatives of various tribes, including the Kiowa, with whom he was intimately acquainted. Portions of the aboriginal material obtained in the field for the use of the congress were subsequently acquired for the National Museum.

In August Dr. Albert S. Gatschet revisited New Brunswick for the purpose of continuing the collection and analysis of Algonquian linguistic material. He sought new aboriginal informants and was able to make satisfactory additions to the recorded dialects of the measurably distinct portion of the great Algonquian stock occupying the northern Atlantic coast.

In September Mr. J. N. B. Hewitt proceeded to various localities in New York and Ontario for the purpose of obtaining additional material pertaining to both the languages and the myths of the Iroquoian Indians, and the work, coupled with efforts to obtain certain unique objects for the National Museum, occupied him in the field until January.

During the autum Mr. J. B. Hatcher, who had previously brought from Patagonia certain valuable ethnologic material for the Museum, returned to the field and resumed collecting and the making of photographs illustrating the habits and habitations of the Tehuelche tribe and the natives of Tierra del Fuego. His work was not completed at the end of the year.

Dr. Willis E. Everett, acting as a special agent of the Bureau, visited various remote districts in Alaska and contiguous British territory during the year, and obtained a quantity of linguistic data of considerable use in classifying the aboriginess of a little-known district.

Office Research.

WORK IN ESTHETOLOGY.

Throughout much of the year the Director continued giving attention to the synthesis of data in the Bureau archives and in published form, with the view of organizing anthropic science, including ethnology in its several aspects. Among the subjects considered in detail was that of the more spontaneous human activities, normally pleasurable in character, which form the object-matter of esthetology. The researches among the aborigines have thrown much light on this subject, since the symbolic devices, sports, games, and ceremonies of the tribesmen are relatively simple and little differentiated, and hence are readily perceived and synthesized—indeed the synthesis of the esthetic and other activities rests primarily on the observations among the American natives, corroborated by critical observations on other primitive peoples, and finally attested by the facts manifested among advanced peoples. It is convenient to denote the primary activities comprised in the domain of esthetology as pleasures, since they are largely physiologic in character, though, like other activities, chiefly demotic (or collective) in their manifestations; and the activities may be classed as ambrosial pleasures, decoration, athletic pleasures or

sports, games, and fine arts. The definitions and the classification of esthetology have been formulated and printed in such manner as to facilitate examination and further discussion on the part of the collaborators of the Bureau and other students, with a view to final revision and incorporation in a future report, embracing the more systematic results of the researches.

In continuing his researches concerning the collections made in the Florida muck-beds, Mr. Frank Hamilton Cushing has been led to comparative study of a wide range of those products of primitive handicraft expressing symbolic ideas in form, function, and decoration; and certain of his generalizations are of much importance in that they afford a satisfactory basis for the classification and interpretation of many of the protean artifacts of primitive origin. His researches indicate that the primitive implement-maker is actuated by a few dominant ideas, influenced largely by habit, and measurably controlled by simple associations; so that the products of his handiwork, when arranged by function and motive, may readily be grouped in a limited number of categories, which are, at the same time, convenient and significant. The type of ideative association is exemplified by the tomahawk-calumet, which is at once a war weapon and an appurtenance of peace, and hence serves as a symbolic expression of willingness for war and readiness for peace, at the option of the other party; the war concept is emphasized by decorative motives, usually derived from strong and swift animals, while the peace concept is strengthened by emblems in the form of feathers of small birds or other decorative symbols derived from gentle animals; and the antithetic symbolism serves to keep alive the opposing sentiments of amity and enmity in the primitive mind. In this and other cases, the recognition of motive on the part of the maker enables the student to reduce the chaos of protean forms of primitive artifacts to definite order. Although his work has been somewhat retarded by ill health, Mr. Cushing's progress in researches has been satisfactory, and some of his more important results are ready for publication.

When compelled to abandon fieldwork, for reasons already noted, Dr. J. Walter Fewkes turned attention to the collections made during earlier seasons, and began the preparation of a memoir on the decorative symbolism of Pueblo pottery. This memoir was nearly ready for publication at the close of the fiscal year; it embraces various new interpretations of importance, the account of which is reserved for a future report.

WORK IN TECHNOLOGY.

As before noted, the Director made observations on the aboriginal technology revealed in the contents of shell mounds and tumuli of Maine during the earlier part of the fiscal year; and these observations, with other data, were subsequently utilized in defining the science. The technical activities are intimately interrelated, and combine to form a complex group, which is commonly assumed to be irresolvable with scientific precision, but the relations of the activities are so well displayed in primitive culture like that of the American aborigines as to suggest a convenient arrangement for the use of investigators, and such an arrangement has been formulated and placed within reach of the collaborators and others for subjection to the test of actual use. In this arrangement, industries are classified as (1) simple production or substantiation, (2) construction, (3) mechanics, (4) commerce, and (5) the preservation, reconstruction, and improvement of the human body by a series of processes conveniently connoted by the term medicine. Provision has been made for completing and adding details to the outline already prepared, in a form suitable for publication in a future report.

Mr. Cushing's researches have served to illumine those early stages in the growth of industries in which utility was but vaguely perceived, and in which processes were largely ceremonial or symbolic, as when the hunter sought success by imitating the attitude and actions, or by arming himself with the beak or claws of a raptorial

tutelary. The researches conducted in the Bureau have already rendered it clear that decoration, and indeed the greater portion of the fine arts, arises in symbolism and develops through conventionism; and the researches of the year suggest a related genesis for industries. The results of the work are in preparation for full publication.

While among the surviving aborigines of California, Mr. W J McGee was enabled to make observations corroborating and extending generalizations already framed with respect to those of the primitive industries involving the use of stone as material for implements. The several tribes studied may conveniently be classed as Acorn Indians, since acorns form their principal source of food, and since their characteristic industries are conditioned by this food supply. Some of the processes and implements vary from tribe to tribe; e. g., in some tribes the acorns are cracked in the teeth in order that the meats may be extracted, in others they are cracked with spheroidal hammer-stones, and in still others an elongated pestle-like stone, grasped by one hand and used in the fashion of a club or civilized hammer, is employed for the same purpose. Other devices, such as those used for grinding the acorn meats, are substantially alike from tribe to tribe; though it is noteworthy that in each tribe there is a diversity growing out of the age of the apparatus, or the degree of development by use. Thus it is found that the nether millstone, which may be either a ledge or other mass in place of a portable bowlder, is, in the early stages of use, a flat or slightly concave metate, which after more extended use becomes a deeply concave metate, still later a shallow mortar, and at length a deep mortar which may eventually be worn through, if the original mass is not more than 9 to 15 inches in thickness; while the grinding-stone concordantly changes from a simple roller or crusher to a mano (or muller), and finally to a pestle, at first broad and short, but afterwards long and slender. It follows that in this region the northerly device of the mortar and the southerly device of the metate overlap; yet it is much more significant that the overlapping is essentially genetic, and only incidentally geographic. Not infrequently the genesis of an individual mill corresponds with the rise and passing of a family; the young woman may begin life with a bowlder, flat on one side, and a few river-worn cobbles as a mill, which is then used as a metate; gradually the mill develops into a mortar, with a well-rounded and polished pestle, shaped chiefly by wear, perhaps supplemented by slight dressing, on which she grinds vigorously in her old age for the support of her daughters and their husbands, and the growing grandchildren; and on her death apparently the pestle is broken and the bottom knocked out of the mortar. Neglecting the final act, the individual growth of the primitive mill well epitomizes the phylogeny of its species, and demonstrates that in general the mortar must be regarded as the differentiated and eventually degraded offspring of a metate-like prototype, whence sprang also the metate along one line and the quirn and its derivatives along another. It is particularly significant, too, that the milling apparatus still used by the Californian natives consists initially of naturally-formed ledges or bowlders, with stream-worn cobbles for grinders, and that both bowlder and cobble are, for the most part, shaped gradually by wear, without definite recognition of the shaping on the part of the operator—i. e., that the mills represent protolithic culture, rather than the technolithic art characterized by designs and models.

The plan for the Indian Congress at Omaha (mentioned in a preceding paragraph) was formulated chiefly by Mr. James Mooney, in connection with Hon. Edward Rosewater, president of the board of publicity and promotion of the exposition, though conditions connected with administrative control and policing of the Indians assembled on the grounds led to the assignment of a representative of the Indian Bureau, Capt. W. A. Mercer, as officer in charge of the congress; but Mr. Mooney cooperated in the installation and remained on the ground throughout the exposition. In accordance with the plans of Messrs. Mooney and Rosewater, the Indians were domiciled, so far as practicable, in houses or lodges of their own construction, and of

more or less strictly aboriginal type; accordingly the installation afforded an excellent opportunity for the study of native house-building, and of the ceremonies connected with the highly interesting house cult of the native tribes. Among the lodges were two Blackfeet skin tents, made and decorated in aboriginal fashion in every respect, save that cow hides were substituted for buffalo hides. A lodge of special nature was a Wichita grass house, which faithfully exemplified the aboriginal construction, since the structure was an actual example, the oldest in the Wichita village in Indian Territory, repaired, taken down, and reconstructed by aged men and women conversant with the house cult of the tribe. At the close of the exposition this specimen was obtained for the National Museum. These and other structures erected at Omaha and carefully studied by Mr. Mooney have added materially to the knowledge of aboriginal houses.

The researches in California by Messrs. McGee and Holmes extended to basketry and added materially to knowledge of the processes of basket making, especially among the Pomo and Yokai tribes. The Hudson basketry collection comprises examples of twelve different weaves, which have been carefully studied by the collector, Dr. J. W. Hudson, and are described fully in his catalogue; and, in addition, several processes were critically studied in actual use by basket makers. The functions or purposes of the baskets also received careful attention. In this region they form the common utensils of the householders, taking the place sometimes filled by fictile ware and serving various other purposes. They are used as cups, canteens, and other water vessels, as pots for boiling acorn meal and meats (by means of heated stones), as receptacles for stored foods and liquids, and especially as ceremonial and sacramental objects. The researches concerning the aboriginal basketry of California promise [illegible] along different lines as the work proceeds.

WORK IN SOCIOLOGY.

[illegible] extended into the domain of institutions [illegible] in a preliminary [illegible] than the industrial [illegible] are exceedingly [illegible] and the [illegible]

[illegible] Mr. [illegible] obtained [illegible]

social organization, or at least as a tangible and definite expression of consanguineal relation. A third factor in the organization of the Californian aborigines grows out of their industrial status. Since their chief food source is the acorn, and since the oak trees never grow in continuous forests, but are somewhat sparsely distributed among other trees or over the openings of the valleys, the native population was necessarily sparse and scattered, and each tribe tended to remain permanently attached to a definite range; and this sparse distribution permitted and promoted the retention of tribal dialects corresponding to each range. A fourth factor appears in ceremonial observances, apparently growing out of the industrial condition, notably the affine taboo which prohibits communication between sons-in-law and mothers-in-law, and among some of the tribes between daughters-in-law and fathers-in-law and other connections by marriage. The linguistic and industrial ceremonial factors all operate as repulsive forces tending to prevent aggregation of population and intercommunication of tribes, and hence to retard cultural development; and it would appear that the several factors, interacting with cumulative effect, have combined to produce the singular concentration of linguistic stocks in the Pacific coast region. The researches concerning this subject are not yet complete.

During the earlier part of the fiscal year Mr. Mooney continued researches relating to the Kiowa Indians, and noted as a conspicuous characteristic of the tribe the apparent absence of a clan or gentile system; for, despite his intimate acquaintance with and adoption into the tribe, he has never been able to discover unmistakable traces of this commonly prominent feature of primitive social organization. This peculiar characteristic has received attention by the Director and several of the collaborators, and an apparently satisfactory explanation has been discovered: On reviewing the tribal customs it became evident that the widely roving Kiowa enjoyed contact with other tribes, and consequent acculturation in an exceptional if not unique degree. Sometimes the association was amicable, when ideas and devices were freely interchanged; not infrequently the contact was inimical, when the Kiowa were commonly enriched by the acquisition, not only of plunder, but of captives who were subsequently adopted into the tribe; and the general effect of the wide association was to extend the intellectual range and differentiate the blood of the Kiowa. Especially important was the habitual adoption of captives, the effect of which is always to introduce arbitrary relationships tending to break down the natural kinship system; yet hardly less important were the oft-recurring excursions for hunting and plunder, since they involved more or less arbitrary extensions of the consanguineal organization, somewhat analogous to those attending the development of patriarchy among regularly nomadic peoples. Collectively, the consequences of the roving and predatory habits of the Kiowa must have been to subordinate, in exceptional if not unique degree, the prevailing kinship organization characteristic of primitive society and to gloss or even to replace it with the more strictly artificial or demotic system corresponding to that of higher culture. The results of these researches concerning the distinctive organization of the Kiowa will be incorporated in Mr. Mooney's memoir on the heraldic system of the tribe.

WORK IN PHILOLOGY.

Toward the end of the fiscal year the Director made some progress in systemizing the rich linguistic collections in the archives of the Bureau, with a view to formulating plans for further research concerning the aboriginal tongues of America; but the results are not yet ready for announcement.

Mr. J. N. B. Hewitt continued the collection of Iroquoian material, both linguistic and mythologic, and has made satisfactory progress in preparing material for publication. His studies illustrate the importance of combining inquiries concerning primitive myths with linguistic inquiries. Thus certain puzzling inflections introduced in

various terms eluded the best efforts toward analysis throughout the earlier portion of the year; but, on studying the creation myths with the aid of native informants in the course of his field operations, he ascertained that these obscure inflections connote a characteristically primitive notion concerning individual activity or power; e. g., the shaman is supposed to work magic by the sound of his rattle or drum, and the witch to work her evil charms by the action of singing, both acquiring their mystical powers only by and through the supposedly mystical exercise of function in producing the sound, and it is the purpose of some of the obscure linguistic inflections to denote the mystical states recognized in the mythology. It is well known that the aboriginal languages possess inflections for normal states, such as sitting, standing, reclining, moving, etc., but the recent researches show that there are inflections also for mystical states, and that some of these quite significantly correspond with the inflections for singing or dancing. A preliminary announcement of results has been made, and formal publication will follow so soon as the inquiry can be considered complete.

Dr. Albert S. Gatschet continued the preparation of the comparative vocabulary of the Algonquian stock, and at the same time, according to custom, compiled linguistic material for use in reply to numerous inquiries from correspondents for aboriginal terms to be applied to parks, vessels, villages, etc., and for the meaning or etymology of aboriginal terms already in use. The field operations of the year materially enriched the comparative vocabulary, which has already attained such volume and completeness as to yield standards for classifying the tribes comprised in the extensive stock to which it pertains.

Working under a small allotment, Dr. Franz Boas has continued the preparation of linguistic material collected among the tribes of northwestern United States and contiguous Canadian territory. The principal contributions of the year comprise a complete Tsimshian vocabulary and a considerable collection of texts. The texts are in form for publication, and will be transmitted with the next report.

During the year the Bureau was so fortunate as to obtain, through the courteous offices of Dr. Edward Everett Hale, the vocabulary of the Massachusetts (Natick) language laboriously prepared by the late J. Hammond Trumbull, and good progress has been made in arranging the material for publication.

WORK IN SOPHIOLOGY.

Throughout the history of the Bureau, it has been the policy to organize the lines of research in such manner as to permit comparative study of well-defined categories of acfivities and activital products. The maintenance of this policy has been particularly difficult in connection with the science of opinions, or sophiology, since the object matter of the science is more elusive and complex than that of any other branch of knowledge; yet fair progress has been made in the introduction of the comparative method in even this branch of inquiry. During the year Mr. J. N. B. Hewitt has made an important comparative study of the creation myths of several Iroquoian tribes and of two or three Algonquian tribes. The results, which are of much interest, are practically ready for publication. The comparative method has been used with success also by Dr. J. Walter Fewkes in the interpretation of the symbolism depicted on the pottery of the Hopi and other Pueblo people, while the results attained by Mr. Cushing in his technologic researches were made tangible only by constant use of the comparative method in seeking the mystical motives of the primitive artisans. Progress has been made by the director in formulating the method for the guidance of future inquiries.

Although retarded by ill-health, Mrs. M. C. Stevenson made substantial progress in her analysis and discussion of Zuñi mythology during the year, and the portions of her memoir already completed have been withheld from publication pending the revision made necessary by further researches concerning certain of the ceremonies.

Toward the close of the fiscal year Mr. McGee undertook an inquiry concerning certain mystical symbols, such as that known as the swastika, so common among the decorative devices of the American aborigines, and these graphic devices were compared with the mystical number systems involved in the primitive cult of the quarters. The investigation served to indicate that neither finger counting nor quinary and decimal number systems are primitive, but are products of binary and quarternary systems, modified through magnification of the ego in the manner described in previous reports. The inquiry also afforded useful results bearing on the development of right-handedness and on the orientation instinct which survives even in the highest culture stages. A preliminary announcement is made, but the principal results are reserved for incorporation in a report dealing with the time concept of the Papago tribe.

Toward the close of the year Dr. Cyrus Thomas was also led to a comparison of the number systems of the northern tribes with those revealed in the codices and other aboriginal records of Mexico, and began the preparation of a memoir on the subject, designed for incorporation in the next report.

After his return from Omaha, at the close of the Exposition, Mr. Mooney began arranging for publication his extensive collections of Cherokee myths, and by the end of the year he had the greater part of his voluminous data arranged in form for publication, and was engaged in search for parallels in the records comprised in the archives of the Bureau, as well as in the published literature. His memoir will be incorporated in the next report submitted.

DESCRIPTIVE ETHNOLOGY.

Mr. F. W. Hodge continued supervision of the material for the Cyclopedia of Indian Tribes and made such additions to the work as his duties in other directions permitted. Dr. Cyrus Thomas spent the greater part of the year in reviewing and extending the portion of the work relating to the tribes of the Siouan stock. His progress in examining the extensive literature involved and preparing the material for publication was satisfactory. During a portion of the year Col. F. F. Hilder, ethnologic translator, was occupied in translating archaic Spanish records of especial value in connection with the Cyclopedia. One of these is a manuscript written in 1782, describing the tribes of Texas with unequaled fulness. The manuscript is anonymous, but Colonel Hilder has succeeded in identifying the author as Padre Morfi.

COLLECTIONS.

Among the special collections made during the year were those of Messrs. McGee and Holmes in California, comprising stone artifacts in considerable number and variety, basketry, etc., the collections being of special value in that they represent typical prehistoric workmanship and typical modern workmanship combined, and in that they were made on the ground by experts in archeologic and ethnologic research. Another collection of special interest, though of somewhat limited extent, was made in southern Patagonia and Tierra del Fuego by Mr. J. B. Hatcher; a portion of the material was received during the year. A number of typical collections made by correspondents of the Bureau and others were also acquired during the year. One of these includes the Wichita house and house furniture obtained by Mr. Mooney, as indicated elsewhere; another is the suit and regalia of Kahkenaquonaby (afterwards called Dr. Peter Edward Jones), a member of the Messissauga tribe of the Ojibwa; a third is a small but rare and significant lot, including a beautiful example of the stone yoke, or ceremonial collar, obtained from Mexico through the agency of Mr. Holmes.

PROPERTY.

The property of the Bureau was classified and described in some detail in a previous report. During the past year a number of manuscripts have been added to the archives, chiefly by contribution from correspondents, and others have been produced. The collection of photographs of Indian subjects has been materially enlarged, partly through photographing the individuals and groups of Indian delegations to Washington; while the library has increased at a normal rate, chiefly through exchanges.

MISCELLANEOUS.

Library and publications.—Mr. F. W. Hodge has remained in charge of the library, and has also continued editorial work on the reports. During the year he outlined a plan of library arrangement on the basis of the classification of anthropic science set forth in this and preceding reports, thus preparing the way for a systematic catalogue for the use of the collaborators and the many visitors to the Bureau. The editorial work of the year has been especially arduous by reason of the considerable volume of matter in the hands of the printer and the number and elaborateness of the accompanying illustrations; but his work has been performed with energy and ability.

Translation.—During a considerable part of the year Col. F. F. Hilder has been employed as ethnologic translator, and, in addition, has performed the duties of chief clerk. One of his translations is noted in an earlier paragraph; others made from time to time as needs arose have greatly facilitated the preparation of the Cyclopedia of Indian Tribes, the researches concerning the Seri and Papago Indians, and other lines of work.

Illustrations.—Mr. De Lancey W. Gill has remained in charge of the photographic laboratory and of the preparation of illustrations by other than photographic means, and the progress of his work has been highly satisfactory. The additions to the photographic negatives representing Indian visitors to Washington and the work of field parties have been unprecedented.

I have the honor to be, yours, with respect,

J. W. POWELL, *Director.*

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

APPENDIX III.

REPORT ON THE OPERATIONS OF THE INTERNATIONAL EXCHANGE SERVICE FOR THE YEAR ENDING JUNE 30, 1899.

I have the honor to present herewith the report upon the operations of the International Exchange Service for the year ending June 30, 1899.

This service continues to occupy the several rooms in the eastern basement of the Smithsonian Institution which were prepared for its use about six years ago. As the work conducted is chiefly clerical in character, these quarters are fitted up as business offices, being furnished with the requisite desks, sorting tables and bins, book shelves, filing cases for letters, invoices and receipt cards, typewriter, maps, directories, etc. The property acquired during the year has consisted mainly of expendable materials used in the shipment of exchanges and in connection with the office work, such as packing boxes, lumber and hardware, wrapping paper, twine, and stationery. All structural repairs are made by the Smithsonian Institution, but a small amount from the Exchange appropriation was expended for improving the facilities for handling packages and for other necessary minor changes.

The only losses to be recorded for the year were of two shipments of exchanges. One consisted of a case of United States Government documents destined for Uruguay, damaged by the foundering of the steamship *Bellova;* the other of five cases of miscellaneous exchanges which were lost by the sinking of the steamer *La Bourgogne* through collision with the ship *Cromartyshire* on July 4, 1898. It has, fortunately, been possible to obtain and forward duplicates of nearly all the publications so destroyed.

As to the extent of the operations of the service during the past twelve months, it may be said that the total number of packages from all sources received and shipped aggregated 97,835, being 13,627 in excess of the number handled the year before, an increase of 16.18 per cent. The total weight of the shipments for 1899 was 317,883 pounds, an increase of 5.4 per cent as compared with 1898. Of the number of packages named, 58,640 were dispatched abroad from domestic sources, while 39,195 were received from abroad. Departments and bureaus of the United States Government furnished 33,001 of the former and received 16,554 of the latter.

A small increase in the appropriation permitted certain much-desired improvements to be made in connection with ocean transportation, whereby greater dispatch in forwarding and more frequent shipments were rendered possible. Much still remains to be accomplished in this direction, however, before the service can be expected to reach the standard of efficiency expected of it, and at which the Institution has always aimed. It is pleasing, therefore, to note a second increase in the appropriation, to become available on July 1, 1899, which will afford the means for still further advancement along the lines in question.

The number of separate shipments made during 1899 was 567, as against 219 for 1898. The additional labor which this involved was entirely performed by the regular force of clerks and packers, though not without its being severely overtaxed at times.

Through the cooperation of the American minister to Athens exchange relations with Greece, which had been suspended since the outbreak of the Greco-Turkish

war, were resumed in 1898, and on March 4 last the transmission of official exchanges to Turkey, which had also been interrupted, was renewed. Negotiations are in progress with China, Costa Rica, and Japan looking toward a more systematic interchange of public documents with the two former countries and the perfecting of means for the local distribution of miscellaneous publications in the latter.

Upon the outbreak of hostilities between Spain and the United States all transmissions between the two countries were necessarily suspended, the restriction placed upon the postal service and the refusal on the part of steamship companies to accept freight for Spanish ports making any other course impossible. Steps have already been taken toward reestablishing this service, which, it is hoped, will soon be in full operation. Exchanges for Cuba, Puerto Rico, and the Philippines, formerly sent through Madrid, are now forwarded directly to those islands.

In the last report mention was made of the establishment of agencies at Vienna, Austria, and Budapest, Hungary. After a trial of nine months with the former and of seventeen months with the latter, it is gratifying to report that the benefits have been even greater than was anticipated.

Great Britain and Germany still occupy, in the order named, the foremost positions among the patrons of the Exchange Service, both as to the number of correspondents and the quantity of their contributions. In each of these countries the Smithsonian Institution has agents in its own employ, Messrs. William Wesley & Son in London and Dr. Felix Flügel in Leipzig, whose long connection with the service is a sufficient guaranty of their devotion to its interests.

Tabular statement of the work of the International Exchange Service during the fiscal year 1898–99.

Date.	Number of packages handled.	Weight of packages handled.	Number of correspondents June 30, 1899.				Packages sent to domestic addresses.	Cases shipped abroad.
			Foreign societies.	Domestic societies.	Foreign individuals.	Domestic individuals.		
1898.								
July	8,569	21,114						
August	12,390	29,903						
September	5,223	20,442						
October	5,636	16,912						
November	6,366	22,840						
December	6,419	18,899						
1899.								
January	13,032	39,331						
February	4,990	13,554						
March	9,824	23,440						
April	10,392	50,665						
May	7,726	45,891						
June	7,268	14,892						
Total	97,835	317,883	10,322	2,596	13,378	4,673	30,645	1,500
Increase over 1897–98	13,627	16,411	157	63	1,000	291	9,588	170

The following table shows the number of packages of exchanges handled and the increase in the number of correspondents each year from 1893 to 1899:

	1892-93.	1893-94.	1894-95.	1895-96.	1896-97.	1897-98.	1898-99.
Number of packages received	101,063	97,969	107,118	88,878	81,162	84,208	97,835
Weight of packages received.....lbs..	200,928	235,028	326,955	258,731	247,444	301,472	317,883
Ledger accounts:							
Foreign societies	6,896	6,991	8,751	8,022	9,414	10,165	10,322
Foreign individuals	8,554	8,619	9,609	10,878	12,013	12,378	13,378
Domestic societies	2,414	1,620	2,014	2,115	2,445	2,533	2,596
Domestic individuals	5,010	2,993	3,034	3,899	4,136	4,382	4,673
Packages to domestic addresses	29,454	32,931	29,111	34,091	23,619	21,057	30,645
Cases shipped abroad	878	905	1,364	1,043	1,300	1,330	1,500

CORRESPONDENTS.

The record of exchange correspondents at the close of the year contained 30,969 addresses, being an increase of 1,511 over the preceding year. The following table gives the number of correspondents in each country, and also serves to illustrate the scope of the service, whose utility is becoming every year better and more widely appreciated:

Number of correspondents of the International Exchange Service in each country on June 30, 1899.

Country.	Correspondents.			Country.	Correspondents.		
	Libraries.	Individuals	Total.		Libraries.	Individuals	Total.
AFRICA.				AMERICA (NORTH).			
Algeria	20	29	49	Canada	234	389	623
Angola	1	....	1	Central America:			
Azores	5	14	19	British Honduras	4	6	10
Beira	....	1	1	Costa Rica	23	24	47
Canary Islands	1	6	7	Guatemala	37	48	85
Cape Colony	37	61	98	Honduras	8	22	30
Cape Verde Islands	....	4	4	Nicaragua	10	21	31
Congo Free State	....	3	3	Salvador	12	8	20
Egypt	25	46	71	Greenland	2	1	3
French Congo	....	1	1	Mexico	129	108	237
Gambia	....	2	2	Newfoundland	10	10	20
Gold Coast	....	2	2	St. Pierre-Miquelon	1	2	3
Gorée-Dakar	....	3	3	United States	2,596	4,673	7,269
Lagos	2	....	2	West Indies:			
Liberia	2	5	7	Anguilla	....	1	1
Lorenzo Marques	....	2	2	Antigua	4	4	8
Madagascar	1	6	7	Bahamas	2	10	12
Madeira	3	3	6	Barbados	6	9	15
Mauritius	11	6	17	Bermuda	1	12	13
Morocco	....	10	10	Buen Ayre	....	1	1
Mozambique	....	1	1	Cuba	34	82	116
Natal	8	13	21	Curaçao	....	3	3
Orange Free State	....	1	1	Dominica	1	6	7
Réunion	2	....	2	Grenada	1	5	6
St. Helena	2	2	4	Guadeloupe	2	5	7
Sierra Leone	1	2	3	Haiti	5	15	20
South African Republic	11	8	19	Jamaica	10	28	38
Tunis	6	6	12	Martinique	1	3	4
Zanzibar	....	5	5	Montserrat	....	2	2

Number of correspondents of the International Exchange Service in each country on June 30, 1899—Continued.

Country.	Correspondents. Libraries.	Individuals	Total.
AMERICA (NORTH)—c't'd.			
West Indies—Continued.			
Nevis	....	1	1
Puerto Rico	....	4	4
St. Bartholomew	....	2	2
St. Christopher	1	4	5
St. Croix	1	....	1
St. Eustatius	....	1	1
St. Martin	....	2	2
St. Lucia	1	3	4
St. Thomas	....	5	5
St. Vincent	1	2	3
Santo Domingo	2	10	12
Tobago	....	1	1
Trinidad	9	8	17
Turks Islands	1	5	6
AMERICA (SOUTH).			
Argentina	115	96	211
Bolivia	15	5	20
Brazil	99	116	215
British Guiana	14	9	23
Chile	68	67	135
Colombia	30	41	71
Dutch Guiana	2	3	5
Ecuador	13	18	31
Falkland Islands	....	5	5
French Guiana	....	2	2
Paraguay	10	7	17
Peru	26	50	76
Uruguay	35	24	59
Venezuela	28	38	66
ASIA.			
Arabia	....	7	7
Borneo	....	1	1
British Burma	6	6	12
British North Borneo	....	1	1
Celebes	....	1	1
Ceylon	20	9	29
China	32	64	96
Cochin China	4	2	6
Corea	1	7	8
Cyprus	2	3	5
French East Indies	1	1	2
Hongkong	5	7	12
India	179	155	334
Japan	96	202	298
Java	13	22	35
New Guinea	....	1	1
Persia	2	7	9
Philippine Islands	6	10	16
ASIA—continued			
Portuguese India	1	....	1
Siam	4	9	13
Straits Settlements	10	12	22
Sumatra	....	2	2
AUSTRALASIA.			
New South Wales	56	91	147
New Zealand	63	73	136
Queensland	30	37	67
South Australia	38	54	92
Tasmania	16	13	29
Victoria	86	99	185
Western Australia	11	14	25
EUROPE.			
Austria-Hungary	620	754	1,374
Belgium	286	306	592
Bulgaria	12	10	22
Denmark	93	142	235
France	1,491	1,493	2,984
Germany	2,108	2,234	4,342
Gibraltar	....	4	4
Great Britain	1,615	3,261	4,876
Greece	36	33	69
Iceland	16	7	23
Italy	702	633	1,335
Luxemburg	8	2	10
Malta	8	11	19
Netherlands	164	206	370
Norway	113	109	222
Portugal	90	69	159
Roumania	29	40	69
Russia	412	609	1,021
Servia	16	12	28
Spain	144	153	297
Sweden	154	224	378
Switzerland	292	435	727
Turkey	29	69	98
POLYNESIA.			
Fiji Islands	1	3	4
Hawaiian Islands	21	42	63
Marshall Islands	....	1	1
New Caledonia	....	1	1
New Hebrides	1	....	1
Samoa	....	5	5
Tahiti	....	3	3
Tonga	....	1	1
International	33	....	33
Total	12,918	18,051	30,969

EXCHANGE OF GOVERNMENT DOCUMENTS.

The following table shows the number of packages handled during the year for the several branches of the Government. By comparison with the last report it will be observed that there has been an increase this year of 2.5 per cent in the transmissions abroad and of 60 per cent in the receipts. The packages enumerated as sent for the Library of Congress were those forwarded in conformity with the act of Congress of 1867.

Statement of Government exchanges during the year 1898–99.

Name of bureau.	Packages.		Name of bureau.	Packages.	
	Received for.	Sent by.		Received for.	Sent by.
American Historical Association	6	42	Hydrographic Office	57	....
Astrophysical Observatory	1	1	Intercontinental Railway Commission	....	7
Bureau of American Ethnology	198	81	Interstate Commerce Commission	14	30
Bureau of American Republics	4	....	Library of Congress	12,329	13,798
Bureau of Education	81	3	Light-House Board	....	1
Bureau of Medicine and Surgery	2	....	Marine-Hospital Service	13	37
Bureau of the Mint	1	....	National Academy of Sciences	69	795
Bureau of Navigation	3	....	National Museum	267	3,173
Bureau of Statistics, Department of State	1	....	National Zoological Park	15	2
Bureau of Statistics, Treasury Department	41	1	Nautical Almanac Office	26	231
Bureau of Steam Engineering, Navy Department	1	....	Naval Observatory	119	497
Census Office	21	....	Navy Department	6	....
Civil Service Commission	2	4	Office of the Chief of Engineers	30	94
Coast and Geodetic Survey	94	12	Office of Indian Affairs	2	....
Commissioners of the District of Columbia	7	6	Ordnance Office, War Department	3	....
Comptroller of the Currency	1	....	Patent Office	54	1,280
Department of Agriculture	196	5	President of the United States	1	....
Department of the Interior	29	1,354	Signal Office	8	....
Department of Labor	15	4	Smithsonian Institution	2,031	6,221
Department of State	14	3	Superintendent of Documents	....	151
Entomological Commission	6	....	Surgeon-General's Office (Army)	128	347
Fish Commission	69	406	Treasury Department	4	113
General Land Office	4	....	War Department	31	100
Geological Survey	510	3,209	War Records Office	....	53
			Weather Bureau	40	940
			Total	16,554	33,001

RELATIVE INTERCHANGE OF PUBLICATIONS BETWEEN THE UNITED STATES AND OTHER COUNTRIES.

Following is a comparative statement of exchange transmissions by packages between the United States and other countries for the years 1898 and 1899:

Comparative statement of packages received for transmission through the International Exchange Service during the fiscal years ending June 30, 1898, and June 30, 1899.

Country.	1898.		1899.	
	Packages.		Packages.	
	For—	From—	For—	From—
Algeria	85	94	116	[illegible]
Angola	....	....	1	....
Antigua	....	....	[illegible]	....
Argentina	1,302	343	1,534	492
Austria-Hungary	3,076	1,348	3,578	1,381
Azores	4	....	10	....
Bahamas	20	....	11	....
Barbados	6	4	5	....
Belgium	1,634	1,018	1,701	1,382
Bermudas	5	....	8	....
Bolivia	22	....	26	....
Brazil	836	991	903	409
British America	1,951	1,130	2,060	1,281
British Burma	1	....	2	....
British Colonies[1]	42	....	....	....
British Guiana	31	2	37	1
British Honduras	9	....	4	....
Bulgaria	30	1	55	1
Canary Islands	1	....	1	....
Cape Colony	175	5	194	3
Ceylon	....	....	46	....
Chile	670	121	718	....
China	151	165	155	148
Colombia	351	....	356	....
Costa Rica	179	597	214	295
Cuba	80	....	204	11
Cyprus	....	....	3	....
Denmark	827	170	777	127
Dominica	....	....	3	....
Dutch Guiana	8	....	5	....
Ecuador	56	....	61	....
Egypt	105	....	88	21
Fiji Islands	2	....	1	....
France	6,251	2,430	7,022	3,129
Friendly Islands	23	....	27	....
Germany	10,089	4,510	11,219	6,018
Gold Coast	13	....	4	....
Grenada	....	....	3	....
Great Britain and Ireland	10,271	5,204	10,411	13,603
Greece	120	....	395	....
Greenland	5	....	7	....
Guadeloupe	1	....	2	....
Guatemala	57	....	65	....
Guinea	1	....	1	....

[1] During the present year packages were not charged to "British Colonies," but to each specific colony to which they were sent.

Comparative statement of packages received for transmission through the International Exchange Service, etc.—Continued.

Country.	1898. Packages. For—	1898. Packages. From—	1899. Packages. For—	1899. Packages. From—
Haiti	282		283	
Hawaiian Islands	70		85	12
Honduras	11	34	12	72
Hongkong			23	
Iceland	59		49	
India	1,011	77	1,069	89
Italy	3,479	1,079	3,391	1,334
Jamaica	63		67	
Japan	841	18	955	52
Java	131	124	152	66
Korea	2		1	
Lagos			1	
Leeward Islands	2			
Liberia	37		26	
Lourenço Marquez			1	
Luxemburg	43	1	64	
Madagascar	6		4	
Madeira	3		4	
Malta	30		34	
Martinique			4	
Mauritius	44		59	
Mexico	1,221	57	1,506	1,418
Natal	13	2	28	
Netherlands	1,191	421	1,392	543
Newfoundland			25	
New Hebrides	1			
New South Wales	860	100	1,113	261
New Zealand	550	8	572	6
Nicaragua	44		35	
Norway	861	309	1,039	259
Paraguay	21		18	
Persia	2		3	
Peru	380	50	419	186
Philippine Islands	44		47	
Portugal	749	843	635	500
Queensland	544		528	
Reunion			13	
Roumania	53	1	110	110
Russia	2,053	1,247	2,515	1,033
St. Helena	6		6	
St. Kitts			1	
St. Vincent			1	
Samoa	1			
Santa Lucia			3	
Santo Domingo	1		2	
San Salvador	44		53	
Servia	50	2	43	46
Siam	38		38	
Sierra Leone			1	
South African Republic	33	2	1,782	
South Australia	460	36	491	43
Spain	698		450	

Comparative statement of packages received for transmission through the International Exchange Service, etc.—Continued.

Country.	1898. Packages.		1899. Packages.	
	For—	From—	For—	From—
Straits Settlements	35	....	46	....
Sumatra	1	....	1	....
Sweden	2,754	392	1,512	280
Switzerland	1,720	838	1,847	615
Syria	6	....	18	....
Tasmania	354	....	364	....
Trinidad	42	....	57	....
Tunis	18	....	8	....
Turkey	349	3	76	....
Turks Islands	4	....	4	....
United States	21,057	58,640	30,645	62,184
Uruguay	419	84	539	237
Venezuela	348	....	379	....
Victoria	751	87	825	131
West Australia	307	....	324	....
Zanzibar	1	....	....	....

The following list gives the names of companies and other mediums of transportation that have aided the Institution during the past year in the transmission and distribution of exchanges, either without compensation or at minimum rates, some of which have extended similar courtesies to the Institution for many years:

American Board of Commissioners for Foreign Missions, Boston, Massachusetts.
Amundsen, L. O. G., acting consul of Denmark, New York.
Atlas Line of Mail Steamers (Pim, Forwood & Kellock, agents), New York.
Board of Foreign Missions of the Presbyterian Church, New York.
Calderon, Climaco, consul-general of Colombia, New York.
Compagnie Générale Transatlantique, New York.
Cunard Steamship Company (Vernon H. Brown & Co., agents), New York.
Eddy, Thomas A., consul of Uruguay, New York.
Grace, W. R., & Co., New York.
Hamburg-American Line, New York.
Hensel, Bruckmann & Lorbacher, New York.
Holland-American Line, New York.
Mediterranean and New York Steamship Company (Phelps Bros. & Co., agents), New York.
Navarro, Juan N., consul-general of Mexico, New York.
North German Lloyd Steamship Company (Oelrichs & Co., New York, and A. Schumacher & Co., Baltimore, agents).
Panama Railroad Steamship Line (W. J. Herron, agent), New York
Peraza, N. Bolet, consul-general of San Salvador, New York.
Perry, Edward, & Co., New York.
Red "D" Line of Steamships (Boulton, Bliss & Dallett, general managers), New York.
Red Star Line (International Navigation Company, agents), New York.
Röhl, Carlos, consul-general of Argentina, New York.
Santos, Alejandro, consul-general of Bolivia, New York.
Stewart, John, consul-general of Paraguay, Washington, District of Columbia.
Taveira, Luis Augusto de M. P. de A., consul-general of Portugal, New York.

Woxen, Karl G. M., consul of Sweden and Norway, New York.
Yela, Julius, chancellor, consulate of Guatemala, New York.

The following is a list of the Smithsonian correspondents acting as distributing agents, or receiving publications for transmission to the United States, and of countries receiving regularly exchanges through the Institution:

Algeria. (*See* France.)
Angola. (*See* Portugal.)
Argentina: Museo Nacional, Buenos Ayres.
Austria: K. K. Statistische Central-Commission, Wien.
Azores. (*See* Portugal.)
Belgium: Commission Belge des Échanges Internationaux, Brussels.
Bolivia: Oficina Nacional de Inmigracion, Estadística y Propaganda Geográfica, La Paz.
Brazil: Bibliotheca Nacional, Rio de Janeiro.
British America: Packages sent by mail.
British Colonies: Crown Agents for the Colonies, London, England.
British Guiana. (*See* British Colonies.)
British Honduras. (*See* British Colonies.)
Bulgaria. (*See* Germany.)
Canary Islands. (*See* Spain.)
Cape Colony: Colonial Secretary, Cape Town.
Chile: Universidad de Chile, Santiago.
China: Zikawei Observatory, Shanghai.
Colombia: Biblioteca Nacional, Bogotá.
Costa Rica: Oficino de Deposito, Reparto y Canje Internacional, San José.
Cuba: Dr. Vicente de la Guardia, Habana.
Denmark: Kong-Danske Videnskabernes Selskab, Copenhagen.
Dutch Guiana: Surinaamsche Koloniale Bibliotheek, Paramaribo.
Ecuador: Transmissions temporarily suspended.
East India: India Store Department, India Office, London.
Egypt: Société Khédiviale de Géographie, Cairo.
Fiji Islands. (*See* British Colonies.)
France: Bureau Français des Échanges Internationaux, Paris.
Friendly Islands: Packages sent by mail.
Germany: Dr. Felix Flügel, Schenkendorf strasse, 9, Leipzig.
Gold Coast. (*See* British Colonies.)
Great Britain and Ireland: William Wesley & Son, 28 Essex street, Strand, London, England.
Greece: Prof. R. B. Richardson, Director, American School of Classical Studies, Athens.
Greenland. (*See* Denmark.)
Guadeloupe. (*See* France.)
Guatemala: Instituto Nacional de Guatemala, Guatemala.
Guinea. (*See* Portugal.)
Haiti: Secrétaire d'Etat des Relations Extérieures, Port au Prince.
Hawaiian Islands: Foreign Office, Honolulu.
Honduras: Biblioteca Nacional, Tegucigalpa.
Hungary: Dr. Joseph von Körösy, "Redoute," Budapest.
Iceland. (*See* Denmark.)
Italy: Biblioteca Nazionale Vittorio Emanuele, Rome.
Jamaica. (*See* British Colonies.)
Java. (*See* Netherlands.)
Korea: Packages sent by mail.
Leeward Islands. (*See* British Colonies.)

Liberia: Care of American Colonization Society, Washington, District of Columbia.
Luxemburg. (*See* Germany.)
Madagascar. (*See* France.)
Madeira. (*See* Portugal.)
Malta. (*See* British Colonies.)
Mauritius. (*See* British Colonies.)
Mexico. Packages sent by mail.
Mozambique. (*See* Portugal.)
Natal. Agent-General for Natal, London, England.
Netherlands. Bureau Scientifique Central Néerlandais, Den Helder.
New Guinea. (*See* Netherlands.)
New Hebrides. Packages sent by mail.
Newfoundland. Packages sent by mail.
New South Wales. Government Board for International Exchanges, Sydney.
New Zealand. Colonial Museum, Wellington.
Nicaragua. Ministerio de Relaciones Exteriores, Managua.
Norway. Kongelige Norske Frederiks Universitet, Christiania.
Paraguay. Care Consul-General of Paraguay, Washington, District of Columbia.
Persia. (*See* Russia.)
Peru. Biblioteca Nacional, Lima.
Philippine Islands. Packages sent by mail.
Portugal. Bibliotheca Nacional, Lisbon.
Queensland. Registrar-General of Queensland, Brisbane.
Roumania. (*See* Germany.)
Russia. Commission Russe des Échanges Internationaux, Bibliothèque Impériale Publique, St. Petersburg.
Saint Helena. (*See* British Colonies.)
Santo Domingo. Packages sent by mail.
San Salvador. Museo Nacional, San Salvador.
Servia. (*See* Germany.)
Siam. Board of Foreign Missions of the Presbyterian Church, New York.
South African Republic. William Wesley & Son, 28 Essex street, Strand, London.
South Australia. Astronomical Observatory, Adelaide.
Straits Settlements. (*See* British Colonies.)
Sumatra. (*See* Netherlands.)
Syria. Board of Foreign Missions of the Presbyterian Church, New York.
Sweden. Kongliga Svenska Vetenskaps Akademien, Stockholm.
Switzerland. Bibliothèque Fédérale, Bern.
Tasmania. Royal Society of Tasmania, Hobart.
Trinidad. (*See* British Colonies.)
Tunis. (*See* France.)
Turkey. American Board of Commissioners for Foreign Missions, Boston, Massachusetts.
Turks Islands. (*See* British Colonies.)
Uruguay. Oficina de Depósito, Reparto y Canje Internacional, Montevideo.
Venezuela. Museo Nacional, Carácas.
Victoria. Public Library, Museum, and National Gallery, Melbourne.
West Australia. Agent-General, London, England.
Zanzibar. Packages sent by mail.

The distribution of exchanges to foreign countries was made in 1,280 cases, representing 567 transmissions, as follows:

Argentina	32	Mexico[1]	
Austria	53	Natal[2]	
Belgium	38	New South Wales	22
Bolivia	5	Netherlands	29
Brazil	14	New Zealand	8
British colonies	8	Nicaragua	6
Cape Colony	7	Norway	17
China	6	Paraguay	6
Chile	9	Peru	8
Colombia	7	Polynesia	6
Costa Rica	8	Portugal	11
Cuba	10	Queensland	8
Denmark	15	Roumania[3]	
Dutch Guiana	1	Russia	53
East India	14	Salvador	7
Egypt	7	Servia[3]	
France and colonies	139	South Australia	11
Germany	194	South African Republic[2]	
Great Britain and Ireland	274	Sweden	35
Greece	14	Switzerland	37
Guatemala	6	Syria	5
Haiti	2	Tasmania	1
Honduras	4	Turkey	7
Hungary	19	Uruguay	8
Italy	64	Venezuela	6
Japan	18	Victoria	17
Liberia	3	Western Australia	1

Shipments of United States Congressional publications were made on August 31 and September 6, 1898, and January 17, 1899, to the Governments of the following-named countries:

Argentina.	Denmark.	Netherlands.	South Australia.
Austria.	France.	New South Wales.	Spain.
Baden.	Germany.	New Zealand.	Sweden.
Bavaria.	England.	Norway.	Switzerland.
Belgium.	Haiti.	Peru.	Tasmania.
Buenos Ayres.	Hungary.	Portugal.	Uruguay.
Brazil.	India.	Prussia.	Venezuela.
Canada (Ottawa).	Italy.	Queensland.	Victoria.
Canada (Toronto).	Japan.	Russia.	Western Australia.
Chile.	Mexico.	Saxony.	Württemberg.
Colombia.			

Respectfully submitted,

RICHARD RATHBUN,
Assistant Secretary.

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

[1] Packages sent by mail.
[2] Included in transmissions to Great Britain.
[3] Included in transmissions to Germany.

Appendix IV.

REPORT OF THE SUPERINTENDENT OF THE NATIONAL ZOOLOGICAL PARK.

SIR: I have the honor to submit a report of the operations of the park for the fiscal year ending June 30, 1899.

The amount and classes of property belonging to the park at the close of that period are as follows:

Buildings for animals	$52,000
Buildings for administrative purposes	12,000
Office furniture, fixtures, and books	1,500
Machinery, tools, and implements	2,000
Fences and outdoor inclosures	23,000
Roadways, paths, and rustic seats, etc	40,000
Nurseries	1,000
Horses	800
Animals in zoological collection	28,000

These animals were 675 in number, classified as follows:

	Indigenous.	Foreign.	Domesticated.	Total.
Mammals	219	76	98	393
Birds	101	25	56	182
Reptiles	84	16	....	100
Total	404	117	154	675

A list showing these in detail is appended hereto.

During the year the following property has been acquired:

Animals, costing for purchase, collection, and transportation	$3,100
Books, photographs, and apparatus costing	600

The accessions of animals are, in detail, as follows:

Presented	96
Received from United States officers abroad	18
Purchased and collected	207
Lent	19
Received in exchange	21
Born in the National Zoological Park	40
Total	401

The following improvements and repairs have been made: The building temporarily used as an aquarium has been more fully fitted with skylights at an expense of $400; to secure a proper supply of water for the aquarium it was necessary to construct two wells and to lay water pipe from them to the building; the expense of this was $300; tin roofing was placed on portions of the animal house and of the antelope house, at a cost of $450; the walls of the office building have been under-

School Children Studying in Zoological Park.

pinned and part of its interior finished; the cost, including architect's fees, was $3,000.

Work upon the roads has been:

Grading and macadamizing the road to the Klingle Ford entrance from where it crosses the main road (in compliance with the terms of the appropriation act)	$5,000
Necessary repairs to roadways	1,000
Necessary repairs to walks	600
Construction of macadam walk from Quarry road to animal house	1,200
Construction of macadam walk through the valley in which the beavers are situated	300
Construction of a guard wall (stone masonry) along Adams Mill road	500
Grounds along the roadway and by the buildings have been put in order and planted, at an expense of	1,500

Other repairs have been:

Riprapping creek bank to protect road	500
Repairs to bank of sea-lion pond	200
Removal of detritus from pelican pond and paving drain trench	350

Other improvements are:

Building a new fence for llama paddock, inclosures for rodents, etc	1,000
Laying water pipe to various yards	650
Laying terra-cotta drainpipe from aquarium to sea-lion pond	300

The losses to the collection have been as follows:

Deaths	221
Animals exchanged for others	37
Animals returned to owners	17

Most of the deaths have been of small animals, birds and reptiles, many of them dying from lack of suitable quarters.

The important losses were few, the principal ones being as follows: The elephant Golddust, who, after an illness of some months, died of chronic enteritis. The ostrich belonging to the park died from excessive feeding in hot weather, and three sea lions and seven harbor seals died from the ill effects of the exceedingly muddy condition of the water in their pond. An effort has been made to lessen this evil by cleaning and graveling the bottom, but until some arrangement is made for filtering the water flowing into the pond good results with these animals can not be expected. Two of the prong-horn antelope died of a disorder very similar to, if not identical with, the dreaded pest known as the "cattle and game disease," that has made great ravages in foreign zoological gardens. Fortunately, this did not spread to the other herbivora.

Some important exchanges of animals have been effected during the year. Through the kindness of Dr. Alexander Graham Bell the Director of the Zoological Garden at Tokyo, Japan, Dr. Ishikawa, sent twelve beautiful mandarin ducks, which are a valuable addition to the collection. In return six raccoons were sent, which have arrived at their destination in good condition. The director of the Central Park menagerie, Mr. John W. Smith, sent for exchange a nilgai and two zebus. Eight harbor seals were received from Mr. J. H. Starin, the proprietor of Glen Island; a camel from the board of public park commissioners, Baltimore, and a fine tigress and a red kangaroo from Mr. William Bartels.

Two lionesses, two zebu cows, and a male axis deer were deposited by the proprietor of the Barnum & Bailey Shows.

I wish to call attention to the advantage that has accrued to the park from the temporary loan of animals. Eight lion cubs have been born, which, under the agree-

ment made with the owners of the lionesses, became the property of the park. When these young animals arrive at a suitable age they are readily exchanged for other specimens, and thus form a valuable addition to the resources of the park.

In accordance with a design long ago formed by the Secretary of the Smithsonian Institution, means were instituted during the year for interesting officers of the United States stationed abroad in the objects and needs of the National Zoological Park. A circular to officers was prepared showing a map of the park and illustrated with views taken at various points within it. By permission of the Secretaries of State, of War, and of the Navy, copies of this circular were sent to the principal officers on foreign stations. It is expected that considerable accessions to the collection will be made from this source. The circular is reprinted in subsequent pages.

In anticipation of this circular the Navy Department instructed Commander Todd, U. S. N., the officer commanding the U. S. S. *Wilmington*, who was about to make a voyage up the Amazon River, to make such collections as were practicable for the park. The result was most satisfactory. A considerable consignment of animals was made from the *Wilmington*, including a fine male tapir, a harpy eagle, and a number of monkeys, birds, and small mammals. An illustration of the eagle is appended.

The sea-lion pond and the lower ford were materially injured by high water at the breaking up of the ice in the winter. Damage of this sort is certain to occur occasionally, and may be very serious if not guarded against by riprapping the bank. The elk inclosures and the pond for the pelicans just above the high bridge have been greatly damaged by the wash of sand and dirt from the fresh-cut bank along the eastern boundary. This bank, on the top of which is a newly constructed roadway, known as Park Drive, will remain a cause of expense to the park until steps are taken to prevent this wash. This could be done by planting the banks thickly, but to produce the best results and make a permanent improvement the bank should be made less precipitous and the slope extended into the park itself. As this improvement would benefit the park alone, it must be done, if ever, at public expense. The cost would be about $10,000, and I would recommend that Congress be asked to appropriate a sum for this purpose.

Now that roadways leading into the park have been completed at grades established by the District authorities, it is desirable to surround the entire domain by a tight fence which will keep out dogs and other predatory animals, and thus make it possible to keep small game at large in the park—pheasants, woodcock, grouse, and quail, for instance. The estimated cost of such a fence with suitable gates at the entrances is $20,000.

At the request of the Capital Traction Railway Company a new entrance to the park has been made upon the south side to accommodate those who desire to enter by the pathway laid out by this company from the railway "loop" at the eastern end of the Cincinnati street bridge, where a freight station and waiting room have been established. It is not, however, much used. It would be greatly to the convenience of the public if a right of way could be acquired by which foot passengers could proceed from this "loop" across the grounds owned by the colored cemetery association to the Adams Mill road within the park. The distance is not great, being not more than 300 feet, and the descent is easy.

The improvement of access to the park has greatly increased its use by the general public. It is found to be of special importance to schools, and groups of children under charge of teachers may be observed almost any fine day during the school season. There are herewith appended two illustrations showing such groups.

By the act of March 3, 1899, there was appropriated $5,000 to widen the Adams Mill road from Columbia road to the park entrance, and steps have been taken to carry out this provision.

I desire to recommend an increase in the force of watchmen. The establishment of the railway loop at the bridge near the park has largely increased the number of

School Children Studying Botany in Zoological Park.

visitors after the hour when the buildings are closed and the keepers and day watchmen have left. Heretofore one night watchman has been deemed sufficient, but another is now necessary from 4 p. m. to 12.

The circular letter and "Advice to collectors" alluded to in a previous page is reprinted below, omitting most of the illustrations.

SMITHSONIAN INSTITUTION,
Washington, U. S. A., July 1, 1899.

The Secretary, on behalf of the Regents of the Smithsonian Institution, and with the permission of the honorable the Secretaries of State, of War, and of the Navy, calls the attention of officers of the United States on foreign stations to the fact that there is at the capital a National Zoological Park, established by an act of Congress approved April 30, 1890, which provides—

"That the National Zoological Park is hereby placed under the direction of the Regents of the Smithsonian Institution, who are authorized to transfer to it any living specimens, whether of animals or plants now or hereafter in their charge, to accept gifts for the park at their discretion, in the name of the United States, to make exchanges of specimens, and to administer the said Zoological Park for the advancement of science and the instruction and recreation of the people.

"That the heads of the Executive Departments of the Government are hereby authorized and directed to cause to be rendered all necessary and practicable aid to the said Regents in the acquisition of collections for the Zoological Park."

This park, of which some idea may be formed by the accompanying map and illustrations,[1] has been established in an unusually beautiful site near the city of Washington. It is intended to form here a representative national collection which, while especially rich in our native American animals, shall also contain specimens from all parts of the world, and shall be to America what the zoological gardens at London, Paris, and Berlin are to their respective countries.

For several years Congress made no appropriation for the purchase of animals, and the park is still largely dependent upon gifts to increase the collection, which is far from adequate as an exhibit in a national institution.

If officers stationed abroad, who may be interested in animal life, would bear in mind the necessities of the park, many additions could be made to the collection. Almost any foreign animals would be gladly received.

Expenses of boxing and of land transportation, where necessary, will always be paid by the Zoological Park.

Purchase of animals can be made only in exceptional cases, but if the opportunity for any especially desirable acquisition arises, the Secretary of the Smithsonian Institution would be pleased to be advised by letter, or in urgent cases by telegraph. The Secretary would also be glad to correspond with officers who expect to visit regions where interesting animals occur.

Public recognition of gifts is made, the names of donors being placed upon the labels attached to the cages or pens, and a notice of the gifts with the names of the donors is also made a part of the annual report of the Secretary of the Smithsonian Institution.

A list of the most important animals that can be collected in different countries is appended hereto, and concise directions for boxing, shipping, and feeding are given.

S. P. LANGLEY, *Secretary.*

ADVICE TO COLLECTORS.

ANIMALS ESPECIALLY DESIRED.

The new possessions of the United States are comparatively poor in animals, but it is especially desirable to have as full a representation of the fauna as possible. While all will be valued, those whose names are italicized are particularly desirable.

Cuba and Puerto Rico afford the *manatee*, or sea cow, which frequents bays and mouths of rivers; the *flamingo*, spoonbill, ibis, pelican, several species of parrots and parrakeets, a variety of pigeons, the ani, and other interesting birds. Boas of several kinds occur in these islands, and large lizards of different species are very abundant. The agouta (*Solenodon*) and the hutia (*Capromys*), animals a little larger than a common rat, and the crocodile are also found in Cuba, and an interesting macaw occurs in the Isle of Pines.

In the *Philipine Islands* the most notable mammals are the "tamarau," a small wild buffalo found on Mindoro; several species of deer; the "babui," or wild hog;

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[1] Most of the illustrations are not reprinted here.—EDITOR.

monkeys of two species, a small cat; two species of civet cat, or musang; fruit-eating bats of different species, several peculiar large rats, the *colugo*, or *flying lemur*, and the very remarkable and interesting *tarsier*, or "*magou*." Among these the last two and the "tamarau" are especially important. Specimens of the domesticated buffalo also are desired.

Of the birds the eagles, hornbills, cockatoos, parrakeets, the pheasants and pigeons, the megapod, pelican, and the ground cuckoos are perhaps the most important. Among these any *hornbills* or brilliant-plumaged *cockatoos* and *parrakeets* would be specially valued, but specimens of all the larger birds, of both land and water, are desired. The python and other snakes, large lizards, and turtles would also be acceptable.

Below are mentioned a few of the more notable animals to be found in other regions and those which are especially desired.

Central America.—*Tapir*, manatee, West Indian seal, *jaguar*, other cats (except the puma), monkeys, *sloth*, *anteater*, coati-mundi, tayra, kinkajou, tree porcupine, and other large rodents, curassows, parrots and macaws, *king vulture*, flamingo, spoonbill, ibis, crocodiles, large snakes, *iguanas* and other large lizards.

South America.—From the great river valleys of the east and north the following animals are desired:

Tapir, *sloth*, *anteaters*, *great armadillo*, *jaguar*, other cats (except the puma), otter, raccoon, wild dogs and foxes, deer, white-lipped peccary, monkeys and marmosets, capybara, viscacha, paca, coypu, porcupine and other large rodents, curassows of various species, guans, tinamous, toucans, parrots and macaws, harpy and other eagles, king vulture, cariama, screamers, jabiru, flamingo, spoonbill, scarlet ibis, and other large wading birds, caimans, large snakes, and large lizards.

Farther to the south occur the guanaco, the Patagonian cavy, the rhea, or American ostrich, the coscoroba and black-necked swans, and several species of geese and penguins, and among the mountains of Chile, Peru, and Bolivia are found the alpaca and vicuña, the chinchilla, the rare *spectacled bear*, and the *condor*.

Asia.—Southern Asia and the adjacent islands afford the rhinoceros (three species), elephant (female only is desired), *tapir*, buffalo, *gayal*, *gaur*, antelopes, gazelles, deer, chevrotains and muntjacs, wild swine, tiger, leopard, *cheetah* and smaller cats, ichneumons, civet cats, bears, orang, gibbons, langurs and related species, proboscis monkey, macaques of various species, black ape, lemurs, and fruit-eating bats; also eagles, vultures, hornbills, pheasants, jungle fowl, tragopans, fruit pigeons, etc., and crocodiles, pythons, and large poisonous snakes.

On the highlands of the interior are found a number of rare and superb mountain sheep and antelopes, several of which, as the argali or Pamir sheep, the serow, and takin, have never as yet been on exhibition in any American or European zoological garden. The same region affords the musk deer, ounce or snow leopard, yak (female chiefly desired), and rare pheasants. The Bactrian camel and the wild ass also may be had there. In Japan may be had the Japanese bear, a deer, a peculiar goat antelope, an interesting monkey, the raccoon-like dog, otter, badger, wild swine, and pheasants.

Africa.—No other region is so rich in animal life as this continent, from which are desired the elephant, hippopotamus, rhinoceros, *zebra*, quagga, buffalo, *giraffe*, *antelopes* of any species, gazelles, the Abyssinian ibex, the "beden" or Egyptian ibex, the Barbary sheep or "arui," water chevrotain, the wart hog and river hog, lion, leopard, *cheetah* and any smaller cats, zorilla, ratel, genets, ichneumons, suricate, Cape hunting dog and aard wolf, jackals, foxes, hyenas, *gorilla*, *chimpanzee*, the chacma baboon and vervet monkey of South Africa, the gelada and hamadryas baboons of Abyssinia, the mandrill, drill, and other baboons, several species of colobus and mangabey, the green monkey, diana, mona, pluto, and other nearly related species of West Africa, the Barbary ape, lemurs, fruit-eating bats, coney, aard vark, and pangolin, eagles, vultures, secretary bird, parrots and parrakeets, hornbills, doves, fruit pigeons, touracous, francolins, guinea fowls, bustards, the larger wading and water birds, etc.; also the ostriches of Somaliland and North Africa.,, etc. crocodiles, large tortoises, pythons, vipers,and other poisonous snakes, monitors and other large lizards. The gorilla has never yet been brought to America. Special care would have to be taken in boxing, feeding, and caring for a specimen. The *giraffe* has almost ceased to be found in any European or American collection. The true *zebra* of southern Africa is almost extinct. This region affords nearly one hundred species of antelopes and gazelles, and any of these would be especially valued. The *secretary bird*, though not rare, would be interesting; but any of the above would be acceptable.

Madagascar affords a wonderful variety of lemurs, the strange and interesting aye-aye, fruit-eating bats, a peculiar cat-like animal known as the fossa, civet cats, the river hog, several large snakes, and a number of desirable birds.

HARPY EAGLE IN ZOOLOGICAL PARK.

Australia and adjacent islands.—*Kangaroos* and *wallabies* of any species, koala or native "bear," *wombat*, thylacine, dingo, "*Tasmanian devil*," phalangers or opossums, bandicoots, echidna or "spiny ant eater," and *platypus;* cassowaries, emeu, lyre bird, parrots, parrakeets, and cockatoos, fruit pigeons, megapod, brush turkey, black swan, birds of paradise, etc., and large snakes and lizards. The living platypus, or duckbill, is not represented in any American collection. It is somewhat difficult to procure and demands particular care as to quiet and special food in transport, but hardly any animal would excite so general an interest or reflect more credit on its collector.

New Zealand.—Kiwi or apteryx, owl parrot, parrots, and tuatara lizard.

BOXING.

General instructions.—The larger animals, all adult flesh-eating animals, and most other species that are not gregarious should each be given a separate box or compartment. The smaller monkeys may be shipped together, but the adults of the larger species are likely to be ill natured and should be shipped separately, as should also antelope, deer, and sheep, even though young. The young of most other animals may be shipped together.

An illustration of a large shipping box is given above, and detailed plans of the same are shown upon the opposite plate. Similar boxes varying in size according to the size of the animals to be transported can readily be made by any fairly good carpenter and blacksmith. All boxes should be high enough to allow the animals confined in them to stand erect. The inside should be smooth, all cleats, etc., required to strengthen the box being put on the outside. Care should be taken that no nails project inside. Except in metal-lined boxes, holes for ventilation should be bored in the upper part of the box. No cracks should be left near the bottom, as the animals would be liable to get their feet caught in them.

The space in front, between the grating and the bottom of the box, should be kept closed by a removable footboard, except when the animals are being fed or watered. The rear door should be kept locked, all feeding and cleaning being done through the other openings.

A plan is given in the same plate of an iron scraper, which is the handiest implement for cleaning out the cages. It should be of a size and weight to suit the cages in which it is to be used, the handle to be several inches longer than the cage.

These directions and the accompanying plan call for some materials that can only be had from a well-appointed hardware store. It is to be expected that in many cases such materials can not be obtained, and that the plans will then be varied to suit the occasion, stout bars of wood taking the place of iron rods, slats being used instead of wire netting, etc.

Lions, leopards, and other large cats.—The box should be a little longer than the animal and wide enough to permit it to turn around. The front end should be closed by a grating of $\frac{5}{8}$-inch vertical iron bars, 3 inches apart. A space of 3 inches should be left between this grating and the bottom of the box, so that a water pan and food may be passed in. There should also be a frame covered with stout wire netting to fit over the grating on the outside and prevent the animal from reaching out. The rear end should have an opening the full height of the box and wide enough to admit the animal. This should be fitted with a door sliding down from above. A little straw or other like material should be put in the box for bedding.

Bears.—Boxes for bears should be of the same style as for the large cats, but they should be stouter and have a lining of sheet iron.

Deer, antelope, sheep, and goats.—Boxes for animals having horns and hoofs should be long enough to permit their stepping back and forth, and of a width sufficient to permit them to stand comfortably, but *not to turn around.* The upper part of the front end should be made of slats an inch apart, and at the bottom should be a sliding door 6 inches high and the full width of the box. The sliding door at the back should be the full width of the box and high enough to admit the animal. Inside, across the bottom, thin cleats, to give secure footing, should be nailed about 6 inches apart. Sand should be scattered on the bottom, and over this a little straw. No cracks should be left between the boards except in front, as the horns might be caught in them.

Kangaroos may be boxed in much the same manner as antelope, but the boxes should be wide enough to allow the animals to turn around easily, and the rear door be only wide enough to admit the animal.

Hippopotamus and rhinoceros.—Boxes should be wide enough to allow the animal to sway from side to side, but not to turn around. The front end should be closed with a grating of 1-inch vertical bars, 5 inches apart. Two or three stout planks set

[illegible] inches space between, will answer if iron bars can not be had. [illegible] should be left between this grating and the bottom of the [illegible] should be a stout bar across the rear end, with 4 inches space between [illegible] for cleaning out. The rear door, sliding up, should fill the entire [illegible]. Cleats should be nailed across the bottom inside, a little sand [illegible] scattered on it, and over this a good bedding of straw or similar material. [illegible] should have the top, over the animal's head, lined with [illegible].

[illegible] Boxes for these animals should be similar to the last, only [illegible], and with bars 3 inches apart and with 3 inches space at bottom, in front and [illegible].

[illegible] Boxes should be large enough to allow the animals to move about. The front may be of heavy wire netting or a grating of light bars, with a space of 2 inches at the bottom for putting in water pan and food and for cleaning out. A piece of burlap or blanket should be tacked across the top edge in front, to be let down when the temperature is low or the box is being moved. A little bedding of straw or similar material should be put in.

Monkeys bear transportation best when several are sent together, but any that are vicious must be shipped by themselves.

Rodents.—Boxes for gnawing animals should be similar to those for monkeys, but lined with tin or sheet iron. Of most species it will be safe to put several individuals together.

Small cats, weasels, etc.—Boxes should be of sufficient size to permit free movement. Front should be closed with stout wire netting or light iron grating, a space being left at the bottom for food and water.

Birds.—Boxes for birds should have tight back and bottom. For the other three sides and top a frame covered with wire netting will answer, burlap or other cheap material being put over this to protect the birds from drafts. The front may be kept uncovered except when the box is being moved.

Boxes for long-billed birds should have a door in the front so that a pail or other deep vessel for water may be set inside.

Parrots and macaws require metal cages, as they quickly destroy wood.

Boxes for ostriches, emues, and cassowaries should be tight to a height of about 6 inches above the level of the bird's back. Above that point the sides, and also the top, should be made of slats, with the space between them so narrow that the bird can not put its head through. The sides of the box should be padded up to the level of the bird's breast.

There should be a sliding door in one end of the box sufficiently large to admit the bird, and this will also answer for putting in pail for water and feed.

Hay is most satisfactory for bedding, but other dry material of similar character will answer. This bedding material should be cut into short lengths; otherwise the bird will get it twisted about its feet.

Reptiles.—Snakes may be shipped in boxes having tight sides and bottom and top covered with small-mesh wire netting. An old blanket or some soft, dry grass should be put into the box, also a little sand.

Iguanas and other lizards requre the same style of box as snakes. Dry sand only should be put in the bottom.

FEEDING AND CARE.

General instructions.—It is not expected that the directions for feeding, given below, can always be closely followed. It may happen that the articles specified are not obtainable, and that others not mentioned, but equally good, can be had in abundance.

Whenever the animals to be shipped have been for some time in captivity it will be advisable to obtain full information regarding the manner of caring for them, and the food to which they have been accustomed.

Care should be taken that animals are not overfed; if they do not eat all that is given them the quantity should be reduced at once.

Such animals as antelope, deer, hippopotamus, etc., that have been fed on hay and grain, will relish fresh grass and other green food when it is obtainable, but it should be given in small quantities at first.

The cages should be cleaned each day, and all remnants of food should be removed as soon as the animals finish eating.

Water should be given twice a day. The pan should be taken out when the animal is through drinking. The pan is best put into the cage empty, then filled from a common sprinkling pot which has had the "rose" removed.

Any animals that die should of course be removed at once.

Flesh-eating animals.—An adult lion or tiger requires about 10 pounds of meat, including bone, once a day; a leopard or panther about 5 pounds; an ocelot or wild cat 2½ pounds; smaller animals in proportion. The meat should be given "on the bone" whenever possible, as this compels the animal to eat slowly.

The cheetah requires very careful feeding, and it will be best, before shipping, to secure a supply of live chickens or other fowls for its food while in transit.

Bears are omnivorous, and may be fed dry bread, biscuits, boiled rice with sugar, vegetables, and fruit. They do not require meat.

The civet cat, ichneumon, coati, and kinkajou require bread and milk, boiled rice, and milk with sugar, fruit, and a little meat. Small birds and mice will be relished.

Insect-eating animals.—Anteaters and armadillos should have boiled milk beaten up with raw egg. The great anteater will take from two to three pints, with two eggs, and the small species one pint and one egg. They should also have raw meat finely minced and entirely free from long fibers.

Vegetable-eating animals.—Antelope, deer, etc., require hay, oats, or dry bran and oats mixed, and a little green food can be used when obtainable.

Hippopotamus and rhinoceros should be given hay, fresh green food when obtainable, carrots, beets, and other roots. Mixed bran and oats should also be fed, to be moistened for the rhinoceros and thoroughly wet for the hippopotamus.

Tapirs require great care in feeding. Hay, straw, beets, or carrots cut up and mixed with bran may be fed, also boiled rice or potatoes, sweet potatoes or yams, bread, biscuits, boiled Indian corn with grass, cabbage leaves, and small branches of trees. Constant care and watchfulness will be required to arrange a diet upon which the animal will thrive.

Sloths require fruit, such as bananas, figs, etc., lettuce and other green food; also bread and milk.

Kangaroos may be fed on hay, with oats and bran, roots of all kinds, and apples; also green food, such as grass, cabbage leaves, and beet tops.

Rodents will eat green food, roots of any kind, apples, bread, biscuit, etc.

Birds.—Parrots of the larger kinds will eat Indian corn, oats, buckwheat, dry biscuits, apples, pears, grapes, and the various tropical fruits; also lettuce, cabbage leaves, and other green food. The smaller kinds require the same sort of food, except that millet, hemp, and canary seed should be used instead of the larger kinds.

Marsh and water birds will eat small fish, or larger fish cut into small strips; also fresh meat cut into small pieces.

Eagles, owls, and other birds of prey should have fresh meat and fish cut into strips. Live birds, mice, and rats should be given them occasionally when obtainable.

Ostriches, emeus, and cassowaries require beets, carrots, and other roots, cut into small pieces, cabbage leaves, lettuce, etc.; also a small quantity of oats and a very little corn. The food should be varied from day to day as much as possible.

Reptiles.—Some lizards are exclusively vegetable feeders, while others eat only insects, and in many cases it will be necessary to try them with different kinds of food in order to ascertain what they require. For the iguanas and others requiring vegetable food, lettuce, leaves of cabbage, mangrove, etc., and various fruits may be used. Many of the insectivorous species will eat cockroaches and ants. Eggs, both raw and hard boiled (minced finely), should be tried.

It will not be necessary to feed snakes while in transit, as most of them are able to go for a considerable time without food. It is well, however, where it can conveniently be done, to feed them just before shipping. They should be sprinkled with water once in every two or three days, when this can be done without wetting the blanket.

Animals in the National Zoological Park June 30, 1899.

Name.	Number.	Name.	Number.
MAMMALS.		MAMMALS—continued.	
North American species.		*North American species*—Continued.	
American bison (*Bison americanus*)	10	Collared peccary (*Dicotyles tajacu*)	1
Prong-horn antelope (*Antilocapra americana*)	4	Ocelot (*Felis pardalis*)	1
Virginia deer (*Cariacus virginianus*)	7	Puma (*Felis concolor*)	12
American elk (*Cervus canadensis*)	12	Spotted lynx (*Lynx rufus maculatus*)	3
		Gray wolf (*Canis lupus griseo-albus*)	4

Animals in the National Zoological Park June 30, 1899—Continued.

Name.	Number.
MAMMALS—continued.	
North American species—Continued.	
Black wolf (*Canis lupus griseo-albus*)	2
Coyote (*Canis latrans*)	6
Red fox (*Vulpes pennsylvanicus*)	5
Swift fox (*Vulpes velox*)	2
Gray fox (*Urocyon cinereo-argenteus*)	1
North American otter (*Lutra hudsonica*)	1
American badger (*Taxidea americana*)	3
Kinkajou (*Cercoleptes caudivolvulus*)	2
Gray coati-mundi (*Nasua narica*)	2
Common skunk (*Mephitis mephitica*)	1
Raccoon (*Procyon lotor*)	19
Black bear (*Ursus americanus*)	4
Cinnamon bear (*Ursus americanus*)	2
Grizzly bear (*Ursus horribilis*)	2
California sea lion (*Zalophus californianus*)	3
Harbor seal (*Phoca vitulina*)	2
Common pocket gopher (*Geomys bursarius*)	2
California pocket gopher (*Thomomys bottæ*)	2
American beaver (*Castor fiber*)	5
Hutia-conga (*Capromys pilorides*)	1
Woodchuck (*Arctomys monax*)	1
Prairie dog (*Cynomys ludovicianus*)	25
Red-bellied squirrel (*Sciurus aureogaster*)	1
Fox squirrel (*Sciurus niger*)	1
Gray squirrel (*Sciurus carolinensis*)	21
Mountain chipmunk (*Eutamias speciosus*)	18
Beechey's ground squirrel (*Spermophilus beecheyi*)	1
Yellow-headed ground squirrel (*Spermophilus brevicaudus*)	20
Antelope chipmunk (*Spermophilus leucurus*)	2
Canada porcupine (*Erethizon dorsatus*)	2
Mexican agouti (*Dasyprocta mexicana*)	2
Northern varying hare (*Lepus americanus*)	3
Rocky Mountain varying hare (*Lepus americanus bairdii*)	1
Peba armadillo (*Tatusia novemcincta*)	4
Opossum (*Didelphys virginiana*)	1
Domesticated and foreign species.	
Eskimo dog	3
Black spider monkey (*Ateles ater*)	1
Crested agouti (*Dasyprocta cristata*)	2
Hairy-rumped agouti (*Dasyprocta prymnolopha*)	2
Azara's agouti (*Dasyprocta azaræ*)	2
Acouchy (*Dasyprocta acouchy*)	3
Solid-hoofed pig (*Sus scrofa* var.)	1
Zebu (*Bos indicus*)	8
Yak (*Pœphagus grunniens*)	1
Barbary sheep (*Ovis tragelaphus*)	1
Common goat (*Capra hircus*)	16
Cashmere goat (*Capra hircus*)	3
MAMMALS—continued.	
Domesticated and foreign species—Cont'd.	
Nilgai (*Boselaphus tragocamelus*)	1
Indian antelope (*Antilope cervicapra*)	1
Axis deer (*Cervus axis*)	1
Sambur deer (*Cervus aristotelis*)	1
Common camel (*Camelus dromedarius*)	3
Llama (*Auchenia glama*)	4
Guanaco (*Auchenia huanacos*)	1
White-lipped peccary (*Dicotyles labiatus*)	1
South American tapir (*Tapirus americanus*)	1
Indian elephant (*Elephas indicus*)	2
Lion (*Felis leo*)	11
Tiger (*Felis tigris*)	2
Leopard (*Felis pardus*)	2
Spotted hyæna (*Hyæna crocuta*)	2
Wolf hound	2
Grey hound	1
Mastiff	1
St. Bernard dog	2
Pointer	2
Chesapeake Bay dog	2
Bedlington terrier	1
Smooth-coated fox terrier	3
Wire-haired fox terrier	1
Brown French poodle	1
Mongoose (*Herpestes mungo*)	1
Tayra (*Galictis barbara*)	1
Red coati-mundi (*Nasua rufa*)	4
Sun bear (*Ursus malayanus*)	1
Sloth bear (*Melursus labiatus*)	1
Fruit bat (*Pteropus medius*)	2
Malbrouck (*Cercopithecus cynosurus*)	1
Macaque monkey (*Macacus cynomolgus*)	4
Bonnet monkey (*Macacus sinicus*)	1
Gray capparo (*Lagothrix humboldti*)	1
Apella (*Cebus apella*)	1
Capuchin (*Cebus capucinus*)	3
Squirrel monkey (*Chrysothrix sciureus*)	1
Mongoose lemur (*Lemur mongoz*)	1
Albino rat (*Mus rattus*)	10
Crested porcupine (*Hystrix cristata*)	3
Guinea pig (*Cavia porcellus*)	12
English rabbit (*Lepus cuniculus*)	22
Six-banded armadillo (*Dasypus sexcinctus*)	
Gray kangaroo (*Macropus* sp.)	13
Bennett's red-necked wallaby (*Macropus ruficollis bennetti*)	1
Red kangaroo (*Macropus rufus*)	1
Brush-tailed rock kangaroo (*Petrogale penicillata*)	1
BIRDS.	
California towhee (*Pipilo fuscus crissalis*)	1
Clarke's nutcracker (*Nucifraga columbiana*)	2
California jay (*Aphelocoma californica*)	1

Animals in the National Zoological Park June 30, 1899—Continued.

Name.	Number.
BIRDS—continued.	
Road runner (*Geococcyx californianus*)	2
Sulphur-crested cockatoo (*Cacatua galerita*)	2
Leadbeater's cockatoo (*Cacatua leadbeateri*)	1
Bare-eyed cockatoo (*Cacatua gymnopis*)	1
Yellow and blue macaw (*Ara ararauna*)	2
Red and yellow and blue macaw (*Ara macao*)	2
Green paroquet (*Conurus* sp.)	1
Carolina paroquet (*Conurus carolinensis*)	3
White-fronted amazon (*Amazona leucocephala*)	1
Levaillant s amazon (*Amazona levaillanti*)	2
Salle's amazon (*Amazona ventralis*)	1
Gray parrot (*Psittacus erithacus*)	4
Great horned owl (*Bubo virginianus*)	7
Barred owl (*Syrnium nebulosum*)	6
Bald eagle (*Haliæëtus leucocephalus*)	11
Harpy eagle (*Thrasaëtus harpyia*)	1
Golden eagle (*Aquila chrysaëtos*)	2
Red-tailed hawk (*Buteo borealis*)	5
Pigeon hawk (*Falco columbarius*)	1
Turkey vulture (*Cathartes aura*)	2
Ring dove (*Columba palumbus*)	7
Chachalaca (*Ortalis vetula maccallii*)	5
Guan (*Penelope* sp.)	3
Crested curassow (*Crax alector*)	1
Lesser razor-billed curassow (*Mitua tomentosa*)	1
Peafowl (*Pavo cristatus*)	20
Valley partridge (*Callipepla californica vallicola*)	8
Mountain partridge (*Oreortyx pictus*)	5
Sandhill crane (*Grus mexicana*)	1
Whooping crane (*Grus americana*)	1
Green heron (*Ardea virescens*)	4
Great blue heron (*Ardea herodias*)	4
Black-crowned night heron (*Nycticorax nycticorax nævius*)	2
Whistling swan (*Olor columbianus*)	1
Mute swan (*Cygnus gibbus*)	7
Brant (*Branta bernicla*)	3
Canada goose (*Branta canadensis*)	3
Hutchins's goose (*Branta canadensis hutchinsii*)	1
Chinese goose (*Anser cygnoides*)	3
Mandarin duck (*Dendronessa galericulata*)	13
Pintail (*Dafila acuta*)	1
BIRDS—continued.	
Blue-winged teal (*Anas discors*)	2
Pekin duck (*Anas* sp.)	8
Mallard duck (*Anas boschas*)	2
Common duck (*Anas boschas*)	3
American white pelican (*Pelecanus erythrorhynchos*)	8
Florida cormorant (*Phalacrocorax dilophus floridanus*)	4
Snakebird (*Anhinga anhinga*)	2
American herring gull (*Larus argentatus smithsonianus*)	1
Cassowary (*Casuarius galeatus*)	2
REPTILES.	
Alligator (*Alligator mississippiensis*)	20
Snapping turtle (*Chelydra serpentina*)	1
Painted turtle (*Chrysemys picta*)	6
Musk turtle (*Aromochelys odorata*)	2
Mud turtle (*Cinosternum pennsylvanicum*)	5
Terrapin (*Pseudemys sp.*)	1
Gopher turtle (*Xerobates polyphemus*)	1
Tortoise (*Cistudo carolina*)	2
Duncan Island tortoise (*Testudo ephippium*)	2
Albemarle Island tortoise (*Testudo vicina*)	2
Banded basilisk (*Basiliscus vittatus*)	1
Comb lizard (*Ctenosaura sp.*)	2
Wislizen's lizard (*Crotaphytus wislizenii*)	3
Bailey's lizard (*Crotaphytus baileyi*)	1
Alligator lizard (*Sceloporus sp.*)	3
Skink-tailed lizard (*Gerrhonotus scincicauda*)	1
Dipsosaurus (*Dipsosaurus dorsalis*)	5
Gila monster (*Heloderma suspectum*)	
Diamond rattlesnake (*Crotalus adamanteus*)	2
Copperhead (*Ancistrodon contortrix*)	2
Water moccasin (*Ancistrodon piscivorus*)	2
Python (*Python sp.*)	3
Boa (*Boa constrictor*)	3
Yellow tree boa (*Epicrates inornatus*)	2
Anaconda (*Eunectes murinus*)	1
Bull snake (*Pituophis sayi*)	1
Pine snake (*Pituophis melanoleucus*)	1
King snake (*Ophibolus getulus*)	5
California chain snake (*Ophibolus boylii*)	2
Mountain black snake (*Coluber obsoletus*)	2
Garter snake (*Eutænia sirtalis*)	2
Water snake (*Natrix sipedon*)	5
Gopher snake (*Spilotes corais couperri*)	4

List of accessions for fiscal year ending June 30, 1899.

ANIMALS PRESENTED.

Name.	Donor.	Number of specimens.
Malbrouck	Mrs. J. E. Stone, Washington, D. C.	1
Capuchin	....do	1
Eskimo dog	Dr. H. T. Foote, New Rochelle, N. Y.	2
Red fox	Joseph Ligon Young, Washington, D. C.	1
Black bear	Mrs. Dorothy Sherrod Murphy, New York	1
Do	Miss Eleanor Hope Johnson, Chicago, Ill.	1
Common goat	C. C. Nelson, Congress Heights, District of Columbia	1
Hutia conga	Karl Decker, Washington, D. C	1
Pocket gopher	A. Turnbull, Washington, D. C.	1
Gray squirrel	Sterling Helmick, Washington, D. C.	1
Do	J. W. Jones, Washington, D. C.	1
Yellow-headed ground squirrel.	Perry A. Simons, Stanford University, California	1
Woodchuck	Mr. Huguely, Washington, D. C.	1
Do	J. M. C. Eaton, Irvington, N. J.	1
Guinea pig	Mrs. Burns, Washington, D. C.	2
English rabbit	Mrs. R. D. Hassler, Washington, D. C.	1
Do	Dr. L. O. Howard, Washington, D. C.	2
Angora rabbit	Miss Lillian Brooks, Washington, D. C.	1
White rabbit	C. B. Taylor, Washington, D. C.	2
Towhee	Dane Coolidge, Riverside, Cal.	1
Levaillant's Amazon	Captain Hall, Washington, D. C.	1
Great horned owl	E. W. Chesley, Gunston Wharf, Va.	1
Do	Dr. W. F. Hutchinson, Winchester, Va.	2
Do	Dr. G. W. Cook, Washington, D. C.	1
Barred owl	Dr. Lewis Johnson, Washington, D. C	1
Short-eared owl	Miss L. M. Milstead, Newington, Va.	1
Barn owl	Donor unknown	1
American osprey	A. M. Nicholson, Orlando, Fla.	1
Pigeon hawk	Dr. Roland Steiner, Grovetown, Ga	1
Bald eagle	Lieut. Com. M. B. Buford, U. S. N	1
Do	Maj. Gen. Nelson A. Miles, U. S. A	1
Do	Hon. R. A. Alger, Washington, D. C.	2
Do	J. F. Kelly	1
Do	R. G. Abbott, Washington, D. C.	1
Golden eagle	G. H. Tice, Monero, N. Mex.	2
Red-tailed hawk	Dr. W. F. Hutchinson, Winchester, Va	1
Do	John Grahe, Frederick, Md.	1
Do	Charles F. Brooke, Sandy Spring, Md	2
Guan	Col. Cecil Clay, Washington, D. C	3
Great blue heron	A. M. Nicholson, Orlando, Fla.	1
Roseate spoonbill	Karl Decker, Washington, D. C	1
Mandarin duck	Dr. Ishikawa, director of Zoological Gardens, Tokyo, Japan, through Dr. Alexander Graham Bell.	11
Alligator	Mrs. G. W. Driver, Washington, D. C.	1
Do	Sherwood Catlett, Washington, D. C	1
Do	Mrs. Mackay-Smith, Washington, D. C	1
Do	Mr. Craig, Washington, D. C.	1
Alligator lizard	Dane Coolidge, Riverside, Cal	3
Horned lizard	....do	6
Common lizard	Ralph Howell and Lee Cary, Washington, D. C.	5
Gila monster	W. W. Wilson, Casa Grande, Ariz.	1
Mountain boomer	A. B. Walker, Cameron, Ind. T	3
Diamond rattlesnake	Dr. J. J. Kinyoun, U. S. Marine Hospital Service	1

List of accessions for fiscal year ending June 30, 1899—Continued.

ANIMALS PRESENTED—Continued.

Name.	Donor.	Number of specimens.
Prairie rattlesnake	L. W. Purinton, Banner, Kans	2
Yellow tree-boa	Capt. C. G. Stevenson, U. S. V., New York	1
do	Capt. A. C. Hansard, Luquillo, Puerto Rico	1
Bull snake	L. W. Purinton, Banner, Kans	3
do	Dane Coolidge, Riverside, Cal	2
Mountain racer	....do	1
Hog-nosed snake	L. W. Purinton, Banner, Kans	1

RECEIVED FROM UNITED STATES OFFICERS ABROAD.

Gray capparo	Commander C. C. Todd, U. S. N	1
Capuchin	....do	2
Apella monkey	....do	1
Black spider monkey	....do	1
Squirrel monkey	....do	1
Ocelot	....do	1
Kinkajou	....do	1
Red coati-mundi	....do	1
Gray coati-mundi	....do	1
South American tapir	....do	1
White-lipped peccary	....do	1
Acouchy	....do	3
Yellow-thighed caique	....do	1
Harpy eagle	....do	1
Crested curassow	....do	1

ANIMALS LENT.

Macaque monkey	N. K. Sabler, Washington, D. C	1
Lion	Barnum and Bailey	2
Indian elephant	A. E. Randle, Congress Heights, District of Columbia	1
Zebu	Barnum and Bailey	2
Common goat	C. H. Roeder, Washington, D. C	6
Axis deer	Barnum and Bailey	1
White-fronted amazon	Miss Virginia Cull, Washington, D. C	1
Salle's amazon	Louis Hopfenmaier, Washington, D. C	1
Bald eagle	E. S. Schmid, Washington, D. C	1
Red-tailed hawk	....do	2
Python	R. G. Payne, Washington, D. C	1

ANIMALS RECEIVED IN EXCHANGE.

Macaque monkey	Donald Burns, New York	2
Do	E. S. Schmid, Washington, D. C	1
Tiger	Wm. Bartels, New York	1
Red coati-mundi	Donald Burns, New York	1
Harbor seal	J. H. Starin, Glen Island, New York	8
Fruit bat	Wm. Bartels, New York	2
Zebu	J. W. Smith, Central Park menagerie, New York	2
Nilgai	....do	1
Arabian camel	Board of public park commissioners, Baltimore	1
Do	Wm. Bartels, New York	1
Red kangaroo	....do	1

Animals purchased and collected.

Mongoose lemur (*Lemur mongoz*)	1
Leopard (*Felis pardus*)	1
Common skunk (*Mephitis mephitica*)	2
Red coati-mundi (*Nasua rufa*)	1
Sun bear (*Ursus malayanus*)	1
Sloth bear (*Melursus labiatus*)	1
Yak (*Poephagus grunniens*)	1
Barbary sheep (*Ovis tragelaphus*)	1
Indian antelope (*Antilope cervicapra*)	1
Prong-horn antelope (*Antilocapra americana*)	1
Sambur deer (*Cervus aristotelis*)	1
Arabian camel (*Camelus dromedarius*)	1
California pocket gopher (*Thomomys bottæ*)	2
Common pocket gopher (*Geomys bursarius*)	1
Chipmunk (*Eutamias speciosus*)	38
Yellow-headed ground squirrel (*Spermophilus brevicaudus*)	19
Beechey's ground squirrel (*Spermophilus beecheyi*)	17
Antelope chipmunk (*Spermophilus leucurus*)	2
Prairie dog (*Cynomys ludovicianus*)	22
Eugene Island kangaroo (*Macropus eugenii*)	1
Bennett's red-necked wallaby (*Macropus ruficollis bennetti*)	2
California jay (*Aphelocoma californica*)	7
Road runner (*Geococcyx californianus*)	3
Barred owl (*Syrnium nebulosum*)	2
Golden eagle (*Aquila chrysaëtos*)	1
Valley partridge (*Callipepla californica vallicola*)	10
Mountain partridge (*Oreortyx pictus*)	12
Sand-hill crane (*Grus americana*)	1
Green heron (*Ardea virescens*)	4
Great blue heron (*Ardea herodias*)	2
Brant (*Branta bernicla*)	1
Pintail (*Dafila acuta*)	1
Blue-winged teal (*Anas discors*)	2
Mallard (*Anas boschas*)	2
Florida cormorant (*Phalacrocorax dilophus floridanus*)	4
Snake bird (*Anhinga anhinga*)	4
Cassowary (*Casuarius galeatus*)	2
Duncan Island tortoise (*Testudo ephippium*)	2
Albemarle Island tortoise (*Testudo vicina*)	2
Wislizenius's lizard (*Crotaphytus wislizenii*)	3
Bailey's lizard (*Crotaphytus baileyi*)	1
Spotted-tailed dragon (*Callisaurus ventralis*)	13
Skink-tailed lizard (*Gerrhonotus scincicauda*)	1
Dipsosaurus (*Dipsosaurus dorsalis*)	5
Glass snake (*Ophiosaurus ventralis*)	1
California rattlesnake (*Crotalus lucifer*)	1
Bull snake (*Pituophis sayi*)	2
California chain-snake (*Ophibolus boylii*)	1

Animals born in the National Zoological Park.

Lion (*Felis leo*)	4
Puma (*Felis concolor*)	7
Wild-cat (*Lynx rufus maculatus*)	1

Gray wolf (*Canis lupus griseo-albus*)	3
Red coati-mundi (*Nasua rufa*)	1
Zebu (*Bos indicus*)	1
Common goat (*Capra hircus*)	2
Prong-horn antelope (*Antilocapra americana*)	3
American elk (*Cervus canadensis*)	4
Virginia deer (*Cariacus virginianus*)	1
Collared peccary (*Dicotyles tajaçu*)	2
Gray kangaroo (*Macropus sp.*)	1
Peafowl (*Pavo cristatus*)	5
European swan (*Cygnus gibbus*)	5

SUMMARY.

Animals on hand July 1, 1898	549
Accessions during the year	401
	950
Deduct losses (by exchange, death, and returning of animals)	275
On hand June 30, 1899	675

Respectfully submitted.

FRANK BAKER, *Superintendent.*

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

RACOON IN ZOOLOGICAL PARK

Appendix V.

REPORT OF THE WORK OF THE ASTROPHYSICAL OBSERVATORY FOR THE YEAR ENDING JUNE 30, 1899.

SIR: I prefix to the usual report an approximate statement of the present condition of the Astrophysical Observatory:

1. The character and value of property in the possession of the Observatory—

(*a*) Buildings	$6,300
(*b*) Apparatus	27,700
(*c*) Library and records	5,200
Total value of Observatory property	39,200

2. Accessions and improvements of property in the period covered by this report—

(*a*) *Buildings.*—A corner of the main building has been enlarged and fitted up to form a more commodious office and library, and a new laboratory building consisting of one story and basement has been built. These additions were made from last year's appropriation, in accordance with authorization of Congress, at the cost of $2,300. The fence surrounding the Observatory has been enlarged and entirely repainted, at a cost of $100; and repairs to plumbing made, at a cost of $25. Total for buildings and repairs, $2,425.

(*b*) *Apparatus.*—Expenditures for astronomical and physical apparatus, $800. The cooling plant and the automatic temperature-regulating system have been extended, at a cost of $500. Fire-extinguishing apparatus has been procured, at a cost of $100. Miscellaneous expenditures for apparatus, $100. Total for apparatus, $1,500.

(*c*) *Library.*—Books, periodicals, and illustrations have been procured, at an expenditure of $200. Total accessions to value of property,[1] $4,000.

3. Losses of property—

These have been trifling, and consist in wear and tear and accidental breakage to the extent of $50.

The Work of the Observatory.

The work which has occupied the staff of the Observatory during the past year has been very varied. The observations made have been in new fields, though to some extent subsidiary to those of previous years. Much attention has been given to preparing results for publication, and it is gratifying to be able to state that Volume I of the Annals of the Astrophysical Observatory is at last in the hands of the printer.

For convenience in describing the work in detail, it may be divided into three categories, as follows:

A. Observations.

B. Preparation for publication.

C. Miscellaneous matters.

A.—Observations.

The observations themselves may be considered under two heads:

1. On the dispersion of rock salt.
2. On miscellaneous subjects.

[1] About half of this amount was chargeable to last year's appropriation.

(1) On the dispersion of rock salt.

This subject has been investigated by several observers, first by yourself at Allegheny about 1886, and since then by Julius, Paschen, Rubens and Snow, and others. The object of making a further study of it was to very materially increase the accuracy of determination in the infra-red portion of the spectrum, with the hope of establishing the wave-lengths of the infra-red solar absorption lines discovered at this Observatory to a degree of accuracy corresponding with the accuracy of our knowledge of their minimum deviations in the spectrum of our great salt prism. There appeared to be strong grounds for hope of succeeding in this endeavor, in the consideration of the extraordinary facilities of the Observatory for such a research.

Accordingly, the apparatus was made ready in July and August of 1898, and was actually tried in the latter part of August. Active work was, however, deferred until the latter part of December, and was done chiefly in January and February of 1899. While the method and results will be fully described in the forthcoming publication, a brief statement will be appropriate here.

The radiations of the sun were used as the source of energy up to a wave-length of 4μ, but from this point to 6.5μ (where the research was stopped partly because the grating used was no longer applicable, and partly because further progress was of no particular interest) the radiations of an iron gauze mantle heated in a Kitson lamp[1] were employed. The radiations, from whichever source, fell first upon a slit 10 centimeters high, then upon a concave diffraction grating, and then upon the slit of the spectro-bolometer, which remained practically as used in taking solar bolographs. The grating apparatus being mounted according to Rowland's well-known device, radiations whose wave-lengths were multiples of each other fell upon the slit of the spectro-bolometer, and passed through to have their prismatic deviations determined. For instance, if the apparatus was adjusted so that the well-marked line of wave-length 0.5616μ in the fourth-order spectrum was found by visual observation falling at the center of the slit, then it would be certain that radiations of the following multiples of this wave-length also fell there: 4/3, 4/2, and 4/1. The two latter would be well within the infra-red.

Suppose, now, the driving clock of the spectro-bolographic apparatus to be started, and a curve automatically produced just as a bolograph would be. The form of this curve would be a straight line only broken by the minute accidental deflections of the galvanometer, except where the narrow bands of radiation at the above wave-lengths caused narrow, steep-sided elevations. If we suppose, still further, that either before or after this curve was made the direct sunlight was reflected upon the slit of the spectro-bolometer for several minutes, then a well-known portion of the solar spectrum energy curve would appear in its proper relative position on the same plate, and the positions of all the sharp elevations corresponding to known wave-lengths could readily be measured on the comparator with reference to determined solar absorption lines of the short bolographs. Thus the wave-lengths at as many points as desired could be determined without any circle readings whatever.

Practically, this process (somewhat altered in details) was gone through with for 38 positions between wave lengths 0.76μ and 6.5μ; and not only once, but several times, with all the care for accuracy which could be taken. As a result, it may be said that the wave lengths of the absorption lines in the infra-red solar spectrum discovered at this observatory can be told with an accuracy of about 3 parts in 10,000, while previous to this determination 1 part in 100 would have been all that could be claimed.

[1] This lamp, which burns vaporized petroleum oil, was very kindly placed by the makers at the disposal of the Institution for the purpose.

(2) *Observations on miscellaneous subjects.—Distribution of energy from terrestrial sources.*

A number of energy curves, some of which are here given, were taken, in which the Kitson lamp, provided with mantles of various kinds, was the source. Among the mantles tried were the ordinary Welsbach (which consists of impure thorium oxide) and others composed of pure thorium oxide, iron oxide, uranium oxide, etc.[1] The distribution of energy between different wave lengths with these sources, so different in illuminating power, is much less diversified than would be supposed, and goes strongly to show the wastefulness even of the Welsbach light as a source of illumination. For the invisible infra-red in all cases includes by far the major portion of the energy, and not the visible spectrum, as is the case with the sun and still more with phosphorescent substances. However, by the employment of a second spectroscope, or "sifting train," to exclude the stray infra-red radiations, we were able to determine the distribution of the relatively small amount of energy in the visible spectra of the various sources, and to show how far the ordinary Welsbach mantle outstripped them all for light, especially in the red, orange, and yellow.

In these experiments a very considerable number (at least fifty) of absorption bands were discovered at wave lengths beyond 4μ, which were most probably due to the gases given off by the lamp in burning, and perhaps solely to carbon dioxide.

Absorption in the solar spectrum.—All the bolographic records, extending back to 1893, were carefully examined with regard to the changes in absorption noted in last year's report, and such changes were found to be more extensive and frequent than had been supposed. The great decreases in absorption at Ψ and Ω were found to occur every spring, and to a lesser extent every fall; but were occasionally found in the winter also, but never in the summer. Such changes were found sometimes to go through their whole cycle in a week, and the absorption here is found to largely increase with declining sun.

The effect of water to absorb in this region was studied. Narrow cells of glass, whose absorption was known, were filled with water and placed in the path of the beam in taking bolographs of the solar spectrum. It was thus shown that "liquid" water absorbs most strongly at the particular regions where these annual variations are noticed. A fraction of a millimeter thickness of water was found enough to produce a great effect beyond the wave length 1.2μ, and 2 millimeters thickness produced almost complete absorption of the solar rays beyond 1.2μ.

Constants of prisms.—In the course of the reduction of measures on bolographs the exact index of refraction at the A line for the salt and fluorite prisms was required. Several measurements of these quantities made at different times failing of satisfactory agreement, several interesting things came out in finding the sources of error.

Rock salt prisms have not constant angles. It was shown that with a rising temperature the faces of the prism, if at first flat, became convex, and all three angles of the prism increase, unless they be determined at the centers of the faces. After the discovery of the very appreciable value of this change with our great prisms, it was at once found that their faces, when polished flat, are considerably concave after coming to their constant temperature. This is because of the surface heating caused by the friction of the polisher. It is now the practice to leave the faces polished very slightly convex, to allow for this temperature change.

Effect of this on the accuracy of bolographs and on the determination of prism angles. After a very considerable amount of analytical investigation it was shown that such concavity of prism faces as was present when the bolographs were taken need introduce no error of appreciable magnitude in the relative deviations of the bolographs, provided the angle of the prism was determined to within 10 seconds of

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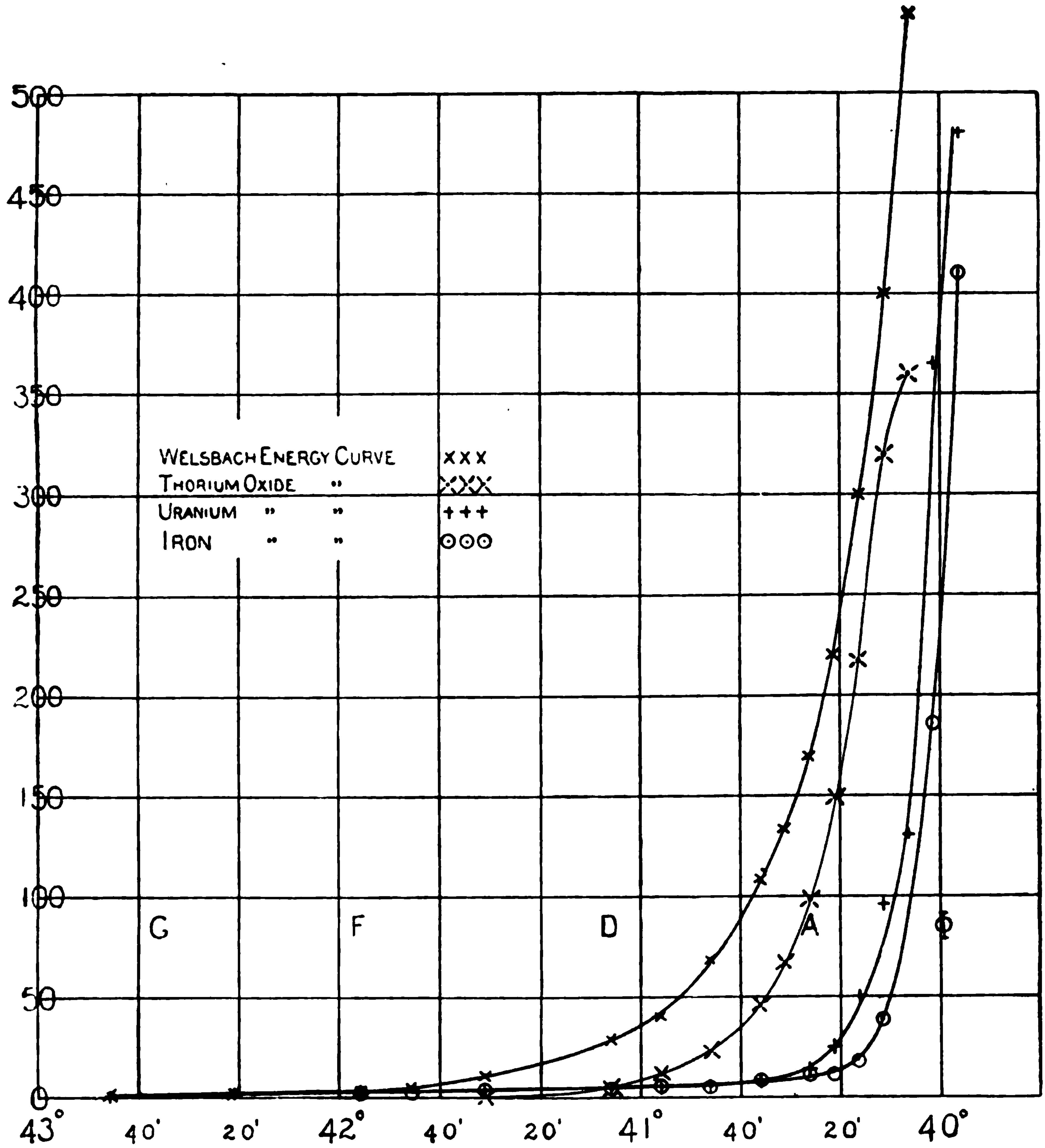
[1] The Observatory is indebted to Mr. Waldron Shapleigh, chemist of the Welsbach Company, for valuable advice and material used in this research.



BOLOMETRIC CURVES, SHOWING THE DISTRIBUTION OF ENERGY FROM WELSBACH AND OTHER HEATED MANTLES.

Ordinates are galvanometer deflections. Abscissæ are deviations in the spectrum of a 60° rock-salt prism.

arc at a point a little nearer the back of the prism than the center of the faces, and this was done in practice. However, in determining absolute quantities like the index of refraction at A, it was found necessary to be far more particular.

Measurement of refractive indices at A.—Diaphragms just wide enough for visual resolution of A were placed symmetrically on the faces, and the prism was so placed on the prism table that exactly the same beam of light entered the diaphragm in one position of the prism as in the other, in measuring both the angle and the minimum deviation. In this way very excellent accord was obtained between several series of measures, and the following constants were fixed for the refractive indices of rock salt and fluorite in air at 20° C. and 760 mm. pressure. Average wave length of radiations, 0.7604 μ.

For rock salt $n = 1.536818 \pm .000009$
For fluorite $n = 1.431020 \pm .000006$

Do all rock-salt prisms have the same dispersion? We were led to believe the affirmative upon this very important question by recorded results from many prisms, but we have conclusive evidence in the following comparison of the dispersion of three salt prisms, two from Russian and one from Bavarian salt, between wave lengths 0.4 μ and 4.0 μ. The results indicated the affirmative, for the differences in the refractive indices in all this range never exceeded the probable experimental error of determination. To be more precise, the results at A were as follows:

Prism R. B. I. $n = 1.536818 \pm .000009$
Prism R. B. II. $n = 1.536844 \pm .000006$
Prism S. P. L. T. $n = 1.536812 \pm .000005$

At other points the differences were of the same order of magnitude.

It follows then, as you have anticipated and elsewhere pointed out, that this most interesting crystal, whose optical application from the time of Melloni to the commencement of these observations has been chiefly qualitative as a transmitter of special radiations, can now be used quantitatively with practical convenience in the form of a 60° prism, as a standard of refraction to which all wave lengths may be referred with the same order of precision as to the grating.

The temperature coefficient of refractive indices for rock-salt prisms.—Bolographs were taken at low and high constant temperatures, and from these in connection with former results the temperature coefficient for the whole range of radiations covered by our bolographs was accurately determined.

Comparison of the efficiency of the bolometer and thermopile.—It will be recalled that the thermopile has recently been made far more delicate and efficient by improvements of Rubens, so that with him and with some others it has displaced the bolometer for radiation work. A comparison made here between one of these instruments and our bolometer, No. 20, shows the latter, though of only one-fifteenth the surface, to give twice the deflection at the galvanometer when substituted for the thermopile. The galvanometer was besides more free from "drift" and "wiggle" with the bolometer, and there was no "creep" to the deflection with it, while such "creep" lasted 5 or 10 seconds with the thermopile. The bolometer has besides the advantage that it can be made more strictly linear and far narrower than the thermopile, and is capable of exact setting in the spectrum. To offset these advantages, the thermopile requires no battery or balancing coils, and costs but about one-thirtieth as much as the bolometer with its necessary accessories. Nevertheless, to make it equal to the bolometer as regards "wiggle" and "drift" and capacity for accurate setting it would require a mounting at least a fourth as costly as the bolometer and its accessories. On the whole the bolometer has the advantage, except in cost.

B.—PREPARATION FOR PUBLICATION.

Owing to the great addition in new matter, and to recent alterations in apparatus and methods, much new copy had to be added to the manuscript previously prepared, and all had to be revised.

Nearly 60,000 comparator measurements of various kinds were reduced through various stages. Several large scale plots were made and used in connection with the comparison of the dispersion of rock salt and fluorite, and with the determination of wave lengths in the rock salt, fluorite, and glass prismatic spectra.

Illustrations consisting of bolographs, photographs, plots, and spectrum maps have been gotten ready for reproduction, and the whole has been sent to the engraver.

C.—MISCELLANEOUS MATTERS.

Apparatus.—Among the several pieces of new apparatus which have been procured are two deserving of special attention. These are the new rock-salt prism "R B. II" and the new bolometer case.

The salt for this prism was obtained from the salt mines of Russia through the efforts and courtesies of officers of both the Russian and United States Governments. The prism was cut and polished by Brashear, and in its final form can not but be a source of great gratification, for with the exception of a few tiny bubbles within, it is clear of flaw or defect of any kind, and forms a great 60° prism, 15 centimeters on the edge and 17.5 centimeters high, as clear and perfect as the mind could conceive.

The new bolometer case, in which every accessory of the bolometer but the battery and galvanometer is combined in very moderate compass, required considerable shop-work after its arrival, owing to the carelessness of the maker, but is now in a most satisfactory condition and is a great advance on any previous form.

Proposed new galvanometer.—Despite the success of efforts to increase the sensitiveness of the bolometric apparatus, there still remains much to be desired, for we see and photograph objects by radiations so feeble as to be still utterly beyond the powers of even our latest heat-measuring apparatus. Experience suggests the possibility that still further advances may be made by increasing the available sensitiveness of the bolometric apparatus. This will be understood when it is said that if our present galvanometer were absolutely free from magnetic and mechanical disturbances it could easily be made to indicate electric currents six hundred times as small as could now be distinguished with certainty. Designs have been prepared and are already nearly executed whereby this great improvement to the galvanometer is hoped to be realized.

Renovation of apparatus.—The siderostat and other apparatus were thoroughly overhauled and renovated late in the year 1898.

The new building.—Great want of room has been felt at the Observatory, owing to the introduction of constant-temperature chambers, cooling apparatus, and other bulky impedimenta. The new building of one story and basement is designed to be used for miscellaneous measurements before crowded out or done with great difficulty for lack of room. A very stable pier is one of the features of this building.

Meeting of committee of astronomers and physicists.—Much pleasure was felt in extending the use of the new library and office room to a committee which drafted the constitution of the American Astronomical and Astrophysical Society, February 8, 1899.

Personnel.—No changes have occurred in the permanent Observatory staff. Mr. C. E. Mendenhall closed his temporary connection with it September 1, 1899.

CONCLUSION.

In conclusion it may be said that the researches of the present year have well finished our investigation of the infra-red solar spectrum by the accurate determination

of the wave lengths involved. While our knowledge of the infra-red still remains less complete than that of the visible spectrum, both in the number of absorption lines mapped and in the accuracy of determination of their wave lengths, yet the difference in the methods of observation must be recalled. On the one hand are the most powerful gratings with all the advantages of direct photography, while on the other is only a simple prism, in whose dark spectrum we grope for cold lines and measure their wave lengths indirectly. The results of the latter process are 750 lines determined in wave lengths to an accuracy of 3 parts in 10,000, and besides—what photography does not give—an exact knowledge of the distribution of the sun's energy.

Respectfully submitted.

C. G. ABBOT,
Aid Acting in Charge Astrophysical Observatory.

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution, Washington, D. C.

APPENDIX VI.

REPORT OF THE LIBRARIAN FOR THE YEAR ENDED JUNE 30, 1899.

SIR: I have the honor to present herewith the report upon the operations of the library of the Smithsonian Institution during the fiscal year ended June 30, 1899.

The following statement shows the number of volumes, parts of volumes, pamphlets, and charts received between July 1, 1898, and June 30, 1899:

	Quarto or larger	Octavo or smaller.	Total.
Volumes	493	868	1,361
Parts of volumes	20,781	10,165	30,946
Pamphlets	454	1,390	1,844
Charts	....	....	181
Total	....	....	34,332

The accession numbers run from 390,914 to 413,772 in the record book.

The additions to the libraries of the Secretary, the office, and the Astrophysical Observatory number 318 volumes and pamphlets and 1,985 parts of volumes, making a total of 2,303 and a grand total of 36,663 accessions for the year.

In accordance with the general plan for the increase of the library 845 letters were written for new exchanges and for completing series already in the library, as a result 241 new periodicals and serials were added to the list and 408 defective series were either completed or added to as far as the publishers could supply the missing parts.

Since the removal of the Library of Congress to its new building, and the better facilities therein for the care of books, it has been found possible to send a much greater portion of the Smithsonian library to the Library of Congress than heretofore, and this proportion may be expected to be increased in the future.

A special room has been set apart in the Institution for assembling prints and books relating to the fine arts. It is hoped that this room will be suitably fitted up during the coming year.

An account of the operations of the Museum library is presented in connection with the reports of the Museum. This collection is constantly growing in value, but in both buildings the space as well as the number of assistants are entirely inadequate to meet the growth of recent years.

The small circulating library established for the employees of the Institution has been catalogued and arranged and has been a source of much pleasure and instruction.

I attended the second conference on an international catalogue of scientific literature held in London October 11-13, 1898, as the representative of the United States Government.

Very respectfully,

CYRUS ADLER, *Librarian.*

Mr. S. P. LANGLEY,

Secretary of the Smithsonian Institution.

Appendix VII.

REPORT OF THE EDITOR.

Sir: I have the honor to submit the following report on the publications of the Smithsonian Institution for the year ending June 30, 1899:

The Institution proper publishes three series of works: The Contributions to Knowledge, in quarto, and the Miscellaneous Collections and Annual Report, in octavo. The Contributions and Collections are printed in limited editions, at the expense of the Smithsonian fund, for distribution to the principal libraries and learned institutions of the world, while the Report is printed by order of Congress in large editions for general distribution. So great is the demand for the Report, however, that it is impracticable to have an individual list of recipients, but the volume is distributed to libraries in principal centers, and so widely scattered that the books may be available to the greatest number of persons.

Under the general direction of the Institution are also published the Proceedings and Bulletin of the National Museum, the Museum volume of the Smithsonian Report, and the Annual Report and Bulletin of the Bureau of American Ethnology; and through the Secretary of the Institution there are transmitted to Congress the Annual Report of the American Historical Association and the Report of the National Society of the Daughters of the American Revolution. The Institution has, however, no copies of either of the last two documents at its disposal, except that a small number of the historical reports are available for exchange with historical societies at home and abroad.

I. CONTRIBUTIONS TO KNOWLEDGE.

No memoir of the Contributions was completed during the year.

II. MISCELLANEOUS COLLECTIONS.

Three works in the series of collections were issued, entitled First Supplement of Bibliography of Chemistry, Index to Literature of Thallium, and Index to Literature of Zirconium.

1170. A Select Bibliography of Chemistry, 1892–1897. By H. Carrington Bolton. First Supplement. Octavo. IX, 489 pages.

In the preface the author describes this work as follows:

The Select Bibliography of Chemistry, 1492–1892, was published in 1893; this First Supplement includes works omitted in that volume and brings the literature of chemistry down to the close of the year 1897. In the following pages the lines of the original work have been followed, the term chemistry being taken in its fullest significance, the range of topics will be seen in the Subject-Index and their distribution in the table on page VII. This supplement does not embrace academic dissertations, a catalogue of which is nearly ready for the press.

As in the first volume, the titles are grouped in sections with a view to facilitating reference: I, Bibliography; II, Dictionaries; III, History; IV, Biography; V, Chemistry, Pure and Applied; VII, Periodicals. Section VI, Alchemy, has been dropped. The scope of each section is explained in the first volume, and it need only be here pointed out that in each (excepting those of biography and periodicals) the titles are arranged alphabetically by authors, translations of each work following the original in the alphabetical order of the English names of the languages. The

order is the same as in the table on page vii. In the Section of Biography the titles are placed under the names of the persons described, with cross references from the authors.

1171. Index to the Literature of Thallium, 1861–1896, by Martha Doan. Octavo pamphlet of 26 pages.

1173. Index to the Literature of Zirconium, by A. C. Langmuir and Charles Baskerville. Octavo pamphlet of 29 pages.

III. SMITHSONIAN ANNUAL REPORTS.

The Smithsonian Reports for 1896 and 1897, except the 1897 Museum volume, were distributed during this fiscal year, their completion having been unavoidably delayed. The separate papers of the Smithsonian volume of the 1896 report were enumerated in detail in the last report of the editor, as also were the general contents of the 1897 volume. Neither the 1898 Smithsonian volume nor the 1897 and 1898 Museum volumes of the report had been completed at the close of the year.

1166. Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution for the year ending June 30, 1896. Report of the United States National Museum. Washington: Government Printing Office, 1898. 8°. xxiv+1,107 pages, with 175 plates.

1167. Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution to July, 1897. Washington: Government Printing Office, 1898. 8°. xlvii+686 pages, with 92 plates.

IV. PAPERS FROM SMITHSONIAN REPORTS.

1088. Report of S. P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1897. Octavo pamphlet of 80 pages, with 5 plates.

1127. Journal of Proceedings of the Board of Regents of the Smithsonian Institution. Report of executive committee. Acts and resolutions of Congress. (From the Smithsonian Report for 1897.) Octavo pamphlet of 38 pages.

1128. Aspects of American Astronomy, by Simon Newcomb. (From the Smithsonian Report for 1897.) Octavo pamphlet of 11 pages.

1129. The Beginnings of American Astronomy, by Edward S. Holden. (From the Smithsonian Report for 1897.) Octavo pamphlet of 8 pages.

1130. The Evolution of Satellites, by G. H. Darwin. (From Smithsonian Report for 1897.) Octavo pamphlet of 16 pages.

1131. Electrical Advance in the Past Ten Years, by Elihu Thomson. (From the Smithsonian Report for 1897.) Octavo pamphlet of 12 pages.

1132. The X-Rays, by W. C. Röntgen. (From Smithsonian Report for 1897.) Octavo pamphlet of 19 pages.

1133. Cathode Rays, by J. J. Thomson. (From Smithsonian Report for 1897.) Octavo pamphlet of 12 pages with 3 plates.

1134. Story of Experiments in Mechanical Flight, by S. P. Langley. (From Smithsonian Report for 1897.) Octavo pamphlet of 13 pages, with 1 plate.

1135. On Soaring Flight, by E. C. Huffaker, with introduction by S. P. Langley. (From Smithsonian Report for 1897.) Octavo pamphlet of 24 pages.

1136. The Revival of Alchemy, by H. C. Bolton. (From Smithsonian Report for 1897.) Octavo pamphlet of 11 pages.

1137. Diamonds, by William Crookes. (From Smithsonian Report for 1897.) Octavo pamphlet of 17 pages.

1138. The Discovery of New Elements within the last Twenty-five Years, by Clemens Winkler. (From Smithsonian Report for 1897.) Octavo pamphlet of 10 pages.

1139. An Undiscovered Gas, by William Ramsay. (From Smithsonian Report for 1897.) Octavo pamphlet of 12 pages.

1140. Fluorine, by Henri Moissan. (From Smithsonian Report for 1897.) Octavo pamphlet of 14 pages.

1141. Light and its Artificial Production, by O. Lummer. (From Smithsonian Report for 1897.) Octavo pamphlet of 27 pages.

1142. Explorations of the Upper Atmosphere, by Henri de Graffigny. (From Smithsonian Report for 1897.) Octavo pamphlet of 16 pages, with 3 plates.

1143. The Explorations of the Free Air by means of Kites at Blue Hill Observatory, by A. Lawrence Rotch. (From Smithsonian Report for 1897.) Octavo pamphlet of 8 pages, with 3 plates.

1144. The Debt of the World to Pure Science, by John Stevenson. (From Smithsonian Report for 1897.) Octavo pamphlet of 12 pages.

1145. The Age of the Earth as an Abode Fitted for Life, by Lord Kelvin. (From Smithsonian Report for 1897.) Octavo pamphlet of 21 pages.

1146. Rising of the Land around Hudson Bay, by Robert Bell. (From Smithsonian Report for 1897.) Octavo pamphlet of 9 pages.

1147. Crater Lake, Oregon, by J. S. Diller. (From Smithsonian Report for 1897.) Octavo pamphlet of 11 pages, with 16 plates.

1148. The Function and Field of Geography, by J. Scott Keltie. (From Smithsonian Report for 1897.) Octavo pamphlet of 19 pages.

1149. Letters from the Andrée Party. (From Smithsonian Report for 1897.) Octavo pamphlet of 12 pages, with 7 plates.

1150. Scientific Advantages of an Antarctic Expedition, by John Murray and others. (From Smithsonian Report for 1897.) Octavo pamphlet of 16 pages.

1151. Recent Progress in Physiology, by Michael Foster. (From Smithsonian Report for 1897.) Octavo pamphlet of 16 pages.

1152. The Factors of Organic Evolution from a Botanical Standpoint, by L. H. Bailey. (From Smithsonian Report for 1897.) Octavo pamphlet of 23 pages.

1153. The Law which underlies Protective Coloration, by Abbott H. Thayer. (From Smithsonian Report for 1897.) Octavo pamphlet of 6 pages, with 5 plates.

1154. Life History Studies of Animals, by L. C. Miall. (From Smithsonian Report for 1897.) Octavo pamphlet of 24 pages.

1155. The Royal Menagerie of France, and the National Menagerie established on the Fourteenth of Brumaire of the Year II (November 4, 1793), by E. T. Hamy. (From Smithsonian Report for 1897.) Octavo pamphlet of 11 pages.

1156. Botanical Opportunity, by William Trelease. (From Smithsonian Report for 1897.) Octavo pamphlet of 18 pages.

1157. Mescal: A New Artificial Paradise, by Havelock Ellis. (From Smithsonian Report for 1897.) Octavo pamphlet of 12 pages.

1158. The Unity of the Human Species, by Marquis de Nadaillac. (From Smithsonian Report for 1897.) Octavo pamphlet of 21 pages.

1159. Recent Research in Egypt, by W. M. Flinders-Petrie. (From Smithsonian Report for 1897.) Octavo pamphlet of 5 pages.

1160 A Study of the Omaha Tribe: The Import of the Totem, by Alice C. Fletcher. (From Smithsonian Report for 1897.) Octavo pamphlet of 10 pages, with 3 plates.

1161. A New Group of Stone Implements from the Southern Shores of Lake Michigan, by W. A. Phillips. (From Smithsonian Report for 1897.) Octavo pamphlet of 14 pages, with 10 plates.

1162. A Preliminary Account of Archæological Field Work in Arizona in 1897, by J. Walter Fewkes. (From Smithsonian Report for 1897.) Octavo pamphlet of 23 pages, with 23 plates.

1163. The Building for the Library of Congress, by Bernard R. Green. (From Smithsonian Report for 1897.) Octavo pamphlet of 8 pages, with 13 plates.

1164. Francis Amasa Walker, by George F. Hoar and Carroll D. Wright. (From Smithsonian Report for 1897.) Octavo pamphlet of 19 pages.

1169. Report of S. P. Langley, secretary of the Smithsonian Institution, for the year ending June 30, 1898. Octavo pamphlet of 89 pages, with 3 plates.

Report upon the condition and progress of the U. S. National Museum during the year ending June 30, 1896, by G. Brown Goode, assistant secretary of the Smithsonian Institution, in charge of the U. S. National Museum. (From Museum volume of Smithsonian Report for 1896.) Octavo pamphlet of 284 pages, with 4 plates.

An account of the U. S. National Museum, by Frederick W. True, executive curator. (From Museum volume of Smithsonian Report for 1896.) Octavo pamphlet of 38 pages.

Prehistoric Art: or the Origin of Art as Manifested in the Works of Prehistoric Man, by Thomas Wilson, curator, division of prehistoric archæology. (From Museum volume of Smithsonian Report for 1896.) Octavo pamphlet of 340 pages and 74 plates.

Chess and Playing Cards: Catalogue of games and implements for divination exhibited by the U. S. National Museum in connection with the department of archæology and paleontology of the University of Pennsylvania at the Cotton States and International Exposition, Atlanta, Georgia, 1895. By Stewart Culin, director of the museum of archæology and paleontology, University of Pennsylvania. (From Museum volume of Smithsonian Report for 1896.) Octavo pamphlet of 278 pages and 50 plates.

Biblical Antiquities: A description of the exhibit at the Cotton States and International Exposition, Atlanta, 1895. By Cyrus Adler, custodian, section of historic religious ceremonials, and I. M. Casanowicz, aid, division of historic archæology. (From Museum volume of Smithsonian Report for 1896.) Octavo pamphlet of 81 pages, with 46 plates.

The Lamp of the Eskimo, by Walter Hough. (From Museum Volume of Smithsonian Report for 1896.) Octavo pamphlet of 32 pages, with 24 plates.

V. NATIONAL MUSEUM PUBLICATIONS.

Proceedings of the U. S. National Museum, Volume XX, published under the direction of the Smithsonian Institution, Washington, Government Printing Office, 1898. 8°. xii + 932 pages, with 97 plates.

The contents of this volume were enumerated in the editor's report for last year.

The following separate papers, comprising Volume XXI of the Proceedings, were distributed during the year in pamphlet form, but the completed volume was not issued:

Proc. 1140. Contributions Toward a Monograph of the Lepidopterous Family Noctuidæ of Boreal North America. A Revision the Species of Acronycta (ochsenheimer) and of certain Allied Genera. By John B. Smith and Harrison G. Dyar. 8°. pp. 1–194, with 22 plates.

Proc. 1141. Descriptions of the Species of Cycadeoidea, or Fossil Cycadean Trunks, thus far determined from the Lower Cretaceous Rim of the Black Hills, by Lester F. Ward. 8°. pp. 195–229.

Proc. 1142. On Some New Parasitic Insects of the Subfamily Encyrtinæ, by L. O. Howard. 8°. pp. 231–248.

Proc. 1143. On the Coleopterous Insects of Galapagos Islands, by Martin L. Linell (deceased). 8°. pp. 249–268.

Proc. 1144. The Birds of the Kuril Islands, by Leonhard Stejneger. 8°. pp. 269–296.

Proc. 1145. Description of a Species of Actaeon from the Quarternary Bluffs at Spanish Bight, San Diego, California, by Robert E. C. Stearns. 8°. pp. 297–299.

Proc. 1146. Report on a Collection of Japanese Diptera, presented to the U. S. National Museum by the Imperial University of Tokyo, by D. W. Coquillett. 8°. pp. 301-340.

Proc. 1147. Notes on the Mammals of the Catskill Mountains, New York, with General Remarks on the Fauna and Flora of the Region, by Edgar A. Mearns. 8°. pp. 341-360, with 6 figures.

Proc. 1148. Topaz Crystals in the Mineral Collection of the U. S. National Museum, by Arthur S. Eakle. 8°. pp. 361-369, with 22 figures.

Proc. 1149. Notes on Cytherea (Tivela) crassatelloides Conrad, with Descriptions of many varieties, by Robert E. C. Stearns. 8°. pp. 371-377, with 3 plates.

Proc. 1150. On the Occurrence of Amphiuma, the so-called Congo Snake, in Virginia, by Hugh M. Smith. 8°. pp. 379, 380.

Proc. 1151. Description of a new species of Spiny-tailed Iguana from Guatemala, by Leonhard Stejneger. 8°. pp. 381-383.

Proc. 1152. Cambrian Brachiopoda: Obolus and Lingulella, with Descriptions of new species, by Charles D. Walcott. 8°. pp. 385-420, with 3 plates.

Proc 1153. A Revision of the Wrens of the Genus Thryomanes Sclater, by Harry C. Oberholser. 8°. pp. 421-450.

Proc. 1154. American Oniscoid Diplopoda of the Order Merocheta, by O. F. Cook. 8°. pp. 451-468, with 4 plates.

Proc. 1155. The Osteology and Relationships of the Family Zeidæ, by Edwin Chapin Starks. 8°. pp. 469-476, with 6 plates.

Proc. 1156. A Contribution to the Knowledge of the variations of the Tree Frog Hyla regilla, by Frederick Cleveland Test. 8°. pp. 477-492, with 1 plate.

Proc. 1157. Japanese Hymenoptera of the Family Tenthredinidæ, by C. L. Marlatt. 8°. pp. 493-506.

Proc. 1158. A Contribution to a Knowledge of the Fresh-water Crabs of America. The Pseudothelphusinæ. By Mary J. Rathbun. 8°. pp. 507-537, with 17 figures.

Proc. 1159. Notes on a Collection of Fishes from Mexico, with Description of a new species of Platypœcilus, by Barton A. Bean. 8°. pp. 539-542, with 1 figure.

Proc. 1160. The Leeches of the U. S. National Museum, by J. Percy Moore. 8°. pp. 543-563, with 1 plate.

Proc. 1161. On the Occurrence of Caulolepis longidens Gill, on the Coast of California, by Charles Henry Gilbert. 8°. pp. 565, 566.

Proc. 1162. The Brachyura collected by the U. S. Fish Commission steamer *Albatross* on the voyage from Norfolk, Virginia, to San Francisco, California, 1887-1888. By Mary J. Rathbun. 8°. pp. 567-616, with 4 plates.

Proc. 1163. On the Nomenclature of the Whalebone Whales of the Tenth Edition of Linnæus's Systema Naturæ, by Frederick W. True. 8°. pp. 617-635.

Proc. 1164. A New Snake from the Eocene of Alabama, by F. A. Lucas. 8°. pp. 637, 638, with 2 plates.

Proc. 1165. Notes on the Capture of Rare Fishes, by Barton A. Bean. 8°. pp. 639, 640.

Proc. 1166. The Feather-tracts of North American Grouse and Quail, by Hubert Lyman Clark. 8°. pp. 641-653, with 3 plates and 4 figures.

Proc. 1167. Note on Oxycottus acuticeps (Gilbert) from Sitka and Kadiak, Alaska, by Tarleton H. Bean and Barton A. Bean. 8°. pp. 655, 656.

Proc. 1168. African Diplopoda of the Genus Pachybolus, by O. F. Cook. 8°. pp. 657-666, with 3 plates.

Proc. 1169. The Diplopod Family Striariidæ, by O. F. Cook. 8°. pp. 667-676, with 2 plates.

Proc. 1170. African Diplopoda of the Family Gomphodesmidæ, by O. F. Cook. 8°. pp. 677-739, with 7 plates.

Proc. 1171. Hydroida from Alaska and Puget Sound, by Charles Cleveland Nutting. 8°. pp. 741-753, with 3 plates.

Proc. 1172. The Fossil Bison of North America, By Frederic A. Lucas. 8°. pp. 775–771, with 20 plates and 2 figures.

Proc. 1173. Petrographic Report on Rocks from the United States-Mexico Boundary, by Edwin C. E. Lord. 8°. pp. 773–782, with 1 plate.

Proc. 1174. The Land Reptiles of the Hawaiian Islands, by Leonhard Stejneger 8°. pp. 783–813, with 13 figures.

Proc. 1175. Key to the Isopods of the Pacific Coast of North America, with Descriptions of Twenty-two new Species, by Harriet Richardson. 8°. pp. 815–869, with 34 figures.

Proc. 1176. Description of a New Species of Subterranean Isopod, by W. P. Hay. 8°. pp. 871–872, with 1 plate.

Proc. 1177. Synopsis of the Recent and Tertiary Leptonacea of North America and the West Indies, by William H. Dall. 8°. pp. 873–897, with 2 plates.

Proc. 1178. Description of a new Genus and Species of Discoglossoid Toad from North America, by Leonhard Stejneger. 8°. pp. 899–901, with 1 plate.

Bulletin of the U. S. National Museum, No. 47. The Fishes of North and Middle America: a descriptive catalogue of the species of fish-like vertebrates found in the waters of North America, north of the Isthmus of Panama. By David Starr Jordan and Barton Warren Evermann. Parts II and III, 8°, pp. xxx, 1241–2183, xxiv, 2183a–3136. Part I of this Bulletin was published in 1896. The complete work will include an atlas of plates.

VI. BUREAU OF AMERICAN ETHNOLOGY.

No publications were completed by the Bureau of Ethnology during the year, the seventeenth, eighteenth, and nineteenth reports being in various stages of printing or preparation.

VII. ASTROPHYSICAL OBSERVATORY.

There was transmitted to the Public Printer near the close of the fiscal year the manuscript and drawings of Volume I, Annals of the Astrophysical Observatory, to be printed in quarto form in the general style of the Smithsonian Contributions to Knowledge. Some progress was made in engraving the illustrations, but no proof of the text was received.

VIII. NATIONAL ZOOLOGICAL PARK.

The Zoological Park issued the following pamphlet: Animals Desired for the National Zoological Park at Washington, District of Columbia, United States of America. Washington: Government Printing Office, 1899. Imperial octavo, pp. 16, with a map, 11 plates, and 14 text figures.

IX. AMERICAN HISTORICAL ASSOCIATION.

The Annual Report of the American Historical Association for the year 1897 was completed during the year, and the report for 1898 was transmitted to the Public Printer. The contents of the 1897 volume were enumerated in the last report by the editor. The 1898 volume contains the following papers:

Report of Proceedings of Fourteenth Annual Meeting in New Haven, Connecticut, December 28–30, 1898, by Herbert B. Adams, secretary.

Report of the treasurer. List of committees and officers.

Inaugural address by Prof. G. P. Fisher, president of the association, on the Function of the Historian as a Judge of Historic Persons.

The Historical Manuscripts in the Library of Congress, by Herbert Friedenwald.

American Colonial History (1690–1750), by C. M. Andrews.

Study of American Colonial History, by H. L. Osgood.

A Forgotten Danger to the New England Colonies, by Frank Strong.
An Examination of Peters's "Blue Laws," by W. F. Prince.
The Connecticut Gore Land Company, by Albert C. Bates.
The Society of Separatists of Zoar, Ohio, by George B. Landis.
Southern Economic History: Tariff and Public Lands, by J. C. Ballagh.
Diplomatic Relations of the Confederate States with England (1861–1865), by J. M. Callahan.
American Diplomacy, by Edwin A. Grosvenor.
Lessons from the Recent History of European Dependencies, by Henry E. Bourne.
The Constitutional Questions Incident to the Acquisition and Government by the United States of Island Territories, by Simeon E. Baldwin.
Germans in America, by Ernest Bruncken.
The Real Origin of the Swiss Republic, by William D. McCrackan.
Erasmus, the Prince of the Humanists, by George Norcross.
The Cambridge School of History, by Mary R. W. Stubbert.
Municipal Government in the Twelfth Century, by John M. Vincent.
The Study of History in Schools. Report of the Committee of Seven to the American Historical Association.
Historical Manuscripts Commission. Third Report.

X. DAUGHTERS OF THE AMERICAN REVOLUTION.

The act of incorporation of the National Society of the Daughters of the American Revolution, approved by the President February 26, 1896, requires "That said society shall report annually to the Secretary of the Smithsonian Institution concerning its proceedings, and said Secretary shall communicate to Congress such portions thereof as he may deem of national interest and importance."

In accordance with the foregoing act, the Secretary of the Institution submitted to Congress the report of the Daughters of the American Revolution, 1890 to 1897, and in the Senate it was referred to the Committee on Education and Labor and ordered to be printed. The report has been issued as Senate Doc. No. 164, Fifty-fifth Congress, third session, and forms a pamphlet of 129 pages, with 34 plates. The contents include the act of incorporation, list of officers, constitution and by-laws, work of the Continental Congress and national board of management, work of the chapters, and several appendixes.

Respectfully submitted.

A. Howard Clark, *Editor.*

Mr. S. P. Langley,
Secretary of the Smithsonian Institution.

Appendix VIII.

REPORT OF THE REPRESENTATIVE OF THE SMITHSONIAN INSTITUTION AND NATIONAL MUSEUM, TRANS-MISSISSIPPI AND INTERNATIONAL EXPOSITION, OMAHA, NEBRASKA, 1898.

Sir: I have the honor to submit the following report on the Trans-Mississippi and International Exposition, held at Omaha, Nebr., from June 1 to October 31, 1898:

The act of Congress approved June 10, 1896, authorizing a display of the resources of the Government at the exposition, provided for participation by the Smithsonian Institution, and the sum of $24,088.81 was allotted by the Government board of management for the expenses involved in the preparation, transportation, and maintenance of the exhibit.

All dependencies of the Institution were represented by displays at the exposition.

The space assigned to the Institution was located in the central part of the Government building, and comprised about 4,000 square feet. The frontage on the main aisle was about 84 feet and the depth 42 feet. The exhibit was planned to give a general idea of the scope and character of the Institution and its several bureaus. For obvious reasons, the activities of the National Museum were more thoroughly illustrated than those of other bureaus.

Though the exhibits selected were as far as possible of interest intrinsically, the main object was to illustrate methods of work rather than to display exhaustive collections, which, indeed, would have been impossible in view of the limited space available. The methods employed by the Institution in the classification, mounting, and labeling, and general installation of objects of diverse kinds were fully shown. The cases—with the exception of those specially built for the exhibit of the Smithsonian Institution proper, and the stationary wall cases—were of uniform dimensions (units), and finished in every instance in a substantial and attractive style. A harmonious color scheme was maintained throughout the entire display.

SMITHSONIAN INSTITUTION PROPER.

The exhibit of the Smithsonian Institution proper, occupying a quadrant under the dome of the building, was as follows:

A complete set of the publications of the Institution and its bureaus. (In a special upright case.)

A cast of the bronze tablet recently placed on the tomb of Smithson in Genoa, Italy. (In front of the same case.)

Personal relics of Smithson: Photographic copies of the title pages of his more important papers; the seal of the Institution. (In table cases.)

A picture of the Smithsonian building. (In a table case.)

Framed portraits of the Secretaries of the Smithsonian Institution: Joseph Henry (1846–1878), Spencer Fullerton Baird (1878–1887), Samuel Pierpont Langley (elected 1887).

Copies of the history of the first half century of the Smithsonian Institution in different bindings; title page and illustrations for the same. (In a table case.)

Objects and papers relating to the Hodgkins fund: Copies of publications; Hodgkins medals in silver and bronze (portrait of Thomas G. Hodgkins, who bequeathed the fund to the Smithsonian Institution in 1891 for use in promoting researches on atmospheric air). (In a table case.)

Two large photographs of the ærodrome of Professor Langley.

The exhibit of the Smithsonian Institution proper was selected, under the direction of the Secretary, by Assistant Secretary Richard Rathbun.

BUREAU OF AMERICAN ETHNOLOGY.

This Bureau exhibited three large panels of illustrations selected from its annual reports, showing the scope of the work. A set of the Bureau's reports was included in the case containing the publications of the Smithsonian Institution and its dependencies.

The exhibit was assembled by Mr. W J McGee, ethnologist in charge, under the supervision of Maj. J. W. Powell, Director of the Bureau.

At the Indian congress held in conjunction with the exposition a miniature Kiowa Indian camping circle was exhibited by the Bureau, under the charge of Mr. James Mooney, who collected the material for the same.

NATIONAL ZOOLOGICAL PARK.

The exhibit of the National Zoological Park consisted of a model of the park, showing its topography, wooded areas, animal houses, and inclosures; water-color and pen-and-ink sketches of the animal houses and of picturesque points in the park, and transparencies from photographs of characteristic features of the park. This exhibit was assembled by Dr. Frank Baker, superintendent of the park.

BUREAU OF INTERNATIONAL EXCHANGES.

This Bureau exhibited a map showing the distribution of its correspondents throughout the world, and a large diagram indicating the growth of the service by decades since the year 1850.

The exhibit was prepared by Mr. W. I. Adams, chief clerk of the Bureau, under the direction of the assistant secretary of the Institution.

ASTROPHYSICAL OBSERVATORY.

The exhibit of the Astrophysical Observatory was selected and prepared under the direction of the Secretary of the Institution. The following objects were included:

Photographs of the exterior and interior of the Observatory building; the bolometer, or electric thermometer, an instrument of extreme delicacy, the invention of Professor Langley; photographs of other instruments in the Observatory, such as the siderostat, galvanometer and spectrometer; and enlarged photographs of the spectrum of the sun.

NATIONAL MUSEUM.

In the organization of the exhibit of the National Museum two principal objects were kept in view: First, to indicate the comprehensiveness of the scope of the Museum; second, to represent the manner in which series of objects are arranged, labeled, and displayed in the Museum halls at Washington.

In carrying out the first idea only an outline could be presented owing to lack of space. As regards the second, it should be remarked that the cases, fittings, and labels employed were for the most part from the regular stock of the Museum and were of exactly the same style as those used in Washington. A few new methods regarded as improvements were introduced.

The Museum is divided into three departments, each of which is again subdivided into divisions and sections. The exhibits are here given by departments.

DEPARTMENT OF ANTHROPOLOGY.—The exhibit of this department of the Museum was planned and prepared by Mr. W. H. Holmes, head curator, assisted by the scientific staff.

The exhibit occupied nineteen cases nearest the rotunda of the Government building. The principal functions of this department of the Museum are to preserve and

study the varied phenomena of human culture and especially to present to the public by means of exhibits the leading facts of human effort and progress.

Much attention was given to the native American peoples and culture, and to the history of the United States, but a symmetrical presentation of race history calls for illustrations from the whole field of anthropology, and all times and all races were made to contribute.

The group of exhibits presented at the exposition was intended to illustrate the achievements of the race along a few of the more important lines of activity. Each series of objects epitomized the subject treated and presented the leading steps of progress in the simplest possible manner.

The subjects illustrated were as follows:

Fire making and illumination.—The discovery of the use of fire and the making of fire by artificial means was illustrated by a single series of objects. The story began with the fire of volcanoes and lightning, followed by the kindling and keeping of fire, and closed with the utilization of the electric spark. Illumination was represented by two series: (1) The torch, and (2) the lamp.

Tools of general use.—The tools and utensils employed by men in the various arts were arranged in series, beginning with the simplest and ending with the highest forms. Tools of general use were illustrated in eight series, as follows: The hammer, the ax (American), the ax (European), the adz, the knife, the saw, the drill, the scraper.

Weapons.—Weapons have performed an important part in the history of man and culture, and the steps that lead up from the stone and club held in the hand to the steel sword and the machine gun were illustrated by two series of objects: (1) Weapons for use in the hand—piercing and slashing weapons; (2) projectile weapons—the bow and arrow, the pistol and the gun.

Exploitative arts.—Of the various exploitative or material-acquiring activities so necessary to the sustenance of the race, only one group—the art of fishing—was illustrated. The four series shown were as follows: (1) the dart, (2) the toggle, (3) the hook, (4) the sinker.

Domestic arts.—The domestic arts were represented by one series illustrating the development of cooking arts, and by three series showing the table utensils employed in eating and drinking—the cup, the spoon, the knife, and fork. The development of the tobacco pipe was shown in this connection.

The great group of elaborative activities concerned in manufacture, was illustrated by three exhibits: the ceramic art, the textile art, and sculpture.

Ceramic art.—Ceramics included four series: (1) implements and devices employed in manufacture: (2) the vase, (3) glass, (4) enamel.

Textile art.—Weaving had three series: (1) the spindle, (2) the shuttle, (3) the loom.

Sculpture, stone-shaping.—Sculpture was represented by four series: (1) prehistoric stone-shaping (Europe), (2) aboriginal American sculpture, (3) sculpture of civilized nations, (4) implements employed in stone-shaping.

The book.—A small series was devoted to the history of the book, and the method of assembling the several parts—the tablets or writing sheets being the feature considered.

Musical instruments.—Four series were devoted to the development of as many varieties of musical instruments: (1) Wind instruments, (2) reed instruments, (3) stringed instruments, (4) percussion instruments.

Photography.—Photography was represented by three interesting series, showing steps of progress: (1) The camera, (2) the lens, (3) the picture.

Transportation.—The history of water transportation was epitomized in four of its leading features: (1) The development of the hull, (2) methods of hand propulsion, (3) the wheel, (4) the screw propeller.

The subject of land transportation was partially shown in three series: (1) The burden bearer and the sliding and the rolling load, (2) the wheeled vehicle, (3) the steam locomotive.

Electricity.—Electrical inventions, representing one of the youngest and most marvelous branches of activity, were shown in three series: (1) experimental apparatus, (2) transmitting apparatus, (3) recording apparatus.

Groups of figures illustrating practice of primitive arts.—Associated with the development of exhibits were a number of life-sized figures, modeled in plaster and appropriately costumed, intended to illustrate the practice of the arts in their primitive stages. They give a vivid impression of primitive processes as contrasted with the methods and machinery of advanced civilization. The subjects presented were as follows:

The driller.—An Eskimo man in reindeer-skin costume using a bow drill for boring ivory.

The flint flaker.—A Powhatan Indian roughing out stone implements.

The hominy huller.—A southern Indian woman pounding corn in a wooden mortar. Figure in plaster with costume restored from drawings made in Colonial times.

The skin dresser.—Sioux woman using a scraping or graining tool.

The potter.—Papago Indian woman modeling an earthen vessel, simple process.

The metal worker.—A Navaho Indian making silver ornaments; process probably, in part at least, introduced by whites.

The belt weaver.—A Zuñi girl with primitive loom weaving a belt.

Department of Biology.—The exhibit of this department of the Museum was planned and prepared by Dr. F. W. True, head curator, assisted by the scientific staff.

The department of biology covers the entire field of zoology and botany. In selecting a topic for illustration in the small space available, two ideas were kept in view: First, to present a series of objects significant in itself and at the same time likely to be of especial interest to visitors to the exposition; and, second, to have this series sufficiently diversified in character to show the various methods employed in the Department.

The exhibit comprises the characteristic animals of the marine and fresh waters of North America, from the lowest to the highest forms, and the principal types of seaweeds.

Lower invertebrates.—Three cases were devoted to the lower invertebrates, such as crustaceans, worms, star-fishes, and other echinoderms, jelly-fish, corals, and other coelenterates, and foraminifera, and other protozoans. These were arranged in zoological order, from the lower to the higher forms. The corals and other low forms were in a special case near the side entrance.

Mollusks.—A floor case in the regular series was devoted to typical forms of North American shells, chiefly marine.

Insects.—Aquatic and semiaquatic insects occupied the adjoining wall.

Fishes.—A series of painted casts representing the principal families of North American fishes occupied one-half of a long wall-case. About seventy families in all were represented. Included with the casts was a small series of skeletons of the lowest forms of fishes.

Reptiles and batrachians.—The principal types of North American aquatic reptiles and batrachians, such as the turtles, terrapin, water snakes, frogs, salamanders, etc., were exhibited in the wall-case. Casts of the larger sea turtles were exhibited on the wall directly above the case. These were the leatherback turtle, the green turtle, and the loggerhead.

Birds.—A large series included the principal aquatic birds of North America. They were arranged in zoological order as nearly as circumstances permitted in the wall-case.

Mammals.—The following porpoises were represented by full-sized casts from life. The harbor porpoise (*Phocaena phocaena*), the black-fish (*Globicephalus melas*), the grampus (*Grampus griseus*), and the common dolphin (*Delphinus delphis*).

Seaweeds.—Aquatic plants were represented by seaweeds, a full series of the principal American types of which were exhibited in two cases on the central aisle.

DEPARTMENT OF GEOLOGY.—The exhibit of this department was planned and arranged by Dr. George P. Merrill, head curator, assisted by the scientific staff.

The exhibit was planned and arranged to convey an idea to the public of the scope of the department. To this end the space was divided in equal portions between the divisions of systematic geology, mineralogy, paleontology, and paleobotany.

Division of systematic and applied geology.—In this division were two cases filled with examples of cave deposits and concretionary structures, and five cases containing minerals, rocks, and ores of economic value, including a characteristic series of the iron ores of the United States, and a systematic series of ores of the minor metals, including those of mercury, nickel, and cobalt, antimony, bismuth, and others of the rarer metals. Also a collection of nonmetallic minerals of economic importance, including various salts used in chemical manufacture, abrasives, fictile materials, asbestus, mica, mineral pigments, graphite, coals, and other natural hydrocarbon compounds.

Division of mineralogy.—The exhibit of this division treated of the systematic arrangement and chemical classification of the several representatives of the mineral kingdom.

The series was grouped under two classes—elements and compounds of elements. The compounds of the elements were further divided in accordance with chemical laws and grouped under certain prominent types according to, and which take their names from, their more negative constituents, as follows: Compounds of the halogens, fluorides, chlorides, bromides, and iodides; compounds of sulphur, selenium, and tellurium; also arsenic, antimony, and bismuth, including sulphides, selenides, and tellurides; arsenides, antimonides, and bismuthides, sulpharsenides, and sulphantimonides; also sulphosalts, oxygen compounds, including oxides and the oxygen salts, borates, aluminates, chromites ferrites, manganites, selenites and tellurites, carbonates and tantalates, nitrates, vanadates, phosphates, arsenates, selenates and tellurates, chromates; molybates, tungstates, iodates, and uranadates. Each of these groups was preceded by a descriptive label giving the name of the type and a brief description of its more prominent character. Following the descriptive label, arranged in order from left to right, were several representatives of the type selected, so far as possible, to illustrate the character of the group as a whole.

Section of invertebrate fossils.—In one case was shown the largest American ammonites, a group of chambered shells related to the living Pearly Nautilus. Another case was devoted to crinoids, a group of animals related to the starfishes. The following two cases were occupied by trilobites, crustaceous animals which became extinct shortly after the great coal series of the Mississippi Valley had been formed. Still another case was devoted to lampshells, or brachiopods. The interior structure of some of these shells was illustrated by models showing, among the characters, the spiral skeleton for the support of the arms.

The crinoids, trilobites, and brachiopods were arranged in sytematic order, and the series of specimens representing each general group was preceded by a descriptive label. All these fossils were mounted on uniform buff-colored encaustic floor tiles, a method of installation recently introduced into the National Museum.

Section of vertebrate fossils.—In this section was shown a series of specimens of some of the earlier forms of fishes, and a series of teeth and spines of Carboniferous sharks. These two series were accompanied by illustrations showing the forms of fishes when restored, and the position of the spines and their relations to the fins. Specimens of the fishes from the Eocene formation of Green River and of Mosasaur reptiles from the chalk formation of western Kansas were also exhibited.

The Smithsonian Institution, the National Museum and its departments of anthro-

pology, biology, and geology, the Bureau of American Ethnology, the National Zoological Park, and the Astrophysical Observatory were awarded commemorative bronze medals and accompanying diplomas "for interesting and instructive exhibits," and 25 members of the administrative and scientific staffs of the Smithsonian Institution and its dependencies were awarded individual commemorative bronze medals and accompanying diplomas "for valuable services rendered" in planning, preparing, and installing exhibits.

Mr. W. V. Cox, chief special agent for the Smithsonian Institution and National Museum, was secretary of the Government board of management.

The following is a classified statement of the expenditures of the funds allotted to the Smithsonian Institution, corrected to July 15, 1900:

Summary of allotments made to the Smithsonian Institution.

Original allotment	$20,000.00
Transfer from Interior Department (April 1, 1898)	700.00
Transfer from common fund	1,000.00
Amount necessary to close account	1,888.81
	24,088.81

Classified statement of the expenditures of the funds allotted to the Smithsonian Institution.

Services		$9,984.25
Special services		753.70
Travel:		
Railroad fare	$1,411.49	
Sleeping-car fare	413.00	
Subsistence	692.00	
Incidental expenses	65.52	
		2,582.01
Freight		999.24
Cartage		293.40
Expressage		99.15
Exhibition cases, frames		1,666.00
Lumber and millwork		505.78
Hardware, etc		141.00
Glass, paints		1,446.16
Supplies and preparators' material		431.90
Packing material		103.38
Specimens, etc		2,293.34
Decorations		172.25
Office and miscellaneous expenses		3.20
		21,474.76
Transfer to building fund		1,708.33
Transfer to common fund		905.72
		2,614.05
		24,088.81

Respectfully submitted,

FREDERICK W. TRUE,
Representative, Smithsonian Institution and National Museum,
Trans-Mississippi and International Exposition.

Mr. S. P. LANGLEY,
Secretary, Smithsonian Institution.

GENERAL APPENDIX

TO THE

SMITHSONIAN REPORT FOR 1899.

ADVERTISEMENT.

The object of the GENERAL APPENDIX to the Annual Report of the Smithsonian Institution is to furnish brief accounts of scientific discovery in particular directions; reports of investigations made by collaborators of the Institution, and memoirs of a general character or on special topics that are of interest or value to the numerous correspondents of the Institution.

It has been a prominent object of the Board of Regents of the Smithsonian Institution, from a very early date, to enrich the annual report required of them by law with memoirs illustrating the more remarkable and important developments in physical and biological discovery, as well as showing the general character of the operations of the Institution; and this purpose has, during the greater part of its history, been carried out largely by the publication of such papers as would possess an interest to all attracted by scientific progress.

In 1880 the Secretary, induced in part by the discontinuance of an annual summary of progress which for thirty years previous had been issued by well-known private publishing firms, had prepared by competent collaborators a series of abstracts, showing concisely the prominent features of recent scientific progress in astronomy, geology, meteorology, physics, chemistry, mineralogy, botany, zoology, and anthropology. This latter plan was continued, though not altogether satisfactorily, down to and including the year 1888.

In the report for 1889 a return was made to the earlier method of presenting a miscellaneous selection of papers (some of them original) embracing a considerable range of scientific investigation and discussion. This method has been continued in the present report, for 1899.

THE WAVE THEORY OF LIGHT: ITS INFLUENCE ON MODERN PHYSICS.[1]

By A. CORNU.[2]

Our era is distinguished from preceding ages by wonderful utilization of natural forces; man, that weak and defenseless being, has been enabled by his genius to acquire an extraordinary power, and to bend to his use those subtle yet dreadful agents whose very existence was unknown to our ancestors. This marvelous increase of his material power in modern times is due only to the patient and profound study of natural phenomena, to the exact knowledge of the laws that governed them, and to the skillful combining of their effects. But what

[1] Printed in Nature July 27, 1899, with the following notes: "We are glad to be able to publish this week a translation of the Rede lecture delivered at Cambridge by Prof. Alfred Cornu, professor of experimental physics in the École polytechnique, Paris, and a foreign member of the Royal Society, on the occasion of the recent celebration of the jubilee of Sir George Stokes as Lucasian professor of mathematical physics. Professor Cornu delivered the lecture in French, and we are indebted to him for the translation of his brilliant discourse."

[2] Besides the interest presented by a glance on the progress and the influence of optical science, this lecture offers the conclusions of a careful study on Newton's treatise of optics. It will be seen that the thought of the great physicist has been singularly altered by a sort of legendary interpretation developed in the elementary treatises where the emission-theory is expounded. In order to make the theory of fits clearer, the commentators have imagined to materialize the luminous molecule under the form of a rotating arrow offering now its head, now its side. This mode of exposition has contributed to lead to the belief that the whole emission-theory was comprehended in this rather childish image.

Nowhere in his treatise does Newton give a mechanical illustration of the luminous molecule: he confines himself to the description of facts, and sums them up in an empirical statement without any hypothetical explanation. Moreover, he denies the opinion that he raises any theory, though he holds occasionally as very probable the intervention of the waves excited in the ether.

So that the general impression resulting from the reading of the treatise, and above all of the "queries" in the third book, is the following: Newton, far from being the adversary of the Cartesian system, as he is commonly represented, looks, on the contrary, very favorably at the principles of this system. Struck by the resources which the undulatory hypothesis would offer for the explanation of the luminous phenomena, he would have adopted it, if the grave objection concerning the rectilinear propagation of light (only recently solved by Fresnel) had not prevented him.

is peculiarly instructive is the disproportion between the primitive phenomenon and the greatness of the effects which industry has drawn from it. Thus, those formidable engines, based on electricity or steam, grew neither from lightning nor the volcano; they had their birth from scarcely perceptible phenomena which would have remained forever hidden from the vulgar eye, but that penetrating observers were able to recognize and appreciate. This humble origin of most of the great discoveries which are to-day a benefit to the whole human race, shows us plainly that the scientific spirit is at present the mainspring of the life of nations, and that it is in the onward march of pure science that we are to look for the secret of the growing power of the modern world. Whence a series of questions which demand more and more the attention of all. How did this taste toward the study of natural philosophy, so dear to the ancient philosophers, abandoned for centuries, again revive and grow? What are the phases of its advance? How appeared the new notions which have so deeply modified our ideas on the mechanism of nature's forces? What paths, rich in discoveries, lead us gradually unawares to those admirable generalizations in accordance with the vast plan forseen by the founders of modern physics? These are the questions which as a physicist I intend to inquire into before you. The subject is rather abstract, I might say severe. But no other has seemed more worthy of your attention during the fête which the University of Cambridge celebrates to-day in honor of the Lucasian Professorship Jubilee of Sir Geo. Gabriel Stokes, who in his fine career has laid a master hand on the very problems which seemed to me the most conducive to the progress of natural philosophy. The subject is all the more fitted here, as in citing the names of those great minds to whom modern science is most indebted, we found amongst those who most honored the University of Cambridge—its professors and fellows—Sir Isaac Newton, Thomas Young, George Green, Sir George Airy, Lord Kelvin, Clerk Maxwell, Lord Rayleigh, and the memory of that glory which links to-day back through the centuries would add luster to the present ceremony.

Let us then, in a rapid glance of the scientific revival, point out the secret but mighty influence which has been the directing force of modern physics. I am inclined to attribute to the study of light, and to the attraction it has for the highest minds, one of the most effective causes of the return of ideas toward natural philosophy, and consider optics as having exercised on the advance of science an influence it would be difficult to exaggerate. This influence, already clear at the dawn of the experimental philosophy under Galileo, grew so rapidly that to-day it is easy to foresee a vast synthesis of natural forces founded on the principles of the wave theory of light. This influence is easy to understand if we reflect that light is the way by which knowledge of the exterior world reaches our intelligence. It is, in fact, to sight

that we owe the quickest and most perfect notions of the objects around us: our other senses, hearing, feeling, also bring their share of learning, but sight alone affords us abundant means of simultaneous information such as no other sense can. It is, therefore, not surprising that light, this lasting link between us and the outward world, should intervene with the varied sources of its inner constitution to render more precise the observation of natural phenomena. Thus each discovery concerning new properties of light has had an immediate effect on the other branches of human knowledge, and has indeed determined the birth of new sciences by affording new means of investigation of unexpected power and delicacy.

Optics are really a modern science. The ancient philosophers had no idea of the complexity of what is vulgarly called light; they confounded in the same name what is proper to man, and what is exterior. They had, however, perceived one of the characteristic properties of the link, which exists between the source of light and the eye, which receives the impression, "Light moves in a straight line." Common experience had revealed this axiom through the observation of the shining trains that the sun throws across the skies, piercing misty clouds, or penetrating into some dark space. Hence arises two empirical notions—the definition of the ray of light, and that of the straight line. The one became the basis of optics, the other that of geometry.

Very little remains to us of the ancient books upon optics. Yet we are aware that they knew the reflection of the luminous rays on polished surfaces and the geometrical explanation of the images formed by mirrors.

We must wait many centuries until the scientific revival for a new progress in optics (but then a very considerable one) opens the new era; it is the invention of the telescope.

The new era begins with Galileo, Boyle, and Descartes, the founders of experimental philosophy. All devote their life to meditations on light, colors, and forces. Galileo lays the base of mechanics, and with the refracting telescope that of astro-physics. Boyle improves experimentation. As to Descartes, he embraces with his penetrating mind the whole of natural philosophy; he throws away the occult causes admitted by the scholastics, and proclaims as a principle that all phenomena are governed by the laws of mechanics. In his system of the universe, light plays a prominent part:[1] it is produced by the waves excited in the subtle matter which, according to his view, pervades space. This subtle matter (which represents what we call to-day the ether) is considered by him as formed of particles in immediate contact; it constitutes thus at the same time the vehicle of the forces existing between the material bodies which are plunged in it. We recognize the famous "vortices of Descartes," sometimes admired, sometimes

[1]Le Monde de M. Descartes, ou le Traité de la Lumière (Paris, 1664).

baffled during the last centuries, but to which skillful contemporaneous physicists have rendered the importance they deserve.

Whatever may be the opinions granted to the exactness of the deductions of this great philosopher, we must be struck by the boldness with which he proclaims the connection of the great cosmical problems and foretells the solutions which actual generations did not yet entirely accept, but drew insensibly to.

In Descartes's view the mechanism of light and that of gravitation are inseparable; the seat of corresponding phenomena is this subtle matter which pervades the universe, and their propagation is performed by waves around the acting centers.

This conception of the nature of light shocked the opinions in vogue; it raised strong opposition. Since the oldest times it was the habit to imagine the luminous ray as the trajectory of rapid projectiles thrown by the radiant source. Their shock on the nerves of the eye produce vision; their resistance or changes of speed, reflection or refraction. The Cartesian theory had, however, some seductive aspects which brought defenders. The waves excited on the surface of still water offer so clear an image of a propagated motion around a disturbing center! On the other hand, do not the sonorous impressions reach our ear by waves? Our mind feels yet a real satisfaction in thinking that our most sharp and delicate organs are both impressed by a mechanism of the same nature.

Yet a serious difference arose. Sound does not necessarily travel in straight lines as light does. It travels round any object opposed to it, and will follow the most circuitous routes with scarcely any loss of strength. Physicists were thus divided into two camps. In one the partisans of emission, in the other those of the wave theory, each system boasting itself superior, and indeed each being so in certain respects. Other phenomena had to be examined in order to decide between them.

The chance of discovery brought to view several phenomena which ought to have decided in favor of wave theory, as was proved a century later; but the simplest truth does not prevail without long endeavor.

A strange compromise was effected between the two systems, helped on by a name great among the greatest, and for a century the theory of emission triumphed.

The tale is a strange one. In 1661 a young scholar, full of eagerness and penetration, enters Trinity College, Cambridge; his name is Isaac Newton. He has already in his village read Kepler's Optics. Almost immediately, and while following Barrow's lectures upon optics, he studies the geometry of Descartes with passionate care; with his savings he buys a prism that he might examine the properties of color and meditate deeply on the causes of gravitation. Eight years later his masters think him worthy to succeed Barrow in the

Lucasian professorship, and in his turn he also teaches optics. The pupil soon becomes greater than his teacher, and he gives out this great result: White light which seemed the type of pure light is not homogeneous; it consists of rays of different refrangibility, and he demonstrates it by the celebrated experiment of the solar spectrum, in which a ray of white light is decomposed into a series of colored rays like a rainbow; each shade of the color is simple, for the prism does not decompose the shade. This is the origin of the spectral analysis. This analysis of white light brought Newton to explain the colors of the thin plates which are, for instance, observed in soap bubbles. The fundamental experiment, that of Newton's rings, is one of the most instructive in optics, while the laws that govern it are of admirable simplicity.

The theory was expounded in a discourse addressed to the Royal Society, with the title, "A new hypothesis concerning light and color."

This discourse called forth from Hooke a sharp complaint. Hooke also had already examined the color of thin plates, and endeavored to explain them in the wave system. He had the merit, which Newton himself readily granted, to substitute for the progressive wave of Descartes a vibrating one—a new and extremely important notion. He had even noticed the part of the two reflecting services of the thin plate and the mutual action of the reflected waves. Consequently Hooke should have been the very forerunner of the modern theory if he had had, as Newton, the clear intelligence of the simple rays. But his vague reasoning to explain the colors takes away all demonstrative value from his theory.

Newton is very affected by this complaint of priority, and combats the arguments of his adversary by remarking that the wave theory is inadmissible because it does not explain the existence of the luminous ray and of the shadows. He denies the opinion that he has raised a theory; he certifies that he does not admit either the wave hypothesis or the emission, but he says:

"He shall sometimes, to avoid circumlocution and to represent it conveniently, speak of it as if he assumed it and propounded it to be believed."

And, really, in the Proposition XII (second book of his Optics)[1] which constitutes what was since called the theory of fits, Newton remains absolutely on the ground of facts. He says simply, the phe-

[1] Prop. XII.—Every ray of light in its passage through any refracting surface is put into a certain transient constitution or state, which in the progress of the ray returns at equal intervals, and disposes the ray at every return to be easily transmitted through the next refracting surface, and between the returns to be easily reflected by it. (Sir Isaac Newton, Opticks: or a Treatise of the Reflections, Refractions, Inflexions, and Colors of Light. London, 1718. Second edition, with additions, p. 293.)

" It was, by the great mathematician, who had the glory of submitting the motions of all celestial bodies to the one law of universal gravitation.

To-day this theory is abandoned; it is condemned by the *experimentum crucis* of Arago, realized by Fizeau and Foucault. One ought, however, to acknowledge that it has constituted a real progress by the precise and new notions which it contains. The ray of light, considered up till then, was simply the trajectory of a particle in rectilinear motion; the ray of light, such as Newton described it, possesses a regular periodic structure, and the period or interval of fits, characterizes the color of the ray. This is an important result. It only requires a more suitable interpretation to transform the luminous ray into a vibratory wave; but we had to wait a century, and Dr. Thomas Young, in 1801, had the honor of discovering it.

Resuming the study of thin plates, Thomas Young shows that everything is explained with extreme simplicity, if it be supposed that the homogeneous luminous ray is analogous to the sonorous wave produced by a musical sound; that the vibrations of ether ought to compose—that is to say, to interfere—according to the expression that he proposes as to their mutual actions.

Although Young had taken the clever precaution of supporting his views by the authority of Newton,[1] the hypothesis found no favor; his principle of interference led to this singular result, that light added to light could, in certain cases, produce darkness, a paradoxical result contradicted by daily experience. The only verification that Young brought forward was the existence of dark rings in Newton's experiment; darkness due, according to him, to the interference of waves reflected on the two faces of the plate. But as the Newtonian theory interpreted the fact in a different manner, the proof remained doubtful, an *experimentum crucis* was wanting. Young did not have the good success to obtain it.

The theory of waves relapsed then once more into the obscurity of controversy, and the terrible argument of the rectilinear propagation was raised afresh against it. The most skilled geometers of the period Laplace, Biot, Poisson—naturally leaned to the Newtonian opinion; Laplace in particular, the celebrated author of the Mecanique Celeste, had even taken the offensive. He was going to attack the theory of waves in its most strongly fortified intrenchments, which had been raised by the illustrious Huygens.

Huygens, indeed, in his "Traité de la Lumière," had resolved a problem before which the theory of emission had remained mute; that is to say, the explanation of the double refraction of Iceland spar. The wave theory (on the contrary) reduced to the simplest geometrical

[1] The Bakerian lecture "On the theory of light and colors," by Thomas Young. Phil. Trans. of the Royal Society for the year 1802.

construction the path of the two rays, ordinary and extraordinary. Experiment confirmed the results in every point. Laplace succeeded in his turn (with the help of hypotheses of the constitution of luminous particles) to explain the path of these strange rays. The victory of the theory of particles then appeared complete. A new phenomenon arrived also appropriately to render it striking.

Malus discovered that a common ray of light reflected under a certain angle acquired unsymmetrical properties similar to those rays from a crystal of Iceland spar. He explained this phenomenon by an orientation of the luminous molecule, and, consequently, named this light polarized light. This was a new success for emission.

The triumph was not of long duration. In 1816 a young engineer scarcely out of the École Polytechnique, Augustin Fresnel, confided to Arago his doubts on the theory then in favor, and pointed out to him the experiments which tended to overthrow it.

Supporting himself on the ideas of Huygens, he attacked the formidable question of rays and shadows, and had resolved it; all the phenomena of diffraction were reduced to an analytical problem, and observations verified calculation marvelously. He had, without knowing it, rediscovered Young's reasonings as well as the principle of interference; but more fortunate than he, he brought the *experimentum crucis*—the two-mirror experiment; there, two rays, issuing from the same source, free from any disturbance, produced when they met, sometimes light, sometimes darkness. The illustrious Young was the first to applaud the success of his young rival, and showed him a kindness which never changed.

Thus, thanks to the use of the two-mirror experiment, the theory of Dr. Young—that is to say, the complete analogy of the luminous ray and the sound wave—is firmly established.

Moreover, Fresnel's theory of diffraction shows the cause of their dissimilarity; light is propagated in straight lines because the luminous waves are extremely small. On the contrary, sound is diffused because the lengths of the sonorous waves are relatively very great.

Thus vanished the terrible objection which had so much tormented the mind of great Newton.

But there remained still to explain another essential difference between the luminous wave and the sonorous wave; the latter undergoes no polarization. Why is the luminous wave polarized?

The answer to this question appeared so difficult that Young declared he would renounce seeking it. Fresnel worked more than five years to discover it. The answer is as simple as unexpected. The sound wave can not be polarized because the vibrations are longitudinal; light, on the other hand, can be polarized because the vibrations are transverse that is to say, perpendicular to the luminous ray.

Henceforth the nature of light is completely established. All the

phenomena presented as objections to the undulatory theory are explained with marvelous facility, even down to the smallest details.

I would fain have traced by what an admirable suite of experiment and reasoning Fresnel arrived at this discovery, one of the most important of modern science; but time presses.

It has sufficed me to explain how very great the difficulties were which he had to overcome in order to establish it.

I hasten to point out its consequences.

You saw, at starting, the purely physiological reasons which make the study of light the necessary center of information gathered by human intelligence. You judge now, by the march of this long development of optical theories, what preoccupations it has always caused to powerful minds interested in natural forces. Indeed, all the phenomena which pass before our eyes involve a transmission to a distance of force or movement; let the distance be infinitely great, as in celestial space, or infinitely small, as in molecular intervals, the mystery is the same. But light is the agent which brings us the movement of luminous bodies. To fathom the mechanism of this transmission is to fathom that of all others, and Descartes had the admirable intuition of this when he comprehended all these problems in a single mechanical conception. Here is the secret bond which has always attracted the physicists and geometers toward the study of light. Looked at from this point of view, the history of optics acquires a considerable philosophical importance; it becomes the history of the successive progress of our knowledge on the means that nature employs to transmit movement and force to a distance.

The first idea which came to the mind of man (in the savage state) to exercise his force beyond his reach is the throwing of a stone, of an arrow, or of some projectile; this is the germ of the theory of emission. This theory corresponds to a philosophical system which assumes an empty space in which the projectile moves freely. At a more advanced degree of culture, man having become a physicist, has had the more delicate idea of the transmission of movement by waves, suggested at first by the study of waves; afterwards by that of sound.

This second way supposes, on the other hand, that space is a plenum; there is no longer here transport of matter; particles oscillate in the direction of propagation, and it is by compression or rarefaction of a continuous elastic medium that movement and force are transmitted. Such has been the origin of the theory of luminous waves. Under this form it could only represent a part of the phenomena. It was therefore insufficient.

But geometers and physicists before Fresnel did not know of any other undulatory mechanism in a continuous medium.

The great discovery of Fresnel has been to reveal a third mode of

transmission quite as natural as the preceding one, but which offers an incomparable richness of resources. These are the waves of transverse vibrations excited in an incompressible continuous medium; those which explain all the properties of light.

In this undulatory mode the displacement of particles brings into play an elasticity of a special kind. This is the relative slipping of strata concentric to the disturbance which transmits the movement and the effort. The character of these waves is to impose on the medium no variation of density as in the system of Descartes. The richness of resource mentioned above depends upon the fact that the form of the transverse vibration remains indeterminate, and thus confers on waves an infinite variety of different properties.

The rectilinear, circular, and elliptical forms characterize precisely the polarizations, so unexpected, which Fresnel discovered, and by the aid of which he has so admirably explained the beautiful phenomena of Arago produced by crystallized plates.

The possible existence of waves which are propagated without change of density, has profoundly modified the mathematical theory of elasticity. Geometers found again in their equations waves having transverse vibrations which were unknown to them. They learned besides, from Fresnel, the most general constitution of elastic media, of which they had not dreamed.

It is in his admirable memoir on double refraction that this great physicist set forth the idea that in crystals the elasticity of the ether ought to vary with the direction, an unexpected condition and one of extreme importance, which has transformed the fundamental bases of molecular mechanics; the works of Cauchy and Green are the striking proofs of it. From this principle Fresnel concluded the most general form of the surface of the luminous wave in crystals, and found (as a particular case) the sphere and ellipsoid that Huygens had assigned to the Iceland spar crystal. This new discovery excited universal admiration among physicists and geometers. When Arago came to expound it before the Académie des Sciences, Laplace, who had been such a long time hostile, declared himself convinced. Two years later Fresnel, unanimously elected a member of the Academy, was elected with the same unanimity foreign member of the Royal Society of London. Young himself transmitted to him the announcement of this distinction, with personal testimony of his sincere admiration.

The definite foundation of the undulatory theory imposes the necessity of admitting the existence of an elastic medium to transmit the luminous movement. But does not all transmission to a distance of movement or of force imply the same condition? To Faraday is due the honor of having, like a true disciple of Descartes and Leibnitz, proclaimed this principle, and of having resolutely attributed to reac-

tions of surrounding media the apparent action at a distance of electrical and magnetic systems. Faraday was recompensed for his boldness by the discovery of induction.

And since induction acts even across a space void of ponderable matter, one is forced to admit that the active medium is precisely that which transmits the luminous waves—the ether.

The transmission of a movement by an elastic medium can not be instantaneous; if it is truly luminous ether that is the transmitting medium, ought not the induction to be propagated with the velocity of luminous waves?

The verification was difficult. Von Helmholtz, who tried the direct measurement of this velocity, found, as Galileo formerly, for the velocity of light a value practically infinite.

But the attention of physicists was attracted by a singular numerical coincidence. The relation between the unity of electrostatic quantity to the electro-magnetic unit is represented by a number precisely equal to the velocity of light.

The illustrious Clerk Maxwell, following the ideas of Faraday, did not hesitate to see in the relationship the indirect measure of the velocity of induction, and by a series of remarkable deductions he built up this celebrated electro-magnetic theory of light, which identifies in one mechanism three groups of phenomena completely distinct in appearance—light, electricity, and magnetism.

But the abstract theories of natural phenomena are nothing without the control of experiment.

The theory of Maxwell was submitted to proof, and the success surpassed all expectation. The results are too recent and too well known, especially here, for it to be necessary to insist upon them.

A young German physicist, Henry Hertz, prematurely lost to science, starting from the beautiful analysis of oscillatory discharges of Von Helmholtz and Lord Kelvin, so perfectly produced electric and electro-magnetic waves that these waves possess all the properties of luminous waves. The only distinguishing peculiarity is that their vibrations are less rapid than those of light.

It follows that one can reproduce with electric discharges the most delicate experiments of modern optics—reflection, refraction, diffraction, rectilinear, circular, elliptic polarization, etc. But I must stop, gentlemen. I feel that I have assumed too weighty a task in endeavoring to enumerate the whole wealth which waves of transverse vibrations have to-day placed in our hands.

I said at the beginning that optics appeared to me to be the directing science in modern physics.

If any doubt can have arisen in your minds, I trust this impression has been effaced to give place to a sentiment of surprise and admira-

tion in seeing all that the study of light has brought of new ideas on the mechanism of the forces of nature.

It has insensibly restored the Cartesian conception of a single medium refilling space, the seat of electrical, magnetic, and luminous phenomena. It allows us to foresee that this medium is the depositary of the energy spread throughout the material world, the necessary vehicle of every force, the origin even of universal gravitation.

Such is the work accomplished by optics. It is perhaps the greatest thing of the century!

The study of the properties of waves, viewed in every aspect, is therefore, at the present moment, the most fertile study.

It is that which has been followed in the double capacity of geometer and physicist by Sir George Stokes, to whom we are about to pay so touching and deserved a homage. All his beautiful researches, both in hydrodynamics as well as in theoretical and practical optics, relate precisely to those transformations which various media impose on waves which traverse them.

In the many phenomena which he has discovered or analyzed, movements of fluids, diffraction, interference, fluorescence, Röntgen rays, the dominant idea which I pointed out to you is always visible. It is that which makes the harmonious unity of the scientific life of Sir George Stokes.

The University of Cambridge may be proud of the Lucasian chair of mathematical physics, because from Sir Isaac Newton up to Sir George Stokes it has contributed a glorious part toward the progress of natural philosophy.

THE MOTION OF A PERFECT LIQUID.[1]

By Prof. H. S. Hele-Shaw.

If we look across the surface of a river, we can not fail to observe the difference of the movement at various points. Near one bank the velocity may be much less than near the other, and generally, though not always, it is greater in the middle than near either bank. If we could look beneath the surface and see what was going on there, we should find that the velocity was not so great near the bottom as at the top, and was scarcely the same at any two points of the depth. The more we study the matter, the more complex the motion appears to be; small floating bodies are not only carried down at different speeds and across each other's paths, but are whirled round and round in small whirlpools, sometimes even disappearing for a time beneath the surface. By watching floating bodies we can sometimes realize these complex movements, but they may take place without giving the slightest evidence of their existence.

You are now looking at water flowing through a channel of varying cross section, but there is very little evidence of any disturbance taking place. By admitting color, although its effect is at once visible on the water, it does not help us much to understand the character of the flow. If, however, fine bubbles of air are admitted, we at once perceive (fig. 1) the tumultuous conditions under which the water is moving and that there is a strong whirlpool action. This may be intensified by closing in two sides (fig. 2), so as to imitate the action of a sluice gate, through the narrow opening of which the water has all to pass, the presence of air making the disturbed behavior of the water very evident.

Now you will readily admit it is hopeless to begin to study the flow of the water under such conditions, and we naturally ask, Are there not cases in which the action is more simple? Such would be the case if the water flowed very slowly in a perfectly smooth and parallel river bed, when the particles would follow one another in lines called "stream lines," and the flow would be like the march of a disciplined army

[1] A discourse delivered at the Royal Institution, London, on Friday, February 10, 1899, by Prof. H. S. Hele-Shaw. Printed in Nature September 7, 1899.

instead of like the movement of a disorderly crowd, in which fights taking place at various points may be supposed to resemble local disturbances of whirlpools or vortices.

The model (fig. 3) represents on a large scale a section of the channel already shown, in which groups of particles of the water are indicated by round balls, lines in the direction of flow of these groups, which, for convenience, we may call particles, being colored alternately. When we move these so that the lines are maintained, we imitate "stream-line" motion, and when, at any given point of the pipe, the succeeding particles always move at exactly the same velocity, we have what is understood as "steady motion."

As long as all the particles move in the straight portion of the channel their behavior is easy enough to understand. But as the channel widens out it is clear that this model does not give us the proper distribution. In the model the wider portions are not filled up, as they would be with the natural fluid; for it must be clearly understood that the stream lines do not flow on as the balls along these wires, passing through a mass of dead water, but redistribute themselves so that every particle of water takes part in the flow. Perhaps you may think that if these wires were removed, and the wooden balls allowed to find their own positions, they would group themselves as with an actual liquid. This is not the case; and, for reasons that you will see presently, no model of this kind would give us the real conditions of actual flow. By means of a model, however, we may be able to understand why it is so absolutely essential we should realize the correct nature of the grouping which occurs.

First look at the two diagrams (figs. 4 and 5), which you will see represent channels of similar form to the experimental one. The same number of particles enter and leave in each under apparently the same conditions, so that the idea may naturally arise in your minds that if the particles ultimately flow with the same speed, whatever their grouping in the larger portion of the channel, it can not much matter in what particular kind of formation they actually pass through that wider portion. To understand that is really very important. Let us consider a model (fig. 6) specially made for the purpose. You will see that we have two lines of particles which we may consider stream lines, those on the left colored white and those on the right colored red. The first and last are now exactly 18 inches apart, there being 18 balls of 1 inch diameter in the row. If I move the red ones upward, I cause them to enter a wider portion of the channel, where they will have to arrange themselves so as to be three abreast (fig 7). It is quite clear to you that as I do this their speed in the wider portion of the channel is only one-third of that in the narrow portion, as you will see from the relative positions of the marked particles. Now, directly the first particle entered the wider channel, it commenced to move at a

FIG. 1.

FIG. 2.

MOTION OF PERFECT LIQUID.

Fig. 3.

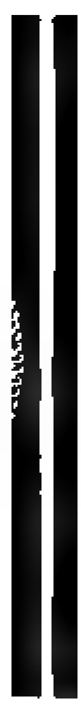


Fig. 4. Fig. 5.

MOTION OF PERFECT LIQUID.

reduced speed, with the result that the particles immediately behind it must have run up against it, exactly in the same way that you have often heard the trucks in a goods train run in succession upon the ones in front when the speed of the engine is reduced; and you will doubtless have noticed that it was not necessary for the engine actually to stop in order that this might take place. Moreover, the force of the impact depended largely upon the suddenness with which the speed of those in front was reduced. Applying this illustration to the model, you will see that the impact of these particles in the wider portion would necessarily involve a greater pressure in that part. Turning next to the white balls, I imitate, by means of the left-hand portion, the flow which will occur in a channel six times as large as the original one, and you now see (fig. 7) that as the particles have placed themselves six abreast, and the first and last row are 3 inches apart instead of 18 inches, the speed in the wider portion of the channel must have been one-sixth of that in the narrow portion. Evidently, therefore, the velocity of the particles has been reduced more rapidly than in the previous case, and the pressure must consequently be correspondingly greater.

We may now take it as perfectly clear and evident that the pressure is greater in the wider portion and less in the narrower portion of the channel. Turning now to the two diagrams, we see that the pressure is in each case greater in every row of particles as in the wider portions of the channel, but that instead of being suddenly increased, as in the model, it is gradually increased. The width of the colored bands, that is, rows of particles, or width apart of stream lines, is a measure of the increased pressure. Thus you will now regard the width of the bands, or, what is the same thing, the distance apart of the stream lines, as a direct indication of pressure and the narrowness or closeness of the stream lines as a direct indication of velocity.

Next notice the great difference between the two diagrams. In one diagram (fig. 4) the change of width is uniform across the entire section. In diagram (fig. 5), however, this is not the case. In the narrowest portion of the channel in each diagram there are seven color bands of little balls, each containing three abreast, but we find that in one diagram (fig. 4) they are equally spaced in the wider part six abreast throughout. In the other diagram (fig. 5) the outer row is spaced eight abreast, the second row rather more than six, and the inner rows rather more than four abreast, and the middle row less than four abreast, making in all forty-two in a row, as in the previous case. One diagram (fig. 5), therefore, will represent an entirely different condition to the state represented by the other diagram (fig. 4), the pressure in the wide part of the latter varying from a maximum at the outside to a minimum in the middle, while the corresponding velocity is greatest in the middle and least at the outside or borders.

Now, when we know the pressure at every point of a liquid and also the direction in which the particles are moving, together with their velocity at every point, we really know all about its motion, and you will see how important the question of grouping is, and that, in fact, it really constitutes the whole point of my lecture to-night. How, then, shall we ascertain which of the two groupings (fig. 4 or 5) is correct, or whether possibly some grouping totally different from either does not represent the real conditions of flow?

Now, the model does not help us very far, because there seems to be no means of making the grouping follow any regular law which might agree with fluid motion. In whatever way we improve such a model, we can scarcely hope to imitate by merely mechanical means the motion of an actual liquid, for reasons which I will now try to explain.

In the first place, apart from the particles having no distinguishing characteristics, either when the liquid is opaque or transparent, they are so small and their number so great as to be almost beyond our powers of comprehension. Let me try, by means of a simple illustration, to give some idea of their number as arrived at by perfectly well recognized methods of physical computation. Lord Kelvin has used the illustration that, supposing a drop of water were magnified to the size of the earth, the ultimate particles would appear to us between the size of cricket balls and footballs. I venture to put the same fact in another way that may perhaps strike you more forcibly. This tumbler contains half a pint of water. I now close the top. Suppose that, by means of a fine hole, I allow one and a half millions of millions (1,500,000,000,000) of particles to flow out per second—that is to say, an exodus equal to about one thousand times the population of the world in each second—the time required to empty the glass would be between (for of course we can only give certain limits) seven million and forty-seven million years.

In the next place, we have the particles interfering with each other's movements by what we call "viscosity."

Of course, the general idea of what is meant by a "viscous" fluid is familiar to everybody, as that quality which treacle and tar possess in a marked degree, glycerin to a less extent, water to a less extent than glycerin, and alcohol and spirits least of all. In liquids, the property of viscosity resembles a certain positive "stickiness" of the particles to themselves and to other bodies, and would be well represented in our model by coating over the various balls with some viscous material, or by the clinging together which might take place by the individuals of a crowd, as contrasted with the absence of this in the case of no viscosity as represented by the evolutions of a body of soldiers. It may be accounted for, to a certain extent, by supposing the particles to possess an irregular shape, or to constantly move across each other's path, causing groups of particles to be whirled round together.

Fig. 1. Fig. 2.

Fig. 3.

Motion of Perfect Liquid.

Whatever the real nature of viscosity is, it results in producing in water the eddying motion which would be perfectly impossible if viscosity were absent, and which makes the problem of the motion of an imperfect liquid so difficult and perplexing.

Now, all scientific advance in discovering the laws of nature has been made by first simplifying the problem and reducing it to certain ideal conditions, and this is what mathematicians have done in studying the motion of a liquid.

We have already seen what almost countless millions of particles must exist in a very small space, and it does require a much greater stretch of the imagination to consider their number altogether without limit. If we then assume that a liquid has no viscosity, and that it is incompressible, and that the number of particles is infinite, we arrive at a state of things which would be represented in the case of the model or the diagram on the wall, when the little globes were perfectly smooth, perfectly round, and perfectly hard, all of them in contact with each other, and with an unlimited number occupying the smallest part of one of the colored or clear bands. This agrees with the mathematical conception of a perfect liquid, although the mathematician has in his mind the idea of something of the nature of a jelly consisting of such small particles, rather than of the separate particles themselves. The solution of the problem of the grouping of the little particles, upon which so much depends, and which may have at first seemed so simple a matter, really represents, though as yet applied to only a few simple cases, one of the most remarkable instances of the power of higher mathematics, and one of the greatest achievements of mathematical genius.

You will be as glad as I am that it is not my business to-night to explain the mathematical processes by which the behavior of a perfect liquid has been, to a certain extent, investigated. You will also understand why such models as we could actually make, or any analogy with the things with which we are familiar, would not help us very much in obtaining a mental picture of the behavior of a perfect liquid. If, for instance, we try to make use of the idea of drilled soldiers, and move the lines with that object in view, we see that instead of the ordinary methods of drill, the middle rank soon gains on the others, and enters again the parallel portion of the channel in a very different relative position to the opposite lines, although the stream lines would all have the same actual velocity when once again in the parallel portion. Since, then, we can not use models or any simple analogy with familiar things, or follow—at any rate this evening—the mathematical methods of dealing with the problem, what way of understanding the subject is left to us?

If we take two sheets of glass, and bring them nearly close together, leaving only a space the thickness of a thin card or piece of paper, and

then by suitable means cause liquid to flow under pressure between them, the very property of viscosity, which, as before noted, is the cause of the eddying motion in large bodies of water, in the present case greatly limits the freedom of motion of the fluid between the two sheets of glass, and thus prevents, not only eddying or whirling motion, but also counteracts the effect of inertia. Every particle is then compelled by the pressure behind and around it to move onward without whirling motion, following the path which corresponds exactly with the stream lines in a perfect liquid.

If we now, by a suitable means, allow distinguishing bands of colored liquid to take part in the general flow, we are able to imitate exactly the conditions we are seeking to understand.

[Professor Hele-Shaw here gave demonstrations of the stream lines in liquids flowing under the conditions of a gradually enlarging and contracting channel. He proved that the condition of flow corresponded closely with that shown in fig. 5 and not with that given in fig. 4. The method of the experiments has already been described in Nature, Vol. LVIII, p. 34, though by using glycerin instead of water much more perfect results were obtained than in those then described.]

But at this stage you may reasonably inquire how it is that we are able to state with so much certainty that the artificial conditions of flow with a viscous liquid are really giving us the stream-line motion of a perfect one; and this brings me to the results which mathematicians have obtained.

The view now shown represents a body of circular cross section, past which a fluid of infinite extent is moving, and the lines are plotted from mathematical investigation and represents the flow of particles. This particular case gives us the means of most elaborate comparison. Although we can not employ a fluid of infinite extent, we can prepare the border of the channel to correspond with any one of the particular stream lines and measure the exact positions of the lines inside.

By means of a second lantern the real flow of a viscous liquid for this case is shown upon the second screen, and you will see that it agrees with the calculated flow round a similar obstacle of a perfect liquid. The diagram shown on the wall is the actual figure employed for comparison and upon which the experimental case was projected. By this means it was proved that the two were in absolute agreement. If we start the impulses as before, in a row, we at once see how the middle particles lag behind the outer ones, as indicated by the width of the bands, showing that it is not necessarily the side stream lines that move more slowly. It may be more interesting to you to see, in addition to the foregoing case—in which, for convenience, and as quite sufficient for measurement only, a semicylinder was employed—the case of a complete cylinder (fig. 8). In this case two different colors are used in alternate bands, and these bands are sent in, not steadily,

FIG. 9.

FIG. 10.

MOTION OF PERFECT LIQUID.

but impulsively, in order to illustrate what I have just pointed out. You will see how the greater width of the color bands before and behind the cylinder indicates an increase of pressure in those regions. This, in a ship-shape form, accounts for the standing bow and stern waves, whereas the narrowing of the bands at the sides indicates an increase of velocity and reduction of pressure, and accounts for the depression of water level, with which you are doubtless familiar, at the corresponding part of a ship.

I will now take a more striking case. If, instead of a circular body, we had a flat plate, the turbulent nature of the flow is evidently very great, as you will see from the view (fig. 9), which is a photograph of the actual flow under these conditions, made visible by very fine air bubbles, and showing water at rest in the clear space behind the obstacle.

We can, however, take steps to reduce this turbulence, and you now see on the second screen the flow by means of apparatus which time does not permit me to describe, but which gives a slow and steady motion that it would be impossible to improve upon in actual conditions of practice, or even, I am inclined to think, by any experimental method. Instead of using air to make this flow clear, we now allow color to stream behind the plate, and you will see that the water still refuses to flow round to the back, and spreads on either side. We have so slow a velocity as not to induce vortex motion, but the inertia of the particles which strike the flat plate causes them to be deflected to either side, exactly as tennis balls in striking against a wall obliquely. The sheet of water is so thick—that is to say, the parallel glass plates are so far apart—that they do not enable the viscosity of the water to act as a sufficient drag to prevent this taking place.

Mathematicians, however, predicted with absolute certainty that with stream-line motion, the water should flow round and meet at the back, a state of things that, however slow we make the motion in the present case, does not occur, owing to the effect of inertia. They have drawn with equal confidence the lines along which this should take place. We could either effect this result with the experiment you have just seen, by using a much more viscous liquid, such as treacle, or, what comes to the same thing, bringing the two sheets of glass nearly close together; and the flow which you are now witnessing (fig. 10) shows the result of doing this. The color bands in front of the plate no longer mix at all with the general body of flow, or are unsteady, as was the case in the last experiment, but flow round the plate, and flow so steadily, that unless we jerk the flow of the color bands, it is impossible to tell in which direction they are actually moving. It is interesting to note that where the divided central color band reunites is clearly shown in the illustration.

While I have been dealing with the stream lines of a perfect liquid your minds will doubtless have turned to the lines along which mag-

netic and electric forces appear to act. We are possibly further from realizing the actual nature of these forces than from a correct conception of the real nature of a liquid. We have long agreed to abandon the old ideas of the electrical and magnetic fluids flowing along these lines, and to substitute instead the idea that these lines represent merely the directions in which the forces act. Now we can easily see that this conception is quite a reasonable one, for in the case of the model it is not necessary to have the row of balls actually moving in order that the effect may be transmitted along the different lines they occupy. If I attempt to raise the plate upon which they rest, the pressure is instantly transmitted through the whole row to the top ball along each line, whatever curve the line may take. In the same way, you will remember that it was not necessary to have the color bands actually in motion, for, though apparently free to move in any direction, they retain their form for a considerable time, and the path along which they would influence each other as soon as the tap is opened would be along those lines in which the liquid was flowing before it was brought to rest. Hence it is possible, with some suitable means, to cause a viscous liquid to reproduce exactly the lines of magnetic and electrical induction. In the case of magnetism and electricity it is, of course, possible, by means of a small magnetic needle or a galvanometer, by exploring the whole surface through which magnetic induction or electrical flow is acting, to plot the lines of force for innumerable cases, where we can work in air or on the surface of the solid conductor.

But in this building it seems natural to take as an example the case first used by the great man to whom the conception of lines of magnetic force is due, for the first reference I have been able to find to such lines is in one of Faraday's earliest papers on the indication of electric currents (Experimental Researches in Electricity, Vol. I, p. 32), in which he says:

"By magnetic curves I mean the lines of magnetic forces, however modified by the juxtaposition of poles, which would be depicted by iron filings, or those to which a very small magnetic needle would form a tangent."

You are all familiar with the way in which iron filings set themselves when shaken over the north and south poles of a magnet. The magnetic lines are then nearly, but not quite, circular curves between the two poles. Now, the mathematics of the subject tell us that if the poles could be regarded as points, the lines of force between them would be perfect circles.

You are now looking at the color bands, the edges, or indeed any portion of which represent lines obtained by admitting colored liquid from a series of small holes round a central small orifice which admits clear liquid and allows them to escape through another small orifice

(called, respectively, in hydromechanics a *source* and *sink*), and I leave it to you to judge how far these curves deviate from the ideal form.

My assistant is now allowing the color to flow, first steadily, and then in a series of impulses, and the latter gives up the conception of waves or impulses of magnetic force, though of course the magnetic transmission force would be instantaneous. Regarded as a liquid, it is here again clear how absolutely the truth of our views concerning the slower movement in the wider portion is verified by this experiment.

A last experiment shows the streams admitted, not from a source, but from a row of orifices in what corresponds to the slowest moving portion of the flow. The result is that the color bands are much narrower, and although the circular forms of the curves are, as in the previous experiment, preserved, the lines are so fine at the point of exit, which, as before, corresponds to the South Pole, as to really approximate to ideal stream lines.

The same method enables us to trace the lines of force through solid conductors, for as long as we confine ourselves to two dimensions of space we may have *flat* conductors of any shape whatever. But it does something more, for by making the film rather deeper in some places than others more particles arrange themselves there, and the lines of flow will naturally tend in the direction of the deeper portion. This will give the stream lines identically the same shape as the magnetic or electrical curves which encounter in their paths a body of less resistance—for instance, a paramagnetic body.

If, on the other hand, at these points the film is made rather thinner, less particles will be able to dispose of themselves in the shallow portion of the film, and hence the lines of flow will be pushed away from this portion, giving us exactly the same forms as magnetic lines of force in a magnetic field in proximity to a diamagnetic body.

Here, again, mathematical methods have enabled lines of actual flow to be predicted, and you may compare the actual flow for the case of a cylindrical paramagnetic body which was worked out some years ago.

You will doubtless not be inclined to question the practical value of stream lines in the subject which we have just been considering, because, unlike the flow of an actual liquid, magnetic lines of force can never be themselves seen, and because there is no doubt as to the correspondence of the directions to the lines of a perfect liquid. It was the conception of these lines in the mind of Faraday, and more particularly their being cut by a moving wire, that enabled him to realize the nature of the subject more clearly than any other man at the time, and to do much toward the rapid development of electrical science and its practical applications.

When we come to consider the relation of the study of the motion of a perfect liquid with hydromechanics and naval architecture, it must

be admitted that the matter is a difficult one. Probably one of the most perplexing things in engineering science is the absence of all apparent connection between higher treatises on hydrodynamics and the vast array of works on practical hydraulics. The natural connection between the treatises of mathematicians and experimental researches of engineers would appear to be obvious, but very little, if any, such connection exists in reality, and while at every step electrical applications owe much to the theories which are common to electricity and hydromechanics, we look in vain for such applications in connection with the actual flow of water.

Now the reason for this appears to be the immense difference between the flow of an actual liquid and that of a perfect one owing to the property of viscosity. A comparison of the various experiments which you have seen to some extent indicates this.

In the first place, let us consider for a moment some of the things which would happen if water were a perfect liquid. In such a case, a ship would experience a very different amount of resistance, because, although waves would be raised, owing to the reasons which we have already seen, the chief causes of resistance, viz, skin friction and eddying motion, would be entirely absent, and of course a submarine boat at a certain depth would experience no resistance at all, since the pressures fore and aft would be equal. On the other hand, there would be no waves raised by the action of the wind, and there would be no tidal flow, but to make up for this rivers would flow with incredible velocity, since there would be no retarding forces owing to the friction of the banks. But the rivers themselves would soon cease to flow because there would be no rainfall such as exists at present, since it is due to viscosity that the rain is distributed, instead of falling upon the earth in a solid mass when condensed. In a word, it may be said that the absence of viscosity in water would result in changes which it is impossible to realize.

We may now briefly try to consider the difference between practical hydraulics and the mathematical treatment of a perfect liquid. The earliest attempts to investigate in a scientific way the flow of water appears to have been made by a Roman engineer about eighteen hundred years ago, an effort being made to find the law for the flow of water from an orifice. For more than fifteen hundred years, however, even the simple principle of flow according to which the velocity of efflux varies as the square of the head, or what is the same thing, the height of surface above the orifice varies as the square of velocity, remained unknown. Torricelli, who discovered this, did so as the result of observing that a jet of water rose nearly to the height of the surface of the body of water from which it issued, and concluded therefore that it obeyed the then recently discovered law of all falling bodies.

Though it was obvious that this law did not exactly hold, it was a long time before it was realized that it was the friction or viscosity of liquids that caused so marked a deviation from the simple theory. Since then problems in practical hydraulics, whether in connection with the flow of rivers or pipes, or the resistance of ships, have largely consisted in the determination of the amount of deviation from the foregoing simple law.

About one hundred years ago it was discovered that the resistance of friction varies nearly in accordance with the simple law of Torricelli, and also, although for a totally different reason, the resistances due to a sudden contraction or enlargement of cross section of channel or to any sudden obstructions appear to follow nearly the same law. Now it is extremely convenient for reasons which will be understood by students of hydraulics to treat all kinds of resistance as following the same law, viz, square of velocity which the variation of head or height of surface has shown to do. But this is far from being exact, and an enormous amount of labor has consequently been expended in finding for all conceivable conditions in actual work tables of coefficients or empirical expressions which are required for calculations of various practical questions. Such data are continually being accumulated in connection with the flow of water in rivers and pipes for hydraulic motors and naval architecture. This is the practical side of the question.

On the other hand, eminent mathematicians, since the days of Newton and the discovery of the method of the calculus, have been pursuing the investigation of the behavior of a perfect liquid. The mathematical methods, which I have already alluded to as being so wonderful, have, however, scarcely been brought to bear with any apparent result upon the behavior of a viscous fluid. Indeed the mathematician has not been really able to adopt the method of the practical investigator and deal with useful forms of bodies such as those of actual ships, or of liquid moving through ordinary channels of varying section, even for the case of a perfect liquid, but he has had to take those cases, and they are very few indeed, that he has been able to discover which fit in with his mathematical powers of treatment.

This brief summary may possibly serve to indicate the nature of the difficulties which I have pointed out, and will show you the vast field there yet lies open for research in connection with the subject of hydromechanics, and the great reception which awaits the discovery of a theoretical method of completely dealing with viscous liquids, instead of having recourse as at present principally to empirical formulas based on the simple law already alluded to.

We may, however, console ourselves with the thought that in the application of the laws of motion themselves to any terrestrial matters the friction of bodies must always be taken into account, and renders

it necessary that we should commence by studying the ideal conditions. In this, as in other matters, the naval architect and engineer must always endeavor, as far as possible, to base their considerations and work upon the secure foundation of scientific knowledge, making allowances for disturbing causes, which then cease to be the source of perplexity and confusion. From this point of view the study of the behavior of a perfect liquid, even when no such form of matter appears to exist, has an interest for the practical man in spite of the deviation of actual liquids from such ideal conditions. If the truth must be told, it is such a deviation from the simple and ideal conditions that really constitutes the work of a professional man, and it is only practical experience which, based upon sound technical knowledge, enables 50,000 tons of steel to be made to span the Firth of Forth, Niagara to be harnessed to do the work of 100,000 horses, or an *Oceanic* to be slid into the sea with as little misgiving as the launch of a fishing boat.

I have, I am afraid, brought you only to the threshold of a vast subject, and in doing so have possibly employed reasoning of too elementary a kind. After all, I may plead that I have followed the dictum of Faraday, who said, "If assumptions must be made, it is better to assume as little as possible." If I have assumed too little knowledge on your part, it is because of the difficulties I have found in the subject myself. If I have left more obscure than I have been able to make clear, it is consoling to think how many centuries were required to discover even what is known at the present time, and we may well be forgiven if we can not grasp at once results which represent the life work of some of the greatest men.

THE FIELD OF EXPERIMENTAL RESEARCH.[1]

By ELIHU THOMPSON.

Physical research by experimental methods is both a broadening and a narrowing field. There are many gaps yet to be filled, data to be accumulated, measurements to be made with great precision, but the limits within which we must work are becoming, at the same time, more and more defined.

The upper ranges of velocities, temperatures, and pressures which manifest themselves in the study of the stellar universe are forever beyond the range of experiment. But while the astronomer must wait for opportunities to observe, the experimenter can control his conditions and employ his methods and his apparatus at once to the question in hand. Still this work must be done within a certain range or must be limited to conditions more or less easy to recognize. In spite of this fact, however, the progress made during the past century is not likely to cease or abate in the next, and the ever-increasing number of workers bodes well for the future enrichment of our science.

Whatever may be our ideas of fundamental entities, as expressed in various theories, whether, as an example, we regard the ether as like an infinitely mobile fluid, or as an incompressible solid, or as a jelly, or whether we incline to think that being an electro-magnetic medium it may be without mechanical properties, which properties depend in some way upon the electro-magnetic nature of the ether, we can not reach sure ground without the experimental test.

The development in the field of research by experiment is like the opening of a mine, which as it deepens and widens continually yields new treasure, but with increased difficulty, except when a rich vein is struck and worked for a time. In general, however, as the work progresses there will be needed closer application and more refined methods. We may, indeed, find our limit of depth in the mine of experiment in inordinate cost, in temperatures too high, or in pressures beyond the limits of our skill to control.

[1] Address of the vice-president and chairman of section B, physics, before the American Association for the Advancement of Science at the Columbus meeting, August, 1899. Printed in Science, August 25, 1899.

It is but a few months since Professor Dewar, by the evaporation of liquid hydrogen in a vacuum, closely approached, if he has not reached, our lower limit of possible temperature. Investigations of the effects of low temperature upon the properties of bodies must, from the present outlook, be forever limited to about 20° C. above absolute zero, unless a lighter gas than hydrogen be discovered upon the earth, the actual existence of which it is, of course, impossible to conjecture. Before the actual experimental demonstration of this limit the limit itself was known to theory, at least approximately, but the spur of the experimenter is the overcoming of difficulties and the possibility of new discoveries which come as surprises. In the case in question a liquid of extremely low density, only one-fourteenth that of liquid nitrogen was produced, while still defined by clear and well-marked refracting surfaces.

When we turn to the consideration of the field for research work at high temperatures we are not confronted by the fact of a physical limit existing which may be approached but never reached. We can imagine no limit to possible increase of temperature, such as is the absolute zero a limit of decrease. While we may actually employ in electric furnaces temperatures which, according to Moissan, have a lower limit of 3,500° C., we can realize the possibility of temperatures existing in the stars measured by tens of thousands or hundreds of thousands of degrees of our temperature scale.

The moderate increase of working temperature given by the electric furnace enabled Moissan and others to reap a rich harvest of experimental results, and the natural inference is that much more might be expected from further extensions of the limits. These limits are, however, already set for us by the vaporization of all known substances. Our furnace itself keeps down the temperature by melting and volatilizing. We may indefinitely increase the energy in an electric arc and thus add to the heat evolved, but the addition only goes to vaporize more material. The limit of work then seems to be readily reached in the electric furnace, no materials for lining being available, not subject either to fusion or vaporization, thus using up the energy which would otherwise go to increase the temperature.

A suggestion as to a possible extension of temperature range may be made here. It may be requisite to work with closed receptacles under pressure, and to discharge through them electric currents of so great energy-value as to attain almost instantaneously the highest temperatures, to be maintained for only a very short time. We may imagine a huge condenser charged to a potential of, say, 10,000 volts as discharged through a limited body of gas contained in a small space within a strong steel tube which has a lining of refractory nonconductor. The energy may thus possibly be delivered so suddenly to a very limited body of material as to result in a momentary elevation of

temperature passing all present known limits and capable of effecting profound changes in molecular constitution. We need all possible extension of the limits of research in this direction in order to discover some clew to the relations which the chemical elements bear to each other. The limit of possible strength of the containing receptacle, or some unforeseen factor, would probably set the new bounds. The point to be here enforced, however, is that far beyond any increase of working range in temperature, obtained in any way, there must still exist a further range unattainable by our best efforts and possibly forever outside of the field of experimental research. Our knowledge of this higher range can alone be derived from a study of the actions going on in the stars and nebulæ.

As with the temperature range, so it is with the pressure range. We may easily work under conditions which involve no pressure, but when we attempt to conduct our inquiries with increase of pressure we soon find a limit to the tenacity of our strongest vessels or to our ability to produce and maintain extreme pressures. We may work, not easily it is true, with pressures up to a few tons to the square inch, but this is as nothing compared to the conditions which we know must exist within the larger celestial bodies, without reference to their condition—solid, liquid, or gaseous. Can we ever hope to experimentally reproduce the condition of a mass of gas so compressed that in spite of a very high temperature its volume is less than that of the same mass cooled to solidification? Yet this extreme of condition must be the normal state within the bodies of many of the stars.

It has been aptly said that many and perhaps most of the important discoveries have been made with comparatively simple and crude apparatus. While this may be true, yet it is probably true also that future advance work is likely to require more and more refined means and greater nicety of construction and adjustment of apparatus. The expense or cost, if not the difficulty of the work, may become so great as to effectually bar further progress in some fields. When instruments require to be adjusted or constructed to such refined limits as a fraction of a wave length of light, but few can be found to undertake the work. The interferometer and echelon spectroscope of Michelson involve such minute adjustments that a wave length of light is relatively thereto a large measure. It is well known that this comparative coarseness of light waves imposes a limit to the powers of optical instruments, as the microscope and telescope, such that no perfection of proportion, construction, and correction of the lenses can remove.

In most fields of research, however, progress in the future will depend in an increasing degree upon the possession, by the investigator, of an appreciation of small details and magnitudes, together with a refined skill in manipulation or construction of apparatus. He must be ready to guide the trained mechanic and be able himself to admin-

ister those finishing touches which often mark the difference between success and failure. There must be in his mental equipment that clear comprehension of the proper adjustment of means to ends which is of such great value in work in new fields. He must also learn to render available to science the resources of the larger workshops and industrial establishments.

The application of physical principles upon a large scale in such works has frequently, in recent years, resulted in great gains to science itself. The resources of the physical laboratory are often relatively small and meager compared with those of the factory. Experimental work in certain lines is now frequently carried on upon a scale so great and under such varied conditions as would be almost impossible outside of a large works.

In no field has this been more true than in that of electricity during the past few years. We need only instance the progress in alternating currents and in relation to the magnetic properties of iron. In large scale operations effects which would be missed or remain masked in work undertaken upon a more restricted scale receive emphasis sufficient to cause them to command attention. The obstacle of increasing costliness of equipment, which in some fields might act as a bar to further progress, can only be overcome by more liberal endowments of laboratories engaged in advance work. Even those in the community who can only understand the value of scientific work when it has been put to practical use may find in the history of past progress that many discoveries in pure science which had not, when made, any apparent commercial importance or value have in the end resulted in great practical revolutions.

Could Volta, when he discovered the pile one hundred years ago, have had any idea of its importance in practical work? Or, did Davy or his contemporaries at the time of his experiments with the arc of flame between the charcoal terminals of his large battery have any suspicion that in less than one hundred years the electric arc would grow to such importance that more than 100,000 arc lamps would become a single year's production in this country alone? Faraday, when he made his researches upon the induction of electric currents from magnetism, could not have had any idea of the enormous practical work in which the principles he dealt with as facts of pure science would find embodiment. When he wound upon the closed iron ring the two coils of wire which enabled him to discover the facts of mutual induction, he had begun, without any suspicion of the fact, the experimental work which gave to science and to practice the modern transformer, now built of capacities ranging up to 2,500 horsepower each, and for potentials of 40,000 to 60,000 volts.

These examples, and many others which might be given, should convince even the most arrogantly practical man of the high value cf

scientific research, not alone as adding to the sum total of knowledge and for the admirable training it gives, but because it can not fail to have an ultimate practical effect. Discoveries which at first seem to have no useful nor practical outcome are often the very ones which underlie development of the greatest importance in the arts and industries.

The work of Hertz upon electric waves was to the physicist a grand experimental demonstration, tending to prove the truth of the electro-magnetic theory of light, and subsequent progress was profoundly influenced by it, though no practical use followed at once. The physicist to-day may see in the wireless telegraph only an extension of Hertz's original work, for he need not consider the commercial or economic outcome. He may, however, recognize the fact that in the wireless telegraph, as developed by Marconi, practice calls for a broader theoretical view. Certain elements of construction and adjustment of apparatus, at first used and regarded as essential from a theoretical standpoint, have already been laid aside. The radiator, with its large polished brass spheres and special spark gap, has been found of no more effect than the simple pair of small balls ordinarily constituting the terminals for high potential discharges. It has been found that the transmitting and receiving apparatus do not require to be attuned, and that the receiving coherer is not the true recipient of the electric wave or disturbance in the ether.

These later developments are, in fact, departures, more or less wide, from the principles underlying the Hertz demonstration. A vertical wire is charged to a high potential and discharges to earth over a spark gap. During the discharge the wire becomes a radiator of electro-magnetic pulses or waves, regardless of the spark radiation. The receiving vertical wire is likewise alone relied upon to absorb the energy. Being in the path of the electro-magnetic wave conveyed in the ether from the transmitting wire, it becomes the seat of electromotive forces which break down the coherer. This, in substance, may be considered as a series of small or microscopic spark gaps which can be crossed by the comparatively low potentials developed in the receiving wire. We are thus taught to recognize the fact that the refinements in methods and apparatus needed for a delicate physical demonstration as of the Hertz waves in this instance may often be laid aside in practical application, where the end to be achieved is different. The sudden discharge of the Marconi transmitting wire may possibly give rise to a series of oscillations or high-frequency alternating waves in the wire; but since the first half of the first wave at each discharge will have the greatest amplitude, it is doubtful if those which follow in the short train have any decided effect upon the receiver. According to this view the fact of the discharge being oscillatory may, indeed, have no essential relation to the work done,

but may be an unavoidable incident of the very sudden discharge which itself would set up a single pulse in the ether sufficiently intense for the work even if unaccompanied by lower amplitude oscillations folfowing the first discharge pulse.

Before leaving the consideration of this most fruitful field of experimental research opened by Hertz it may be stated that the one gap in the work yet to be filled is the actual production of electric waves of a wave length corresponding to those of the spectrum. If this could be done by some direct method, no matter how feeble the effect obtained, the experimental demonstrations of the electric nature of radiant heat and light would be fitly completed. Several years ago it occurred to me that it might be possible to devise a method for accomplishing the end in view, and so close the existing gap. Many years ago an observation on sound echoes showed clearly the production of high-pitch sounds from single pulses, or lower-pitch waves. A bridge over a mile in length was boarded at the sides, and vertical slats regularly and closely placed along its side formed, for a sound wave incident thereon, a series of reflecting edges or narrow vertical surfaces, a kind of coarse grating. It was found that a loud sound or pulse, such as that of a gunshot, emanating from a point near one end of the bridge and two to three hundred feet in a line from the structure, was followed by an echo, which was in reality a high-pitch musical tone. The pitch of this tone corresponded to the spacing of the slats in the bridge considered as a reflecting grating for sound.

Following this principle, it seems possible that a very sudden pulse in the ether or electromagnetic wave, incident at an angle upon a reflecting grating having from 20,000 to 40,000 ruled lines to the inch, if the plane of incidents were at right angles with the rulings, might be thrown into ripples of the wave length of light and yield a feeble luminosity. If the color then varied with the angle of incidence chosen and with the angle through which the reflection passed to the eye the experiment would be conclusive.

Despite the diligent studies which had been made in the invisible rays of the spectrum, both the ultrared and ultraviolet, a work far from completion as yet, the peculiar invisible radiation of the Crookes tube remained unknown until the work of Lenard and Röntgen brought it to the knowledge of the world. The cathode discharge, studied so effectively by Hittorf and Crookes, and by the latter called "radiant matter," was but a part of the whole truth in relation to the radiation in high vacua. It is needless to recount the steps in the discovery of Röntgen rays. We now know that these rays come from the impingement of the "radiant matter," or cathode rays. We know, also, that the higher the vacuum, and therefore the higher the electric potential needed to effect the discharge, the more penetrating or the less easily absorbed is the resulting radiation. Rays have

been produced which in part pass through cast iron nearly an inch thick. The iron acting as a filter absorbs all rays of less penetrating power. A question may here be put, which it will be for future experiment to answer: Can we, by increasing the degree of vacuum in a Crookes tube by the employment of enormous potentials for forcing a discharge through the higher vacuum, produce rays of greater and greater penetrating power? What, in fact, may be the limit—or is there any limit—to the diminution of the wave length in the ether, assuming for the moment that this invisible radiation is somewhat of the same nature as light, but of higher pitch, though it may be unlike light in not representing regular wave trains.

Röntgen radiation, while spoken of as invisible, is in reality easily visible if of great intensity. The parts of the retina which respond and so give the sensation of luminosity are apparently those around the eye and not directly opposite to the iris opening. Those parts of the retina sensitive to the rays are characterized by the preponderance of "rods," giving the simple sensation of illumination, apparently white in the case in question. The "cones," or those portions of the retinal membrane whose function is believed to be the recognition of color or differences of wave length, appear not to be excited by the Röntgen radiation, or only very feebly. If this be true it would account for the less intensity of the luminous effect upon those portions of the retina near the optic axis of the eye. All this favors the view that the Röntgen radiation is without sustained pitch or wave trains, and resembles more a sharp noise or crash in sound.

For pressing experimental work in the highest vacua to its limit, as above suggested, we already have means at command for the production of the most complete exhaustions, requiring extremely high potentials to pass an electric discharge. We have also in well-known forms of high-frequency apparatus the means for producing electromotive forces limited only by our means of insulation. A recent apparatus devised by me and called a dynamostatic machine gives equal capability of producing high potentials of definite polarity, positive and negative. It should not be long therefore before work is undertaken in this suggested direction of pressing this matter of rays of high penetrating power much farther than has been done. The question arises whether any such rays can exist which are not appreciably absorbed in passing through dense substances. They would probably not affect a photographic plate nor a fluorescent screen. If they lost also the property of ionizing a gas and causing electric convection we might not even be able to discover them. That some influence or action in the ether does actually penetrate the dense masses in space is evidenced by gravitation, the mystery of mysteries. We are, however, not justified in going beyond the proved facts which can only be the result of experimental work and close observation. All else is speculation. The energy

source of the Becquerel rays is another mystery apparently far from being cleared up, and if it be true, as recently announced, that a substance named radium has in reality 900 times the power of emitting these rays than is possessed by uranium and thorium, and that the radiation is able to cause visible fluorescence of barium platinocyanide, the mystery but deepens and makes us again think of the possible existence of obscure rays only absorbed and converted by a few special substances.

The diffusion which takes place when Röntgen rays pass through various media is another phenomenon which needs more attention from investigators. This effect seems to be produced by all substances in a greater or less degree. It, however, appears to be nearly absent in the case of those substances which give out light or fluoresce under the rays, as barium platinocyanide and calcium tungstate. It will be important to determine definitely whether the rays diffused by different substances are lowered in pitch or penetrating power as compared with the rays exciting the diffusion; whether, in other words, the rays from a tube with quite high vacuum excite similar rays by diffusion, or rays more absorbable, and if a lowering takes place whether it occurs in like manner and degree for all diffusing media.

The phenomenon may be akin to fluorescence, as when quinia sulphate converts the invisible ultraviolet rays of the spectrum into lower rays or visible light. This action may be at its extreme when barium platinocyanide, excited by Röntgen rays, so lowers the pitch as to produce rays within the visible spectrum, for this compound gives very little or no Röntgen-ray diffusion. Are there substances which under Röntgen rays fluoresce with invisible rays of the order of the ultraviolet of the spectrum? If, as is the case with solid paraffin, the irradiated substance gives rise to considerable diffusion, it can, as I have noted, produce a secondary diffusion in other masses of the same substance, or of other substances, as indicated by feeble fluorescence of the sensitive barium salt, thoroughly screened from the direct source of rays and from the first or primary diffusion. It is probable that Tertiary diffusion could be found if we possessed a far more powerful or continuous source of the rays for exciting the initial diffusion. The ray emission, even in the most powerfully excited tube, is probably so intermittent that the active period is but a fraction of the total time. It may easily be that the limit of intensity of Röntgen-ray emission has not yet been reached, especially when artificially cooled anticathode plates are available.

There is much room for experimental work in this fascinating field. We need for it the means for the production either of a continuous electric discharge at from 60,000 to 100,000 volts or a high-frequency apparatus capable of giving an unbroken wave train; that is, a succession of high period waves of current without breaks or intermissions.

The ordinary high-frequency apparatus for obtaining discharges of high potential from alternating currents gives only a rapid succession of discharges, each consisting of a few rapidly dampened oscillations. These discharges occupy but a small fraction of the total time. This is very different from a continuous sustained wave train, with the successive waves of equal amplitude following each other without break. Such sustained waves will, doubtless, be of use in research, especially in vacuum-tube work, and they would of course convey much more energy than the usual broken or interrupted discharge known as a high-frequency discharge.

Some six or seven years ago I endeavored, while working upon the subject of high frequency, to fill the gap. The result was an apparatus which, with its modifications, deserves more study and experiment than I have been able to give to it. A brief description may not be out of place. A large inductance coil with a heavy iron wire bundle for a core, a coil of relatively few turns with no iron core, and a condenser of variable capacity were connected in series across the mains of a 500-volt electric circuit. The smaller coreless coil and the condenser were arranged to be shunted by an adjustable spark gap with polished ball terminals. By simply closing for a moment the spark gap so as to form a low-resistance shunt around the condenser and the small coil and afterwards slowly separating the balls, the local circuit of the condenser, small coreless coil, and shunting gap become the seat of sustained oscillations, the frequency of which depends upon the relation of inductance and capacity in the local circuit. The energy supplied is that of a continuous current through the large inductance coil with the heavy core. The action of the apparatus is easily comprehended by a little study. The oscillating current in the local circuit may be made to induce much higher potentials in a secondary circuit inductively related thereto. In this case the turns of the secondary in relation to the primary are, as usual, such as to step-up the potential. In other words, the potential developed in the secondary is determined by the transforming ratio.

We thus have a high-frequency apparatus in which the waves are sustained in an unbroken series, and we employ as the source of energy a continuous current circuit. It shows that we may continuously supply energy to an oscillating system and so keep up the amplitude of electric oscillations, the frequency of which is that due to the capacity and inductance of the part of the circuit in which oscillations are set up.

While, in the forms of high-frequency apparatus alluded to, we may obtain almost any differences of electric potential up to millions of volts, assuming the apparatus large enough for the work, we do not get a sustained separation of positive and negative charges, as in the static machine, or in a less complete degree with the inductive coil. Professor Trowbridge, of Harvard, has, however, made use of large

Planté rheostatic machines, the condenser plates of which are charged in parallel from 10,000 small storage cells connected in series. The discharge of the condenser plates is effected after they are connected in series by a suitable connection changing frame moved for the purpose. Very high potential discharges are thus obtained and the polarity is always definite. It is manifest that the size of the apparatus and the perfection of its insulation determine the possible performance. The objection to such an apparatus for experimental research or demonstration is the large number of cells required and the complicated arrangements of circuits for charging them. I have, however, recently succeeded in removing all necessity for the presence of charging cells, and have produced what may be termed a dynamostatic machine which is worked by power or by current from a lighting circuit, either continuous or alternating, and may replace a static machine. It is, of course, not dependent upon the weather. I trust it may be of sufficient interest to merit the following brief description: A small electric motor has, in addition to its commutator, a pair of rings connected to its armature winding for obtaining alternating currents. The shaft of the motor drives synchronously a revolving frame bearing connections which, as in the Planté rheostatic machine, connect a series of condenser plates alternately in parallel for charging and in series for discharging at high potential. A small oil-immersed step-up transformer has its primary connected to the brushes bearing upon the two alternating current rings of the motor, and its secondary, giving say 20,000 volts, is periodically connected to the condenser plates while in parallel, by means of the revolving connection frame. The adjustment is such that only the tops of the alternating waves or their maxima are used to charge the condenser plates, while, also, those halves of the waves which are of the same polarity are alone used, the others being discarded or left on open circuit. The apparatus may be driven by power, in which case the electric motor becomes a dynamo, exciting its own field and supplying alternating current to the primary of the step-up transformer, or suitable alternating currents may drive it as a synchronous motor. Such a machine, run by continuous currents and having only eleven plates, gives sparks between its terminals over 12 inches long in rapid succession. It can be built cheaply, and is a highly instructive machine from the transformations it illustrates.

The machine is also arranged by the addition of a simple attachment so that it may be used to charge insulated bodies, or to charge Leyden-jar condensers or the like, replacing the ordinary static machines. It might, in fact, be used to charge a second range of condenser plates in another rheostatic machine to a potential of 100,000 volts, for example. These, after coupling in series or cascade, might be made to yield potentials beyond any thus far obtained.

The interest in such experimental apparatus and the results obtained

come largely from the apparent ability to secure a representation of the effects of lightning discharges upon a moderate scale, and the possibility of studying the action of air and other gases, as well as liquids and solids, at varying temperatures and pressures under high electric stresses. Broadly considered, however, the similarity of the effects to those produced in a thunder cloud is more apparent than real. The globules of water constituting the electrified cloud do not possess charges of millions of volts potential, the effects of which are seen in the stroke of lightning. The individual globules may possess only a moderate charge. When, however, they are massed together in a large extent of cloud the virtual potential of the cloud as a whole, with respect to the earth, may be enormous, though no part of the cloud possesses it. The cloud mass not being a conductor, its charge can not reside upon its outer surface or upon its lower surface nearest the earth, as with a large insulated conductor. The charge, in fact, exists throughout the mass, each globule of water suspended in the air having its small effect upon the total result.

When the cloud discharges, the main spark branches within and through the cloud mass in many directions. The discharge can at best be only a very partial one, from the nature of the case. These are conditions which are certainly not represented in our experimental production of high-potential phenomenon, except perhaps upon a very small scale in the electrified steam from Armstrong's hydro-electric machine, a type of apparatus now almost obsolete. Yet if we wish to reproduce as nearly as possible, upon a small scale, the conditions of the thunder cloud, we shall be compelled to again resort to it. In volcanic eruptions similar actions doubtless occur and give rise to the thunder clouds which often surround the gases sent out from the crater.

Considering, then, that the conditions in the thunder cloud are so different from those in our experiments with high potentials, we can easily understand that the study of lightning phenomena may present problems difficult to solve. Two forms at least of lightning discharge are quite unknown in the laboratory—namely, globular lightning and bead lightning, the latter the more rare of the two. Personally I can not doubt the existence of both of these rare forms of electric discharge, having received detailed accounts from eyewitnesses. On one occasion, while observing a thunder storm, I narrowly missed seeing the phenomenon of globular lightning, though a friend who was present, looking in the opposite direction, saw it. The explosion, however, was heard, and it consisted of a single detonation like the firing of a cannon. According to the testimony of an intelligent eyewitness, who described the rare phenomenon of bead lightning within an hour after it had been seen, it is a very beautiful luminous appearance, like a string of beads hung in a cloud, the beads being

somewhat elliptical and the ends of their axes in the line of their discharge being colored red and purple respectively. This peculiar appearance, not at any time dazzlingly bright, persisted for a few seconds, while fading gradually.

Again, our knowledge of the aurora is not as yet much more definite or precise than it is in regard to the obscure forms of lightning alluded to above. Whether these phenomena will ever be brought within the field of research by experimental methods is an open question.

The endeavor in the foregoing rather disconnected statements has been to indicate directions in which the field of experiment may be extended and to emphasize the fact that research must be carried on by extension of limits, necessitating more liberal endowment of research laboratories. I have tried to make it clear that the physicist must avail himself of the powers and energies set in play in the larger industrial enterprises, and finally that the field of possible exploration in physics by experimental methods has its natural boundaries, outside of which our advances in knowledge must be derived from a study of celestial bodies.

The riddle of gravitation is yet to be solved. This all-permeating force must be connected with other forces and other properties of matter. It will be a delicate task, indeed, for the total attraction between very large masses closely adjacent, aside from the earth's attraction, is very small.

Scientific facts are of little value in themselves. Their significance is their bearing upon other facts, enabling us to generalize and so to discover principles, just as the accurate measurement of the position of a star may be without value in itself, but in relation to other similar measurements of other stars may become the means of discovering their proper motions. We refine our instruments; we render more trustworthy our means of observation; we extend our range of experimental inquiry, and thus lay the foundation for the future work, with the full knowledge that, although our researches can not extend beyond certain limits, the field itself is, even within those limits, inexhaustible.

LIQUID HYDROGEN.[1]

By Professor DEWAR, M. A., LL. D., F. R. S., M. R. I.

From the year 1878, when the experiments of Cailletet and Pictet were attracting the attention of the scientific world, it became a common habit in text-books to speak of all the permanent gases, without any qualification, as having been liquefied; whereas these experimentalists, by the production of an instantaneous mist in a glass tube of small bore, or a transitory liquid jet in a gas expanding under high compression into air, had only adduced evidence that sooner or later the static liquid form of all the known gases would be attained. Neither Pictet nor Cailletet in their experiments ever succeeded in collecting any of the permanent gases in that liquid form for scientific examination. Yet we meet continually in scientific literature with expressions which lead one to believe that they did. For instance, the following extract from the Proceedings of the Royal Society, 1878, illustrates this point very well:

"This award (Davy medal) is made to these distinguished men (Cailletet and Pictet) for having independently and contemporaneously liquefied the whole of the gases hitherto called permanent."

Many other quotations of the same kind may be made. As a matter of fact six years elapsed, during which active investigation in this department was being prosecuted, before Wroblewski and Olszewski succeeded in obtaining oxygen as a static liquid, and to collect liquid hydrogen, which is a much more difficult problem, has taken just twenty years from the date of the Pictet and Cailletet experiments.

Wroblewski made the first conclusive experiment on the liquefaction of hydrogen in January, 1884. He found that the gas, cooled in a capillary glass tube to the boiling point of oxygen and expanded quickly from 100 to 1 atmosphere, showed the same appearance of sudden ebullition, lasting for a fraction of a second, as Cailletet had seen in his early oxygen experiments. No sooner had the announce ment been made than Olszewski confirmed the result by expanding

[1] Read at meeting of the Royal Institution of Great Britain, Friday, January 20, 1899. Reprinted from Proceedings of the Institution.

hydrogen from 190 atmospheres, previously cooled to the temperature given by liquid oxygen and nitrogen evaporating under diminished pressure. Olszewski, however, declared in 1884 that he saw colorless drops, and by partial expansion to 40 atmospheres the liquid hydrogen was seen by him running down the tube. Wroblewski could not confirm Olszewski's results, his hydrogen being always obtained in the form of what he called a "liquide dynamique," or the appearance of an instantaneous froth. Olszewski himself seven years later repeated his experiments of 1884 on a larger scale, confirming Wroblewski's results, thereby proving that the so-called liquid hydrogen of the earlier experiments must have been due to some impurity. The following extract from Wroblewski's paper states very clearly the results of his work on hydrogen:

"L'hydrogène soumis à la pression de 180 atm. jusqu'à 190 atm., refroidi par l'azote bouillant dans la vide (à la température de sa solidification) et détendu brusquement sous la pression atmosphérique présente une mousse bien visible. De la couleur grise de cette mousse, où l'œil ne peut distinguer des gouttelettes incolores, on ne peut pas encore deviner quelle apparence aurait l'hydrogène à l'état de liquide statique et l'on est encore moins autorisé à préciser s'il a ou non une apparence métallique. J'ai pu placer dans cette mousse ma pile thermo-électrique, et j'ai obtenu suivant les pressions employées des températures de $-208°$ jusqu'à $-211°$ C. Je ne peux pas encore dire dans quelle relation se trouvent ces nombres avec la température réelle de la mousse ou avec la température d'ébullition de l'hydrogène sous la pression atmosphérique, puisque je n'ai pas encore la certitude que la faible durée de ce phénomène ait permis à la pile de se refroidir complètement. Néanmoins, je crois aujourd'hui de mon devoir de publier ces résultats, afin de préciser l'état actuel de la question de la liquéfaction de l'hydrogène."[1]

It is well to note that the lowest thermo-electric temperature recorded by Wroblewski during the adiabatic expansion of the hydrogen (namely, $-211°$) is really equivalent to a much lower temperature on the gas-thermometer scale. The most probable value is $-230°$, and this must be regarded as the highest temperature of the liquid state, or the critical point of hydrogen, according to his observations. In a posthumous paper of Wroblewski's on "The compression of hydrogen," published in 1889, an account appears of further attempts which he had made to liquefy hydrogen. The gas compressed to 110 atmospheres, was cooled by means of liquid nitrogen under exhaustion to $-213.8°$. By suddenly reducing the pressure, as low a temperature as $-223°$ on his scale was recorded, but without any signs of liquefaction. This expansion gives a theoretical temperature of about 15° absolute in the gas particles. The above methods having failed to produce static hydrogen, Wroblewski suggested that the result might be attained by the use of hydrogen gas as a cooling agent. From this time until his death,

[1] Compt. Rend., 1885, 100, 981.

in the year 1888, Wroblewski devoted his time to a laborious research on the isothermals of hydrogen at low temperatures. The data thus arrived at enabled him, by the use of Van der Waal's formulæ, to calculate the critical constants, and also the boiling point of liquid hydrogen.

Olszewski returned to the subject in 1891, repeating and correcting his old experiments of 1884, which Wroblewski had failed to confirm, using now a glass tube 7 millimeters in diameter instead of one of 2 millimeters, as in the early trials. He says:

"On repeating my former experiments I had no hope of obtaining a lower temperature by means of any cooling agent, but I hoped that the expansion of hydrogen would be more efficacious on account of the larger scale on which the experiments were made."

The results of these experiments Olszewski describes as follows:

"The phenomenon of hydrogen ebullition, which was then observed, was much more marked and much longer than during my former investigations in the same direction. But even then I could not perceive any meniscus of liquid hydrogen."

Further:

"The reason for which it has not hitherto been possible to liquefy hydrogen in a static state, is that there exists no gas having a density between those of hydrogen and of nitrogen, and which might be, for instance, 7-10 (H=1). Such a gas could be liquefied by means of liquid oxygen or air as cooling agent, and be afterwards used as a frigorific menstruum in the liquefaction of hydrogen."

Professor Olszewski, in 1895, determined the temperature reached in the momentary adiabatic expansion of hydrogen at low temperatures, just as Wroblewski had done in 1885, only he employed a platinum-resistance thermometer instead of a thermo-junction. For this purpose he used a small steel bottle of 20 or 30 centimeters capacity, containing a platinum-resistance thermometer. In this way the temperatures registered were regarded as those of the critical and boiling points of liquid hydrogen, a substance which could not be seen under the circumstances and was only assumed to exist for a second or two during the expansion of the gaseous hydrogen in the small steel bottle.

The results arrived at by Wroblewski and Olszewski are given in the following table, and it will be shown later on that Wroblewski's constants are nearest the truth.

	Wroblewski, 1885.	Olszewski, 1895.
Critical temperature	—240°	—234°
Boiling point	—250°	—243°
Critical pressure	13 atm.	20 atm.

The accuracy of Wroblewski's deductions regarding the chief constants of liquid hydrogen, following from a study of the isothermals of the gas, is a signal triumph for the theory of Van der Waals and a monument to the genius of the Krakow physicist. From the results we may safely infer that, supposing a gas is hereafter discovered in small quantity four times more volatile than liquid hydrogen, having a boiling point of about 5° absolute, and therefore incapable of direct liquefaction by the use of liquid hydrogen, yet by a study of its isothermals we shall succeed in finding out its most important liquid constants, although the isolation of the real liquid may for the time be impossible.

In a paper published in the Philosophical Magazine, September, 1884, "On the liquefaction of oxygen and the critical volumes of fluids," the suggestion was made that the critical pressure of hydrogen was wrong, and that instead of being 99 atmospheres (as deduced by Sarrau from Amagat's isothermals) the gas had probably an abnormally low value for this constant. This view was substantially confirmed by Wroblewski finding the critical pressure of 13.3 atmospheres, or about one-fourth of that of oxygen. The Chemical News, September 7, 1894, contains an account of the stage the author's hydrogen experiment had reached at that date. The object was to collect liquid hydrogen at its boiling point, in an open vacuum vessel, which is a much more difficult problem than seeing it in a glass tube under pressure and at a higher temperature. In order to raise the critical point of hydrogen to about —210°, from 2 to 5 per cent of nitrogen or air was mixed with it. This is simply making an artificial gas containing a large proportion of hydrogen which is capable of liquefaction by the use of liquid air. The results are summed up in the following extract from the paper:

"One thing can, however, be proved by the use of the gaseous mixture of hydrogen and nitrogen, namely, that by subjecting it to a high compression at a temperature of —200° and expanding the resulting liquid into air, a much lower temperature than anything that has been recorded up to the present time can be reached. This is proved by the fact that such a mixed gas gives, under the conditions, a paste or jelly of solid nitrogen, evidently giving off hydrogen, because the gas coming off burns fiercely. Even when hydrogen containing only some 2 to 5 per cent of air is similarly treated, the result is a white solid matter (solid air) along with a clear liquid of low density, which is so exceedingly volatile that no known device for collecting it has been successful."

This was in all probability the first liquid hydrogen obtained, and the method is applicable to other difficultly liquefactible gases.

Continuing the investigations during the winter of 1894 and the greater part of 1895, the author read a paper before the Chemical Society in December of that year entitled "The liquefaction of air

LIQUID HYDROGEN APPARATUS.

and research at low temperatures,"[1] in which occasion was taken to describe for the first time the mode of production and use of a liquid hydrogen jet. * * *

The report of a Friday evening discourse on "New researches on liquid air"[2] contains a drawing of the apparatus employed for the production of a jet of hydrogen containing visible liquid. This is reproduced in fig. 1. A represents one of the hydrogen cylinders; B and C, vacuum vessels containing carbonic acid under exhaustion and liquid air, respectively; D is the coil; G, the pin-hole nozzle, and F, the valve. By means of this hydrogen jet liquid air can be quickly transformed into a hard solid. It was shown that such a jet could be used to cool bodies below the temperature that it is possible to reach by the use of liquid air, but all attempts to collect the liquid hydrogen from the jet in vacuum vessels failed. No other investigator improved on my results,[1] or has indeed touched the subject during the last three years. The type of apparatus used in these experiments worked well, so it was resolved to construct a much larger liquid-air plant and to combine with it circuits and arrangements for the liquefaction of hydrogen. This apparatus took a year to build, and many months have been occupied in the testing and preliminary trials. The many failures and defeats need not be detailed.

On May 10, 1898, starting with hydrogen cooled to $-205°$ and under a pressure of 180 atmospheres, escaping continuously from the nozzle of a coil of pipe at the rate of about 10 to 15 cubic feet per minute, in a vacuum vessel doubly silvered and of special construction, all surrounded with a space kept below $-200°$, liquid hydrogen commenced to drop from this vacuum vessel into another doubly isolated by being surrounded with a third vacuum vessel. In about five minutes 20 cubic centimeters of liquid hydrogen were collected, when the hydrogen jet froze up, from the accumulation of air in the pipes frozen out from the impure hydrogen. The yield of liquid was about 1 per cent of the gas. The hydrogen in the liquid condition is clear and colorless, showing no absorption spectrum, and the meniscus is as well defined as in the case of liquid air. The liquid must have a relatively high refractive index and dispersion, and the density appears at first sight to be in excess of the theoretical density, namely, 0.18 to 0.12, which we deduce respectively from the atomic volume of organic compounds and the limiting density found by Amagat for hydrogen gas under infinite compression. A preliminary attempt, however, to weigh a small glass bulb in the liquid made the density only about 0.08, or half the theoretical. My old experiments on the density of hydrogen in palladium gave a value for the combined element of 0.62. Not having arrangements at hand to determine the boiling point other than

[1] Proceedings of the Chemical Society, No. 158, 1895.

[2] Proceedings of the Royal Institution, 1896.

a thermo junction which gave entirely fallacious results, experiments were made to prove the excessively low temperature of the boiling fluid. In the first place, if a long piece of glass tubing, sealed at one end and open to the air at the other, is cooled by immersing the closed end in the liquid hydrogen, the tube immediately fills where it is cooled with solid air. A small glass tube filled with liquid oxygen when cooled in liquid hydrogen is transformed into a bluish white solid. This is a proof that the boiling point of hydrogen is much lower than any temperature previously reached by the use of liquid nitrogen evaporating in vacuo, seeing oxygen always remains liquid under such conditions. A first trial of putting liquid hydrogen under exhaustion gave no appearance of transition into the solid state. When the vacuum tube containing liquid hydrogen is immersed in liquid air so that the external wall of the vacuum vessel is maintained at about $-190°$, the hydrogen is found to evaporate at a rate not far removed from that of liquid air from a similar vacuum vessel under the ordinary conditions of temperature. This leads me to the conclusion that with proper isolation it will be possible to manipulate liquid hydrogen as easily as liquid air.

The boiling point of liquid hydrogen at atmospheric pressure in the first instance was determined by a platinum-resistance thermometer. This was constructed of pure metal and had a resistance of 5.3 ohms at 0° C., which fell to about 0.1 ohm when the thermometer was immersed in liquid hydrogen. The reduction of this resistance to normal air thermometer degrees gave the boiling points $-238.2°$ and $-238.9°$, respectively, by two extrapolation methods, and $-237°$ by a Dickson formula.[1] The boiling point of the liquid seems therefore to be $-238°$ C., or 35° absolute, and is thus about 5° higher than that obtained by Olszewski by the adiabatic expansion of the compressed gas and about 8° higher than that deduced by Wroblewski from Van der Waal's equation. From these results it may be inferred that the critical point of hydrogen is about 50° absolute, and that the critical pressure will probably not exceed 15 atmospheres.

If we assume the resistance reduced to zero, then the temperature registered by the electric thermometer ought to be $-244°$ C. At the boiling point of hydrogen registered by the electric-resistance thermometer, if the law correlating resistance and temperature can be pressed to its limits, a lowering of the boiling point of hydrogen by 5° or 6° C. would, therefore, produce a condition of affairs in which the platinum would have no resistance, or would become a perfect conductor. Now, we have every reason to believe that hydrogen, like other liquids, will boil at a lower temperature the lower the pressure under which it is volatilized. The question arises, how much lowering of the temperature can we practically anticipate? For this pur-

[1] See Philosophical Magazine, 45, 525, 1898.

FIG. 2.

FIG. 3.

LIQUID HYDROGEN EXPERIMENTS.

pose we have the boiling point given by the hydrogen-gas thermometer and critical data available, from which we can calculate an approximate vapor-pressure formula, accepting 22° absolute as about the boiling point, 33° absolute as the critical temperature, and 15.4 atmospheres as the critical pressure; then, as a first approximation—

$$\log. p = 6.410 - \frac{77.62}{T} \text{ mm} \ldots\ldots (1)$$

If, instead of using the critical pressure in the calculation, we assume the molecular latent heat of hydrogen to be proportional to the absolute boiling point, then, from a comparison with an expression of the same kind which gives accurate results for oxygen tensions below one atmosphere, we can derive another expression for hydrogen vapor pressures which ought to be applicable to boiling points under reduced pressure.

The resulting formula is—

$$\log. p = 7.0808 - \frac{88}{T} \text{ mm} \ldots\ldots (2)$$

Now formula (1) gives a boiling point of 14.2° absolute under a pressure of 25 millimeters, whereas the second equation (2) gives for the same pressure 15.4° absolute. As the absolute boiling point under atmospheric pressure is about 22°, both expressions lead to the conclusion that ebullition under 25 millimeters pressure ought to reduce the boiling point some 7° C. For some time experiments have been in progress with the object of determining the temperature of hydrogen boiling under about 25 millimeters pressure by the use of the platinum thermometer; but the difficulties encountered have been great, and repeated failures very exasperating. The troubles arise from the conduction of heat by the leads, the small latent heat of hydrogen volume for volume as compared with liquid air, the inefficiency of heat isolation, and the strain on the thermometer brought about by solid air freezing on it and distorting the coil of wire. In many experiments the result has been that all the liquid hydrogen has evaporated before the pressure was reduced to 25 millimeters, or the thermometer was left imperfectly covered. The apparatus employed will be understood from fig. 2. The liquid hydrogen collected in the vacuum vessel A was suspended in a larger vessel of the same kind B, which is so constructed that a spiral tube joins the inner and outer test tubes of which B is made, thereby making an opening into the interior at C. The resistance thermometer D and leads E pass through a rubber cork F, and the exhaustion takes place through C. In this way the cold vapors are drawn over the outside of the hydrogen vacuum vessel and this helps to isolate the liquid from the convective currents of gas. To effect proper isolation, the whole apparatus ought to be immersed in liquid

air under exhaustion. Arrangements of this kind add to the complication, so in the first instance the liquid was used as described. The liquid hydrogen evaporated quietly and steadily under a diminished pressure of about 25 millimeters. Naturally the liquid does not last long, so the resistance has to be taken quickly. Just before the reduction of pressure began the resistance of the thermometer was 0.131 ohm. This result compares favorably with the former observation on the boiling point, which gave a resistance of 0.129 ohm. On reducing the pressure the resistance diminished to 0.114 ohm, and kept steady for some time. The lowest reading of resistance was 0.112 ohm. This value corresponds to − 239.1° C., or only 1° lower on its own scale than the boiling point at atmospheric pressure, whereas the temperature ought to have been reduced at least 5° under the assumed exhaustion according to the gas-thermometer scale. The position of the observation on the curve of the relation of temperature and resistance for No. 7 thermometer is shown on the accompanying diagram (fig. 3). As a matter of fact, however, this platinum thermometer was, when placed in liquid hydrogen, cooled at starting below its own temperature of perfect conductivity, so that no exhaustion was needed to bring it to this point. The question arises, then, as to what is the explanation of this result. Has the platinum resistance thermometer arrived at a limiting resistance about the boiling point of hydrogen, so that at a lower temperature its changes in resistance become relatively small—the curve having become practically asymptotic to the axis of temperatnre? That is the most probable supposition, and it further explains the fact that the temperature of boiling hydrogen obtained by the linear extrapolation of the resistance temperature results in values that are not low enough.

As the molecular latent heats of liquids are proportional to their absolute boiling points, the latent heat of liquid hydrogen will be about two-fifths that of liquid oxygen. It will be shown later, however, that we can reach from 14° to 15° absolute by the evaporation of liquid hydrogen under exhaustion. From analogy it is probable that the practicable lowering of temperature to be obtained by evaporating liquid hydrogen under pressure of a few milimeters can not amount to more than 10° to 12° C., and it may be said with certainty that, assuming the boiling point 35° absolute to be correct, no means are at present known for approaching nearer than 20° to 25° to the absolute zero of temperature. The true boiling point is in reality about −252° C., in terms of the gas-thermometer scale, and the latent heat of the liquid is therefore about two-ninths that of an equal volume of oxygen, or one-fourth that of liquid nitrogen. The platinum-resistance thermometer had a zero point of −263.2 platinum degrees, and when immersed in boiling liquid hydrogen indicated a temperature of −256.8° on the same scale, or 6.4 platinum degrees from the point at which the metal

would theoretically become a perfect conductor. The effect of cooling platinum from the boiling point of liquid oxygen to that of liquid hydrogen is to diminish its resistance to one-eleventh.

The difficulties in liquefying hydrogen caused by the presence of air in the gas have been referred to,[1] and later experiments had for their object the removal of this fruitful source of trouble. This is by no means an easy task, as quantities amounting to only a fraction of 1 per cent accumulate in the solid state and eventually choke the nozzle of the apparatus, necessitating the abandonment of the operation. Later experiments enabled me to procure a larger supply of liquid hydrogen with which the determination of certain physical constants has been continued. The first observations made with a pure platinum-resistance thermometer had given $-238°$ as the boiling point. A new thermometer, constructed of platinum from a different source, gave practically the same value. As these results might be affected by some constant error, the determination was checked by employing a thermometer constructed from an alloy of rhodium and platinum, containing 10 per cent of the former. Alloys had been shown by Professor Fleming and the author to differ from pure metals in showing no sign of becoming perfect conductors at the absolute zero of temperature, and a study of the rhodium-platinum alloy had shown that the change in conductivity produced by cooling from $0°$ to the boiling point of liquid air is regular and may be represented by a straight line. As determined by the rhodium-platinum thermometer, the boiling point of hydrogen was found to be $-246°$, or some $8°$ lower than the platinum thermometer gave. Two ways of explaining the discrepancy between the two values suggested themselves. Pure platinum, although its resistance may be represented by a straight line almost down to the solidifying point of air, shows signs of a departure from regularity at about this point, and the curve may become asymptotic at lower temperatures. On the other hand, the resistance of the rhodium-platinum alloy diminishes less rapidly at these lower temperatures and is much higher than that of pure platinum under similar conditions. It follows that its resistance curve in all probability deviates less from a straight line than is the case with platinum. Either cause would explain the differences observed, but the lower boiling point ($-246°$, or $27°$ absolute) seemed to be the more probable, as it agreed very fairly with the value for the boiling point calculated by the author from Wroblewski's results. As the use of other pure metals or alloys was not likely to lead to more satisfactory results, the problem had to be attacked in a different way, namely, by means of an "air" thermometer containing hydrogen under diminished pressure.

A first attempt has been made at determining the boiling point by a constant-volume hydrogen thermometer working under dimin-

[1] Proceedings, 1898, 14, 130.

ished pressure. This thermometer, which gave the boiling point of oxygen as 90.5° absolute, or −182.5°, gave for hydrogen 21° absolute, or −252°. The three determinations that have been made are, then, as follows: (1) Pure-platinum resistance thermometer, 35° absolute; (2) rhodium-platinum resistance thermometer, 27° absolute; (3) hydrogen thermometer, 21° absolute. From this it appears that the boiling point of hydrogen is really lower than was anticipated, and must range between 20° and 22° absolute. Further experiments will be made with thermometers filled with hydrogen prepared from different sources. A hydrogen thermometer filled with the gas obtained from the evaporation of the liquid hydrogen itself must be employed.

The approximate density of liquid hydrogen at its boiling point was found by measuring the volume of the gas obtained by evaporating 10 cubic centimeters of the liquid, and is slightly less than 0.07, or about one-sixth that of liquid marsh gas, which is the lightest liquid known. It is remarkable that, with so low a density, liquid hydrogen is so easily seen, has so well defined a meniscus, and can be so readily collected and manipulated in vacuum vessels. As hydrogen occluded in palladium has a density of 0.62, it follows that it must be associated with the metal in some other state than that of liquefaction.

The atomic volume of liquid hydrogen at its boiling point is about 14.3, the atomic volumes of liquid oxygen and nitrogen being 13.7 and 16.6, respectively, at their boiling points. The weight of a liter of hydrogen gas at the boiling point of the liquid is about the same as that of air at the ordinary temperature. The ratio of the density of hydrogen gas at the boiling point to that of the liquid is approximately 1:60, as compared with a ratio of 1:255 in the case of oxygen under similar conditions.

The specific heat of hydrogen in the gaseous state and in hydrogenized palladium is 3.4, but may very probably be 6.4 in the liquid substance. Such a liquid would be unique in its properties; but as the volume of one gram of liquid hydrogen is about 14–15 cubic centimeters, the specific heat per unit volume must be nearly 0.5, which is about that of liquid air. It is highly probable, therefore, that the remarkable properties of liquid hydrogen predicted by theory will prove to be less astonishing when they are compared with those of liquid air, volume for volume, at corresponding temperatures.

With hydrogen as a cooling agent we shall get to from 13° to 15° of the zero of absolute temperature, and its use will open up an entirely new field of scientific inquiry. Even so great a man as James Clerk-Maxwell had doubts as to the possibility of ever liquefying hydrogen.[1] He says:

"Similar phenomena occur in all the liquefiable gases. In other gases we are able to trace the existence of attractive force at ordinary

[1] See Scientific Papers, 2, 412.

pressures, though the compression has not yet been carried so far as to show any repulsive force. In hydrogen the repulsive force seems to prevail even at ordinary pressures. This gas has never been liquefied, and it is probable that it never will be liquefied, as the attractive force is so weak."

In concluding his lectures on the nonmetallic elements, delivered at the Royal Institution in 1852, and published the following year, Faraday said:[1]

"There is reason to believe we should derive much information as to the intimate nature of these nonmetallic elements, if we could succeed in obtaining hydrogen and nitrogen in the liquid and solid form. Many gases have been liquefied; the carbonic acid gas has been solidified, but hydrogen and nitrogen have resisted all our efforts of the kind. Hydrogen in many of its relations acts as though it were a metal; could it be obtained in a liquid or a solid condition the doubt might be settled. This great problem, however, has yet to be solved, nor should we look with hopelessness on this solution when we reflect with wonder—and as I do almost with fear and trembling—on the powers of investigating the hidden qualities of these elements—of questioning them, making them disclose their secrets and tell their tales—given by the Almighty to man."

Faraday's expressed faith in the potentialities of experimental inquiry in 1852 has been justified forty-six years afterwards by the production of liquid hydrogen in the very laboratory in which all his epoch-making researches were executed. The "doubt" has now been settled; hydrogen does not possess in the liquid state the characteristics of a metal. No one can predict the properties of matter near the zero of temperature. Faraday liquefied chlorine in the year 1823. Sixty years afterwards, Wroblewski and Olszewski produced liquid air, and now, after a fifteen years' interval, the last of the old permanent gases, hydrogen, appears as a static liquid. Considering that the step from the liquefaction of air to that of hydrogen is relatively as great in the thermodynamic sense as that from liquid chlorine to liquid air, the fact that the former result has been achieved in one-fourth the time needed to accomplish the latter proves the greatly accelerated pace of scientific progress in our time.

The efficient cultivation of this field of research depends on combination and assistance of an exceptional kind; but in the first instance money must be available, and the members of the Royal Institution deserve my especial gratitude for their handsome donations to the conduct of this research. Unfortunately its prosecution will demand a further large expenditure. It is my duty to acknowledge that at an early stage of the inquiry the Hon. Company of Goldsmiths helped low-temperature investigation by a generous donation to the research fund.

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[1] See Faraday's Lectures on the Nonmetallic Elements, pp. 292-293.

During the whole course of the low-temperature work, carried out at the Royal Institution, the invaluable aid of Mr. Robert Lennox has been at my disposal, and it is not too much to say that, but for his engineering skill, manipulative ability, and loyal perseverance, the present successful issue might have been indefinitely delayed. My thanks are also due to Mr. J. W. Heath for valuable assistance in the conduct of the experiments.

SOME OF THE LATEST ACHIEVEMENTS OF SCIENCE.

By Sir William Crookes.

[Extract from address of Sir William Crookes, president of the British Association for the Advancement of Science, at Bristol meeting, 1898.]

* * * Having kept you for the last half hour rigorously chained to earth, disclosing dreary possibilities, it will be a relief to soar to the heights of pure science and to discuss a point or two touching its latest achievements and aspirations. The low-temperature researches which bring such renown to Professor Dewar and to his laboratory in the Royal Institution have been crowned during the present year by the conquest of one of nature's most defiant strongholds. On the 10th of last May Professor Dewar wrote to me these simple but victorious words: "This evening I have succeeded in liquefying both hydrogen and helium. The second stage of low-temperature work has begun." Static hydrogen boils at a temperature of 238° C. at ordinary pressure and at 250° C. in a vacuum, thus enabling us to get within 23° of absolute zero. The density of liquid hydrogen is only one-fourteenth that of water, yet in spite of such a low density it collects well, drops easily, and has a well-defined meniscus. With proper isolation it will be as easy to manipulate liquid hydrogen as liquid air.

The investigation of the properties of bodies brought near the absolute zero of temperature is certain to give results of extraordinary importance. Already platinum resistance thermometers are becoming useless, as the temperature of boiling hydrogen is but a few degrees from the point where the resistance of platinum would be practically nothing or the conductivity infinite.

Several years ago I pondered on the constitution of matter in what I ventured to call the fourth state. I endeavored to probe the tormenting mystery of the atom. What is the atom? Is a single atom in space solid, liquid, or gaseous? Each of these states involve ideas which can only pertain to vast collections of atoms. Whether, like Newton, we try to visualize an atom as a hard spherical body, or, with Boscovitch and Faraday, to regard it as a center of force, or accept the vortex atom theory of Lord Kelvin, an isolated atom is an unknown entity difficult to conceive. The properties of matter—solid, liquid,

gaseous—are due to molecules in a state of motion. Therefore matter as we know it involves essentially a mode of motion, and the atom itself—intangible, invisible, and inconceivable—is its material basis, and may, indeed, be styled the only true matter. The space involved in the motions of atoms has no more pretension to be called matter than the sphere of influence of a body of riflemen—the sphere filled with flying leaden missiles—has to be called lead. Since what we call matter essentially involves a mode of motion, and since at the temperature of absolute zero all atomic motions would stop, it follows that matter as we know it would at that paralyzing temperature probably entirely change its properties. Although a discussion of the ultimate absolute properties of matter is purely speculative, it can hardly be barren, considering that in our laboratories we are now within moderate distance of the absolute zero of temperature.

I have dwelt on the value and importance of nitrogen, but I must not omit to bring to your notice those little-known and curiously related elements which during the past twelve months have been discovered and partly described by Professor Ramsay and Dr. Travers. For many years my own work has been among what I may call the waste heaps of the mineral elements. Professor Ramsay is dealing with vagrant atoms of an astral nature. During the course of the present year he has announced the existence of no fewer than three new gases—krypton, neon, and metargon. Whether these gases, chiefly known by their spectra, are true unalterable elements, or whether they are compounded of other known or unknown bodies, has yet to be proved. Fellow-workers freely pay tribute to the painstaking zeal with which Professor Ramsay has conducted a difficult research and to the philosophic subtlety brought to bear on his investigations. But, like most discoverers, he has not escaped the flail of severe criticism.

There is still another claimant for celestial honors. Professor Nasini tells us he has discovered, in some volcanic gases at Pozzuoli, that hypothetical element, coronium, supposed to cause the bright line 5316.9 in the spectrum of the sun's corona. Analogy points to its being lighter and more diffusible than hydrogen, and a study of its properties can not fail to yield striking results. Still awaiting discovery by the fortunate spectroscopist are the unknown celestial elements, aurorium, with a characteristic line at 5570.7, and nebulum, having two bright lines at 5007.05 and 4959.02.

The fundamental discovery by Hertz of the electromagnetic waves predicted more than thirty years ago by Clerk Maxwell seems likely to develop in the direction of a practical application which excites keen interest—I mean the application to electric signaling across moderate distances without connecting wires. The feasibility of this method of signaling has been demonstrated by several experimenters at

more than one meeting of the British Association, though most elaborately and with many optical refinements by Oliver Lodge at the Oxford meeting in 1894. But not until Sig. Marconi induced the British post-office and foreign governments to try large scale experiments did wireless signaling become generally and popularly known or practically developed as a special kind of telegraphy. Its feasibility depends on the discovery of a singularly sensitive detector for Hertz waves—a detector whose sensitiveness in some cases seems almost to compare with that of the eye itself. The fact noticed by Oliver Lodge in 1889 that an infinitesimal metallic gap subjected to an electric jerk became conducting, so as to complete an electric circuit, was rediscovered soon afterwards in a more tangible and definite form and applied to the detection of Hertz waves by M. E. Branly. Oliver Lodge then continued the work, and produced the vacuum filing-tube coherers with automatic tapper back, which are of acknowledged practical service. It is this varying continuity of contact under the influence of extremely feeble electric stimulus alternating with mechanical tremor which, in combination with the mode of producing the waves revealed by Hertz, constitutes the essential and fundamental feature of "wireless telegraphy." There is a curious and widely spread misapprehension about coherers to the effect that to make a coherer work the wave must fall upon it. Oliver Lodge has disproved this fallacy. Let the wave fall on a suitable receiver, such as a metallic wire, or, better still, on an arrangement of metal wings resembling a Hertz sender, and the waves set up oscillating currents which may be led by wires (inclosed in metal pipes) to the coherer. The coherer acts apparently by a species of end impact of the oscillatory current, and does not need to be attacked in the flank by the waves themselves. This interesting method of signaling—already developing in Marconi's hands into a successful practical system which inevitably will be largely used in light-house and marine work—presents more analogy to optical signals by flash light than to what is usually understood as electric telegraphy, notwithstanding the fact that an ordinary Morse instrument at one end responds to the movements of a key at the other, or, as arranged by Alexander Muirhead, a siphon recorder responds to an automatic transmitter at about the rate of slow cable telegraphy. But although no apparent optical apparatus is employed, it remains true that the impulse travels from sender to receiver by essentially the same process as that which enables a flash of magnesium powder to excite a distant eye.

The phenomenon discovered by Zeeman, that a source of radiation is affected by a strong magnetic field in such a way that light of one refrangibility becomes divided usually into three components, two of which are displaced by diffraction analysis on either side of the mean position and are oppositely polarized to the third or residual con-

stituent, has been examined by many observers in all countries. The phenomenon has been subjected to photography with conspicuously successful results by Prof. T. Preston in Dublin and by Professor Michelson and Dr. Ames and others in America.

It appears that the different lines in the spectrum are differently affected, some of them being tripled with different grades of relative intensity, some doubled, some quadrupled, some sextupled, and some left unchanged. Even the two components of the D lines are not similarly influenced. Moreover, whereas the polarization is usually such as to indicate that motions of a negative ion or electron constitute the source of light, a few lines are stated by the observers at Baltimore, who used what they call the "small" grating of 5 inches width ruled with 65,000 lines, to be polarized in the reverse way.

Further prosecution of these researches must lead to deeper insight into molecular processes and the mode in which they affect the ether; indeed, already valuable theoretic views have been promulgated by H. A. Lorenz, J. Larmor, and G. F. Fizgerald, on the lines of the radiation theory of Dr. Johnstone Stoney, and the connection of the new phenomena with the old magnetic rotation of Faraday is under discussion. It is interesting to note that Faraday and a number of more recent experimenters were led by theoretical considerations to look for some such effect; and, though the inadequate means at their disposal did not lead to success, nevertheless a first dim glimpse of the phenomenon was obtained by M. Fievez, of the Royal Observatory at Brussels, in 1885.

It would be improper to pass without at least brief mention the remarkable series of theoretic papers by Dr. J. Larmor, published by the Royal Society, on the relationship between ether and matter. By the time these researches become generally intelligible they may be found to constitute a considerable step toward the further mathematical analysis and interpretation of the physical universe on the lines initiated by Newton.

In the mechanical construction of Röntgen-ray tubes I can record a few advances, the most successful being the adoption of Prof. Silvanus P. Thompson's suggestion of using for the anticathode a metal of high atomic weight. Osmium and iridium have been used with advantage, and osmium anticathode tubes are now a regular article of manufacture. As long ago as June, 1896, X-ray tubes with metallic uranium anticathodes were made in my own laboratory, and were found to work better than those with platinum. The difficulty of procuring metallic uranium prevented these experiments from being continued. Thorium anticathodes have also been tried.

Röntgen has drawn fresh attention to a fact very early observed by English experimenters—that of the nonhomogeneity of the rays and the dependence of their penetrating power on the degree of vacuum;

rays generated in high vacua have more penetrative power than when the vacuum is less high. These facts are familiar to all who have exhausted focus tubes on their own pumps. Röntgen suggests a convenient phraseology; he calls a low-vacuum tube, which does not emit the highly penetrating rays, a "soft" tube, and a tube in which the exhaustion has been pushed to an extreme degree, in which highly penetrating rays predominate, a "hard" tube. Using a "hard" tube, he took a photograph of a double-barreled rifle, and showed not only the leaden bullets within the steel barrels, but even the wads and the charges.

Benoit has reexamined the alleged relation between density and opacity to the rays, and finds certain discrepancies. Thus, the opacity of equal thicknesses of palladium and platinum are nearly equal, while their densities and atomic weights are very different, those of palladium being about half those of platinum.

At the last meeting of the British Association visitors saw—at the McGill University—Professors Cox and Callendar's apparatus for measuring the velocity of Röntgen rays. They found it to be certainly greater than 200 kilometers per second. Majorana has made an independent determination, and finds the velocity to be 600 kilometers per second with an inferior limit certainly of not less than 150 kilometers per second. It may be remembered that J. J. Thomson has found for cathode rays a velocity of more than 10,000 kilometers per second, and it is extremely unlikely that the velocity of Röntgen rays will prove to be less.

Trowbridge has verified the fact, previously announced by Prof. S. P. Thompson, that fluor-spar, which by prolonged heating has lost its powèr of luminescing when reheated, regains the power of thermoluminescence when exposed to Röntgen rays. He finds that this restoration is also effected by exposure to the electric-glow discharge, but not by exposure to the ultra-violet light. The difference is suggestive.

As for the action of Röntgen rays on bacteria, often asserted and often denied, the latest statement by Dr. H. Rieder, of Munich, is to the effect that bacteria are killed by the discharge from "hard" tubes. Whether the observation will lead to results of pathologic importance remains to be seen. The circumstance that the normal retina of the eye is slightly sensitive to the rays is confirmed by Dorn and by Röntgen himself.

The essential wave nature of the Röntgen rays appears to be confirmed by the fact, ascertained by several of our great mathematical physicists, that light of excessively short wave-length would be but slightly absorbed by ordinary material media and would not in the ordinary sense be refracted at all. In fact, a theoretic basis for a comprehension of the Röntgen rays had been propounded before the rays had been discovered. At the Liverpool meeting of the British Asso-

ciation several speakers, headed by Sir George Stokes, expressed their conviction that the disturbed electric field caused by the sudden stoppage of the motion of an electrically-charged atom yielded the true explanation of the phenomena extraneous to the Crookes high-vacuum tubes—phenomena so excellently elaborated by Lenard and by Röntgen. More recently Sir George Stokes has restated his "pulse" theory and fortified it with arguments which have an important bearing on the whole theory of the refraction of light. He still holds to their essentially transverse nature in spite of the absence of polarization—an absence once more confirmed by the careful experiments of Dr. L. Graetz. The details of this theory are in process of elaboration by Prof. J. J. Thomson.

Meantime, while the general opinion of physicists seems to be settling toward a wave or ether theory for the Röntgen rays, an opposite drift is apparent with respect to the physical nature of the cathode rays. It becomes more and more clear that cathode rays consist of electrified atoms or ions in rapid progressive motion. My idea of a fourth state of matter, propounded in 1881,[1] and at first opposed at home and abroad, is now becoming accepted. It is supported by Prof. J. J. Thomson.[2] Dr. Larmor's theory[3] likewise involves the idea of an ionic substratum of matter; the view is also confirmed by Zeeman's phenomenon. In Germany—where the term cathode rays was invented almost as a protest against the theory of molecular streams propounded by me at the Sheffield meeting of the British association in 1879—additional proofs have been produced in favor of the doctrine that the essential fact in the phenomenon is electrified radiant matter.

The speed of these molecular streams has been approximately measured, chiefly by aid of my own discovery nearly twenty years ago, that their path is curved in a magnetic field, and that they produce phosphorescence where they impinge on an obstacle. The two unknown quantities, the charge and the speed of each atom, are measurable from the amount of curvature and by means of one other independent experiment.

It can not be said that a complete and conclusive theory of these rays has yet been formulated. It is generally accepted that collisions among particles, especially the violent collisions due to their impact on a massive target placed in their path, give rise to the interesting kind of extremely high frequency radiation discovered by Röntgen. It has, indeed, for some time been known that, whereas a charged body in motion constitutes an electric current, the sudden stoppage, or any violent acceleration of such a body, must cause an alternating electric disturbance, which, though so rapidly decaying in intensity as to be practically "dead beat," yet must give rise to an ethereal wave or pulse

[1] Phil. Trans., part 2, 1881, pp. 433–434.
[2] Phil. Mag., October, 1897, p. 312.
[3] Phil. Mag., December, 1897, p. 506.

traveling with the speed of light, but of a length comparable to the size of the body whose sudden change of motion caused the disturbance. The emission of a high-pitched musical sound from the jolting of a dustman's cart (with a spring bell hung on it) has been suggested as an illustration of the way in which the molecules of any solid not at absolute zero may possibly emit such rays.

If the target onto which the electrically-charged atoms impinge is so constituted that some of its minute parts can thereby be set into rhythmical vibration, the energy thus absorbed reappears in the form of light, and the body is said to phosphoresce. The efficient action of the phosphorescent target appears to depend as much on its physical and molecular as on its chemical constitution. The best known phosphori belong to certain well-defined classes, such as the sulphides of the alkine-earthy metals and some of the so-called rare earths; but the phosphorescent properties of each of these groups are profoundly modified by an admixture of foreign bodies; witness the effect on the lines in the phosphorescent spectrum of yttrium and samarium produced by traces of calcium or lead. The persistence of the samarium spectrum in presence of overwhelming quantities of other metals is almost unexampled in spectroscopy; thus 1 part of samaria can easily be seen when mixed with 3,000,000 parts of lime.

Without stating it as a general rule, it seems as if with a nonphosphorescing target the energy of molecular impact reappears as pulses so abrupt and irregular that, when resolved, they furnish a copious supply of waves of excessively short wave-length—in fact, the now well-known Röntgen rays. The phosphorescence so excited may last only a small fraction of a second, as with the constituents of yttria, where the duration of the different lines varies between the 0.003 and the 0.0009 second; or it may linger for hours, as in the case of some of the yttria earths, and especially with the earthy sulphides, where the glow lasts bright enough to be commercially useful. Excessively phosphorescent bodies can be excited by light waves, but most of them require the stimulus of electrical excitement.

It now appears that some bodies, even without special stimulation, are capable of giving out rays closely allied, if not in some cases identical, with those of Professor Röntgen. Uranium and thorium compounds are of this character, and it would almost seem, from the important researches of Dr. Russell, that this ray-emitting power may be a general property of matter, for he has shown that nearly every substance is capable of affecting the photographic plate if exposed in darkness for sufficient time.

No other source for Röntgen rays but the Crookes tube has yet been discovered, but rays of kindred sorts are recognized. The Becquerel rays, emitted by uranium and its compounds, have now found their companions in rays—discovered almost simultaneously by Curie and

Schmidt—emitted by thorium and its compounds. The thorium rays affect photographic plates through screens of paper or aluminum and are absorbed by metals and other dense bodies. They ionize the air, making it an electrical conductor, and they can be refracted and probably reflected, at least diffusively. Unlike uranium rays, they are not polarized by transmission through tourmaline, therefore resembling in this respect the Röntgen rays.

Quite recently Monsieur and Madame Curie have announced a discovery which, if confirmed, can not fail to assist the investigation of this obscure branch of physics. They have brought to notice a new constituent of the uranium mineral pitchblende, which in a four hundred-fold degree possesses uranium's mysterious power of emitting a form of energy capable of impressing a photographic plate and of discharging electricity by rendering air a conductor. It also appears that the radiant activity of the new body, to which the discoverers have given the name of polonium, needs neither the excitation of light nor the stimulus of electricity; like uranium, it draws its energy from some constantly regenerating and hitherto unsuspected store, exhaustless in amount.

It has long been to me a haunting problem how to reconcile this apparently boundless outpour of energy with accepted canons. But, as Dr. Johnstone Stoney reminds me, the resources of molecular movements are far from exhausted. There are many stores of energy in nature that may be drawn on by properly constituted bodies without very obvious cause. Some time since I drew attention to the enormous amount of locked-up energy in the ether; nearer our experimental grasp are the motions of the atoms and molecules, and it is not difficult mentally so to modify Maxwell's demons as to reduce them to the level of an inflexible law and thus bring them within the ken of a philosopher in search of a new tool. It is possible to conceive a target capable of mechanically sifting from the molecules of the surrounding air the quick from the slow movers. This sifting of the swift moving molecules is effected in liquids whenever they evaporate, and in the case of the constituents of the atmosphere wherever it contains constituents light enough to drift away molecule by molecule. In my mind's eye I see such a target as a piece of metal cooler than the surrounding air acquiring the energy that gradually raises its temperature from the outstanding effect of all its encounters with the molecules of the air about it; I see another target of such a structure that it throws off the slow-moving molecules with little exchange of energy, but is so influenced by the quick-moving missiles that it appropriates to itself some of their energy. Let uranium or polonium, bodies of densest atoms, have a structure that enables them to throw off the slow-moving molecules of the atmosphere, while the quick-moving

molecules, smashing onto the surface, have their energy reduced and that of the target correspondingly increased. The energy thus gained seems to be employed partly in dissociating some of the molecules of the gas (or in inducing some other condition which has the effect of rendering the neighboring air in some degree a conductor of electricity) and partly in originating an undulation through the ether, which, as it takes its rise in phenomena so disconnected as the impacts of the molecules of the air, must furnish a large contingent of light waves of short wave-length. The shortness in the case of these Becquerel rays appears to approach without attaining the extreme shortness of ordinary Röntgen rays. The reduction of the speed of the quick-moving molecules would cool the layer of air to which they belong, but this cooling would rapidly be compensated by radiation and conduction from the surrounding atmosphere. Under ordinary circumstances the difference of temperature would scarcely be perceptible, and the uranium would thus appear to perpetually emit rays of energy with no apparent means of restoration.

The total energy of both the translational and internal motions of the molecules locked up in quiescent air at ordinary pressure and temperature is about 140,000 foot-pounds in each cubic yard of air. Accordingly, the quiet air within a room 12 feet high, 18 feet wide, and 22 feet long contains energy enough to propel a one-horse engine for more than twelve hours. The store drawn upon naturally by uranium and other heavy atoms only awaits the touch of the magic wand of science to enable the twentieth century to cast into the shade the marvels of the nineteenth.

While placing before you the labors and achievements of my comrades in science, I seize this chance of telling you of engrossing work of my own on the fractionation of yttria, to which for the last eighteen years I have given ceaseless attention. In 1883, under the title of "Radiant-matter spectroscopy," I described a new series of spectra produced by passing the phosphorescent glow of yttria, under molecular bombardment in vacuo, through a train of prisms. The visible spectra in time gave up their secrets, and were duly embalmed in the Philosophical Transactions. At the Birmingham meeting of the British Association, in 1886, I brought the subject before the chemical section, of which I had the honor to be president. The results led to many speculations on the probable origin of all the elementary bodies—speculations that for the moment I must waive in favor of experimental facts.

There still remained for spectroscopic examination a long, tempting stretch of unknown ultra-violet light, of which the exploration gave me no rest. But I will not now enter into details of the quest of unknown lines. Large quartz prisms, lenses and condensers, specially

sensitized photographic films capable of dealing with the necessary small amount of radiation given by feebly phosphorescing substances,[1] and, above all, tireless patience in collating and interpreting results, have all played their part. Although the research is incomplete, I am able to announce that among the groups of rare earths giving phosphorescent spectra in the visible region there are others giving well-defined groups of bands which can only be recorded photographically. I have detected and mapped no less than six such groups, extending to λ 3060.

Without enlarging on difficulties, I will give a brief outline of the investigation. Starting with a large quantity of a group of the rare earths in a state of considerable purity, a particular method of fractionation is applied, splitting the earths into a series of fractions differing but slightly from each other. Each of these fractions, phosphorescing in vacuo, is arranged in the spectrograph, and a record of its spectrum photographed upon a specially prepared sensitive film.

In this way, with different groups of rare earths, the several invisible bands were recorded—some moderately strong, others exceedingly faint. Selecting a portion giving a definite set of bands, new methods of fractionation were applied, constantly photographing and measuring the spectrum of each fraction. Sometimes many weeks of hard experiment failed to produce any separation, and then a new method of splitting up was devised and applied. By unremitting work—the solvent of most difficulties—eventually it was possible to split up the series of bands into various groups. Then, taking a group which seemed to offer possibilities of reasonably quick result, one method after another of chemical attack was adopted, with the ultimate result of freeing the group from its accompanying fellows and increasing its intensity and detail.

As I have said, my researches are far from complete, but about one of the bodies I may speak definitely. High up in the ultra-violet, like a faint nebula in the distant heavens, a group of lines was detected, at first feeble, and only remarkable on account of their isolation. On further purification these lines grew stronger. Their great refrangibility cut them off from other groups. Special processes were employed to isolate the earth, and using these lines as a test, and appealing at every step to the spectrograph, it was pleasant to see how each week the group stood out stronger and stronger, while the other lines of yttrium, samarium, ytterbium, etc., became fainter, and, at last, practically vanishing, left the sought-for group strong and solitary. Finally, within the last few weeks, hopefulness has emerged into cer-

[1] In this connection I am glad to acknowledge my indebtedness to Dr. Schumann, of Leipzig, for valuable suggestions and detail of his own apparatus, by means of which he has produced some unique records of metallic and gaseous spectra of lines of short wave-length.

tainty, and I have absolute evidence that another member of the rare earth groups has been added to the list. Simultaneously with the chemical and spectrographic attack, atomic-weight deminations were constantly performed.

As the group of lines which betrayed its existence stand alone, almost at the extreme end of the ultra-violet spectrum, I propose to name the newest of the elements Monium, from the Greek μόνος, alone. Although caught by the searching rays of the spectrum, monium offers a direct contrast to the recently discovered gaseous elements, by having a strongly marked individuality; but, although so young and willful, it is willing to enter into any number of chemical alliances.

Until my material is in a greater state of purity I hesitate to commit myself to figures, but I may say that the wave-lengths of the principal lines are 3120 and 3117. Other fainter lines are at 3219, 3064, and 3060. The atomic weight of the element, based on the assumption of R_2O_3, is not far from 118—greater than that accepted for yttrium and less than that for lanthanum.

I ought almost to apologize for adding to the already too long list of elements of the rare earth class—the asteroids of the terrestrial family. But as the host of celestial asteroids, unimportant individually, become of high interest when once the idea is grasped that they may be incompletely coagulated remains of the original nebula, so do these elusive and insignificant rare elements rise to supreme importance when we regard them in the light of component parts of a dominant element, frozen in embryo, and arrested in the act of coalescing from the original protyle into one of the ordinary and law-abiding family for whom Newlands and Mendeleeff have prepared pigeonholes. The new element has another claim to notice. Not only is it new in itself, but to discover it a new tool had to be forged for spectroscopic research.

Further details I will reserve for that tribunal before whom every aspirant for a place in the elemental hierarchy has to substantiate his claim.

AN EXPERIMENTAL STUDY OF RADIO-ACTIVE SUBSTANCES.[1]

By Henry Carrington Bolton.

Professor Röntgen's remarkable discovery, in 1895, of the penetrating rays called by him X-rays, but now equally well known by his own name, was followed in 1896 by Becquerel's discovery that the salts of uranium emit invisible radiations capable of discharging electrified bodies and of producing skiagraphic images on sensitive plates. He found that potassio-uranic sulfate emits rays that pass through black paper and affect photographic plates. This property is not limited to the brilliantly fluorescent uranic salts, but is shared by the nonfluorescent uranous salts. All uranium compounds examined proved to be active, whether phosphorescent or not, whether crystalline, melted, or in solution; and metallic uranium exhibits the phenomenon in a marked degree. The permanence of this property is amazing, substances kept in a double leaden box more than three years emitted rays having almost as much power as when first tested.

Shortly after the announcement by Becquerel, experimenters found that other substances have the power of emitting these "Becquerel rays." Henry found it in phosphorescent zinc sulfid, Niewenglowski in insolated calcium sulfid, Troost in artificial hexagonal blende, and Schmidt in thorium compounds. In 1898 Madame Sklodowska Curie, working in the laboratory of the Municipal School of Industrial Physics and Chemistry in Paris, devised a special apparatus for measuring the electrical conductivity of the air when under the influence of "radio-active bodies," and by its means studied the behavior of the minerals pitchblende, chalcolite, autunite, cleveite, monazite, orangeite, and thorite, and found them all active. Some varieties of pitchblende showed more than three times as much energy as metallic uranium itself, and this led her to the conclusion that the peculiar property was due to some unknown body contained in the mineral, and not to uranium compounds. Associating her husband with her, Madame and Monsieur Curie attacked the mineral pitchblende with acids and

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[1] Read at a meeting of the Chemical Society of Washington held April 21, 1900, at Baltimore, Md.

reagents, and soon obtained results that were presented by Becquerel to the Academy of Sciences, Paris, at a meeting held Monday, July 18, 1898.

These savants showed that pitchblende contains a substance, apparently analogous to bismuth, which emits Becquerel rays 4,000 times stronger than uranium. They were unable to isolate the element having radiant power, but they named it "Polonium," in honor of the native land of Madame Curie. In December of the same year the lady received the Gegner prize of 4,000 francs awarded her by the Academy of Sciences, and later in the same month Monsieur and Madame Curie, together with Monsieur Bémont, director of the Municipal Laboratory, announced the discovery of a second radio-active body in pitchblende, which they called "Radium." Since that date Madame Curie and her husband have industriously carried on investigations, publishing their results in the Comptes rendus; and some German physicists, not gallant enough to leave the enterprising woman a clear field, have announced a few minor discoveries. From these papers we gather the following facts concerning these marvelous bodies.

As yet comparatively little is known of the chemistry of the salts of polonium, since the radio-active substance has not been separated from its companions; on working up the mineral pitchblende the polonium is found in the precipitate thrown down by sulfuretted hydrogen, and insoluble in ammonium sulfid. Solutions of polonium react like those of bismuth, being precipitated by water.

The mixture of substances in which radium shows its activity has been more fully studied. The yet unknown element accompanies barium in analytical separations, its chlorid is wholly soluble in water, and it responds to the usual tests for barium. The spectrum of the substance shows the bands of barium, together with at least fifteen other lines peculiar to radium. (Demarçay, C. R., 129, 716, November 6, 1899.) Attempts to separate radium from barium have been unsuccessful, but by fractional precipitation of the mixed chlorids with alchohol a salt has been obtained having 900 times the activity of uranium.

By operating on half a ton of the residues of uranium minerals, Madame Curie obtained 2 kilograms of material rich in radium; with this attempts were made to determine the atomic weight of radium, and she found the figures 140, the atomic weight of barium being 136.4.

The extraordinary physical properties of the rays emitted by these bodies have commanded the most attention; they possess luminosity, actinic and skiagraphic power, and render the air through which they pass a conductor of electricity. This latter property, the one which led to their discovery, is studied by means of an electroscope of special construction. It consists essentially of a gold leaf (or aluminum-foil)

electroscope inclosed in a metallic box with glass sides and communicating with a metal disk exterior to the box. This disk lies in a horizontal plane a few centimeters above another parallel disk, which serves as a support for the substances under examination. When the electroscope is charged by rubbing the upper disk with a piece of ebonite the gold leaf diverges from the perpendicular and will remain so for some time if undisturbed; on placing a layer of a radio-active body on the insulated lower disk the air between the two disks becomes a conductor and the gold leaf at once resumes its normal position. To estimate the rapidity of the displacement of the gold leaf, a microscope fitted with a micrometer eyepiece is attached to the apparatus at right angles to the axis, and with the aid of a watch beating seconds the time is noted which the gold leaf takes to reach a certain point on the scale of the micrometer.

Becquerel first announced that the rays given out by uranium exhibited the phenomena of polarization, reflection, and refraction, but this was not confirmed by other observers, and on repeating his experiments with radium and with polonium Becquerel got contradictory and negative results. The French chemist observed that the rays emitted by different bodies are very unequally absorbed; the rays of radium and of uranium freely penetrate plates of quartz, fluorite, and mica, but those of polonium are absorbed by these minerals and scarcely penetrate paper. On the other hand, rays of polonium pass through aluminum more freely than those of uranium.

The rays of divers origin are also influenced in different ways by a magnetic field; in an irregular magnetic field formed by a powerful electro-magnet, the rays emitted by radium are deflected and concentrated on the poles. To show this Becquerel devised ingenious experiments giving photographic records. (C. R. 130, 996, December 11, 1899.) On examining the rays of polonium compounds (furnished by Madame Curie) he found that polonium acted differently from radium (C. R., December 26, 1899), and his results failed to confirm the observations of Geisel previously announced. Later Madame Curie also published a note on the dissimilar behavior of the rays of polonium and of radium in a magnetic field. (C. R. 130, 73, January 8, 1900.) The subject has also been studied at Vienna by Stefan Meyer and Egon R. von Schweidler. (Phys. Ztschr. 10, 113.)

Becquerel rays excite phosphorescence in gems, minerals, barium sulfid, calcium sulfid, etc.; in fluorite the phosphorescence remains twenty-four hours after the influence of the radium has been removed, exactly as when exposed to the light of the electric arc.

In studying the power that these rays have of communicating energy to inactive bodies, Madame Curie worked with substances so well purified that they were 50,000 times more powerful than uranium, and the

induced activity measured 1 to 50 times that of uranium; the substances examined were zinc, aluminum, brass foil, lead, platinum, bismuth, nickel, paper, barium carbonate, and bismuth sulfid. Her experiments showed that a true induction of radiant energy is effected, and the energy imparted to metallic plates is not removed by washing with water, although the radium chlorid ("chlorure de barium radifère") is soluble. The activity induced by Becquerel rays persists while that caused by Röntgen rays ceases suddenly on removal of the agent. (C. R. 129, 714, November 6, 1899.)

The actinic power of the rays is shown by exposing the salts to sensitive plates; with the relatively pure material obtained by Madame Curie an exposure of one-half minute sufficed to get an impression. The peculiar power of Röntgen rays is seen by using a barium-platino-cyanid fluoroscope, the rays exciting fluorescence through aluminum, vulcanite, etc. (C. R. 126, 1101, 1898; C. R. 127, 1215, December 26, 1898.)

Madame Curie records obtaining good "photo-impressions" with uranium, uranous oxid, pitchblende, chalcolite, etc., through glass, air, and aluminum.

The spontaneous luminosity of radium compounds was announced by Madame Curie to the Physical Society of Paris in March, 1899 (Rev. chim. pure et appliquée, July, 1899), and in November of that year she published her discovery that the wonderful rays exert chemical action. They transform oxygen into ozone. This was first noticed by the odor of the air in a flask in which radium compounds were confined, and was confirmed by the usual test with potassium iodid starch paper. The rays also produce a certain coloration in glass, changing it to violet; and they transform barium platino-cyanid from yellow to brown, in which state it is less fluorescent, but this can be revived by insolation. (C. R. 129, 823, November 30, 1899.)

At the suggestion of Madame and Monsieur Curie, Monsieur A. Debierne, working in the laboratory of the Sorbonne, examined pitchblende for other radio-active bodies, especially the portion precipitated from solution by ammonia and ammonium sulfid, after separation of the uranium. In October, 1899, he found associated with titanium a substance exhibiting 100,000 times more radiant power than uranium, and having chemical properties distinct from radium and polonium. The rays emitted by this body, named actinium, have the same manifold action as the other substances, with the exception that it is not self-luminous. (C. R. 129, 593, 1899.)

In a more recent paper (C. R. 130, April 12, 1900) Monsieur Debierne finds that actinium is allied to thorium, and suggests that the radio-activity of the latter is due to admixture of the new substance.

To complete this review of the radio-active bodies, brief notice must

be made of two papers by German chemists. Fritz Geisel obtained radium from uranium ores other than pitchblende, and remarks:

"Freshly crystallized barium salts containing radium are only slightly active, but in a few days or weeks they reach a maximum. They are strongest when anhydrous; moisture stops activity and heating restores it." (Ann. Phys. Chem. 69, 91, 1899.)

Becquerel rays have the same intensity in partial vacuum as at ordinary air pressure. This was proved by electrical and photographic experiments made by J. Elster and H. Geitel. (Wiedemann's Ann. 66, 135, 1898.)

Through the enterprise and liberality of the Smithsonian Institution, and by the courtesy of Secretary Langley, I have enjoyed the opportunity of studying small specimens of these rare and costly substances. They comprised 10 grams of "radio-active substances" in two portions, prepared by E. de Haën, manufacturing chemist, of Hanover, Germany, and 4 grams of "chlorure de barium radifère" and 4 grams of "polonium subnitrate" from the Société Centrale de Produits Chimiques (ancienne Maison Rousseau), Paris, said to be prepared according to the instructions of Madame and Monsieur Curie.

The samples from Hanover were marked "A" and "B," respectively, and a memorandum accompanying them stated that "B" excites fluorescence in barium-platino cyanid more energetically than "A," whereas the latter is self-luminous. As a matter of fact, I found both luminous in the dark, and "B" the brighter of the two.

The specimens were inclosed in hermetically sealed bottles and protected from light by strawboard cylinders. On removing the wrappings in a dark room both were seen to emit greenish-white light that gave to the enveloping papers a peculiar glow, similar to the fluorescence produced by Röntgen rays. I here call special attention to the fact that during all the time that I have had the substances under examination they have been kept in the dark, no light reaching them stronger than that of the yellow and orange-red of a photographic dark room, so that insolation has played no part in renewing their energy.

The grayish-white powders proved to be wholly soluble in water, and the solution gave the usual reactions for barium chlorid.

Moistening the radium chlorid with cold water does not immediately stop emission of light, but on heating to boiling the luminosity ceases. On expelling the water and heating in a platinum dish to dull redness, the material resumed its luminosity after a few days in the dark. The fact that radium compounds resume their power of emitting light slowly has been noted by Geisel, but he fails to state whether the salt regains its property without exposure to sunlight.

The substances "A" and "B" were examined with a fluoroscope at first without success, but in a perfectly dark room, after the eye

became sensitive, the screen of platino-barium cyanid was distinctly seen to fluoresce feebly.

The small specimens of these bodies had no perceptible influence in exciting phosphorescence of sulfids of the alkaline earths exposed to their action.

Having at hand no apparatus for measuring the electrical conductivity of the air, my experiments were chiefly directed to ascertaining the action of the rays on sensitive plates.

The photographic experiments were made with Seed nonhalation dry plates (No. 26). To test the approximate actinic power of the bodies "A" and "B," sections of sensitive plates at distances of 5 and 10 inches were exposed at intervals of from 2 to 12 minutes. These gave bands varying in intensity with the duration of action. "B" showed far greater power than "A." By exposing sensitive plates behind an ordinary negative to the entire 10 grams of "radium" from two to three hours, good transparencies were obtained. On substituting Eastman's bromide paper, prints were secured. The distance of the sensitive surfaces from the source of light was about 3 inches.

To get skiagraphic images, plates were enveloped in Carbutt's black paper (nonpermeable to light), and on this was laid a piece of tin foil cut in openwork pattern. After one hour's exposure a negative was obtained plainly showing the pattern. "A" was apparently stronger than "B."

Analogous experiments were carried out with the specimens of "radium" and of "polonium" from Paris. Making allowance for the difference in weight, the radium of German origin was about five times as active as the French. The sample labeled "polonium subnitrate" (weighing 4 grams) had positively no action on the plates used.

Having at my disposal 500 grams exceedingly well-purified uranic nitrate (remaining from previous researches), I examined it for Becquerel rays, but a sensitive plate exposed three hours to the beautifully fluorescent crystallized salt gave no trace of action.

Another incidental experiment may be here mentioned. Having seen it repeatedly stated that Röntgen rays exist in sunlight, I endeavored to test the assertion. I placed in brilliant rays of sun a sensitized plate (Seed, No. 26) inclosed in an ordinary plate holder, on which was laid a thick sheet lead perforated with half inch holes, in form of a quincunx. After six hours' exposure the plate was developed in the usual way and no image was obtained. The plate holder itself proved to be light tight.

The primary source of the energy manifested by these extraordinary substances has greatly puzzled physicists, and as yet remains a mystery. Madame Curie, speculating on the matter, at first proposed the following explanation: She conjectured that all space is continually traversed by rays analogous to Röntgen rays, but far more penetrative and not

capable of being absorbed by certain elements of high atomic weight, such as uranium and thorium.

Becquerel, reflecting on the marvelous spontaneous emission of light, remarked: If it can be proved that the luminosity causes no loss of energy, the state of the uranium is like that of a magnet which has been produced by an expenditure of energy and retains it indefinitely, maintaining around it a field in which transformation of energy can be effected; but the photographic reductions and the excitation of phosphorescence in a sensitive screen require an expenditure of energy, of which the source can only be in the radio-active substances. As this expenditure is slight, perhaps the bodies have a large reserve of energy which can be drawn upon for years without showing loss. At any rate, it has been impossible, says Becquerel, to bring about any appreciable variation in the intensity of the emission by physical influences.

Somewhat later, Becquerel hazarded the opinion that the radiation of radium is composed, at least in part, of cathodic rays; but these have been proved to be material, hence the induced activity must be caused by material particles impinging upon the substances excited. This materialistic theory seems to be confirmed by the results of ingenious experiments made by Madame and Monsieur Curie. They placed a sensitive plate beneath a salt of radium, supported upon a slab of lead, in the vicinity of an electro-magnet. Under these conditions when the current was passing the rays emitted by the chemical salt were bent in curved lines upon the sensitive plate, making impressions.

It may be objected, says a French writer in the Revue générale des Sciences, that this theory requires us to admit actual loss of particles of matter, nevertheless the charges are so feeble that the most intense radiation yet observed would require millions of years for the removal of one milligram of substance.

The same writer raises the question, Which of the observed phenomena is the primary one—does the radiation of radium excite cathodic rays, or do the latter exist in the chemical compounds? And he regards the latter as improbable. The primordial source of energy in radium probably resides, he adds, in the ultra-violet light, and the efflux of material particles that ensues is only a secondary phenomenon, but on a far larger scale than has previously been observed.

Speculations as to the future history and applications of these wonder-working bodies press upon even the dullest imagination. If a few grams of earth-born material, containing probably only a small percentage of the active body, emit light enough to affect the human eye and a photographic plate, as well as rays that penetrate with X-ray power, what degree of luminosity, of actinism, and of Röntgenism is to be expected from an hundredweight of the quintessence of energy purified from interfering matter?

And to what uses is this light-generating material to be applied? Are our bicycles to be lighted with disks of radium in tiny lanterns? Are these substances to become the cheapest form of light for certain purposes? Are we about to realize the chimerical dream of the alchemists—lamps giving light without consumption of oil, perpetually?

Seriously, in what direction is profound study of these substances going to lead us? Will it not greatly extend our knowledge of physical manifestations of energy and their correlation? What bearing will this power of "opening up paths through the air" for currents of electricity have upon our knowledge of heat, light, electricity, and those forms of energy called by the names of Röntgen and Becquerel?

In what corner of the globe will be found the cheap and convenient supply of the raw material yielding the radio-active bodies? Will not chemists be obliged to reexamine much known material by laboratory methods conducted in the dark? Many of us have worked up kilograms of pitchblende to extract uranium oxids, and in so doing have poured down the waste pipe or thrown into the dust bin the more interesting and precious radio-active bodies.

At all events, whatever the future may bring, physicists are deeply indebted to Becquerel, to Madame and Monsieur Curie, for placing in our hands new methods of research and for furnishing a novel basis for speculations destined to yield abundant fruits.

POSTSCRIPT.

Since writing the above an important announcement has become known by the arrival in America of a number of the Berichte der deutschen chemischen Gesellschaft, issued in Berlin May 14, 1900.

Bela von Lengyel, of Budapest, has pointed out that the chemical evidence is insufficient to establish the elementary character of these radio-active bodies and claims to have prepared the so-called "radium" synthetically. By fusing with the heat of the electric arc uranic nitrate mixed with 2 or 3 per cent of barium nitrate and treating the mass with nitric acid, water, and sulfuric acid, successively, he obtained radio-active barium sulfate possessing all the physical properties characteristic of the "element" announced by Madame Curie. The resulting substance gives out actinic rays, Röntgen rays, excites platino-cyanid screens, and causes air to conduct electricity.

The Hungarian chemist has made and examined the chlorid and the carbonate of this substance and finds they have the same properties. He wishes his paper regarded as a preliminary notice, proposing to continue his researches.

Admitting that radio-active bodies can be manufactured to order, are we any nearer explaining their mysterious powers?

Cosmos Club, Washington, D. C., *May 26, 1900.*

THE GROWTH OF SCIENCE IN THE NINETEENTH CENTURY.[1]

By Sir Michael Foster, K. C. B.

He who until a few minutes ago was your president said somewhere at the meeting at Bristol, and said with truth, that among the qualifications needed for the high honor of presidency of the British Association for the Advancement of Science, that of being old was becoming more and more dominant. He who is now attempting to speak to you feels that he is rapidly earning that distinction. But the association itself is older than its president; it has seen pass away the men who, wise in their generation, met at York on September 27, 1831, to found it; it has seen other great men who in bygone years served it as presidents, or otherwise helped it on, sink one after another into the grave. Each year, indeed, when it plants its flag as a signal of its yearly meeting, that flag floats half-mast high in token of the great losses which the passing year has brought. This year is no exception; the losses, indeed, are perhaps unwontedly heavy. I will not attempt to call over the sad roll call; but I must say a word about one who was above most others a faithful and zealous friend of the association. Sir Douglas Galton joined the association in 1860. From 1871 to 1895, as one of the general secretaries, he bore, and bore to the great good of the association, a large share of the burden of the association's work. How great that share was is perhaps especially known to the many men, among whom I am proud to count myself, who during his long term of office served in succession with him as brother general secretary. In 1895, at Ipswich, he left the post of general secretary, but only to become president. So long and so constantly did he labor for the good of the association that he seemed to be an integral part of it, and meeting as we do to-day, and as we henceforward must do, without Douglas Galton, we feel something greatly missing. This year, perhaps even more than in other years, we could have wished him to be among us; for to-day the association may look with joy, not unmixed with pride, on the realization of a project in forwarding which it has had

[1]Address by the president of the British Association for the Advancement of Science at the Dover meeting, 1899. From Report of the Association for 1899, pp. 3–23.

a conspicuous share, on the commencement of an undertaking which is not only a great thing in itself, but which, we trust, is the beginning of still greater things to come. And the share which the association has had in this was largely Sir Douglas Galton's doing. In his address as president of section A, at the meeting of the association at Cardiff in 1891, Prof. Oliver Lodge expounded with pregnant words how urgently, not pure science only, but industry and the constructive arts—for the interests of these are ever at bottom the same—needed the aid of some national establishment for the prosecution of prolonged and costly physical researches, which private enterprise could carry out in a lame fashion only, if at all. Lodge's words found an echo in many men's minds, but the response was for a long while in men's minds only. In 1895, Sir Douglas Galton, having previously made a personal study of an institution analogous to the one desired—namely, the Reichsanstalt at Berlin—seized the opportunity offered to him as president of the association at Ipswich to insist, with the authority not only of the head for the time being of a great scientific body, but also of one who himself knew the ways and wants at once of science and of practical life, that the thing which Lodge and others had hoped for was a thing which could be done, and ought to be done at once. And now to-day we can say it has been done. The National Physical Laboratory has been founded. The address at Ipswich marked the beginning of an organized effort which has at last been crowned with success. A feeling of sadness can not but come over us when we think that Sir Douglas Galton was not spared to see the formal completion of the scheme whose birth he did so much to help, and which, to his last days, he aided in more ways than one. It is the old story—the good which men do lives after them.

Still older than the association is this nineteenth century, now swiftly drawing to its close. Though the century itself has yet some sixteen months to run, this is the last meeting of the British association which will use the numbers 1800 to mark its date.

The eyes of the young look ever forward; they take little heed of the short though ever-lengthening fragment of life which lies behind them; they are wholly bent on that which is to come. The eyes of the aged turn wistfully again and again to the past; as the old glide down the inevitable slope, their present becomes a living over again the life which has gone before, and the future takes on the shape of a brief lengthening of the past. May I this evening venture to give rein to the impulses of advancing years? May I, at this last meeting of the association in the eighteen hundreds, dare to dwell for a while upon the past, and to call to mind a few of the changes which have taken place in the world since those autumn days in which men were saying to each other that the last of the seventeen hundreds was drawing toward its end?

Dover in the year of our Lord 1799 was in many ways unlike the

Dover of to-day. On moonless nights men groped their way in its narrow streets by the help of swinging lanterns and smoky torches, for no lamps lit the ways. By day the light of the sun struggled into the houses through narrow panes of blurred glass. Though the town then, as now, was one of the chief portals to and from the countries beyond the seas, the means of travel were scanty and dear, available for the most part to the rich alone, and for all beset with discomfort and risk. Slow and uncertain was the carriage of goods, and the news of the world outside came to the town (though it, from its position, learnt more than most towns) tardily, fitfully, and often falsely. The people of Dover sat then much in dimness, if not in darkness, and lived in large measure on themselves. They who study the phenomena of living beings tell us that light is the great stimulus of life and that the fullness of the life of a being or of any of its members may be measured by the variety, the swiftness, and the certainty of the means by which it is in touch with its surroundings. Judged from this standpoint, life at Dover then, as indeed elsewhere, must have fallen far short of the life of to-day.

The same study of living beings, however, teaches us that while from one point of view the environment seems to mold the organism, from another point the organism seems to be master of its environment. Going behind the change of circumstances, we may raise the question, the old question, Was life in its essence worth more then than now? Has there been a real advance?

Let me at once relieve your minds by saying that I propose to leave this question in the main unanswered. It may be, or it may not be, that man's grasp of the beautiful and of the good, if not looser, is not firmer than it was a hundred years ago. It may be, or it may not be, that man is no nearer to absolute truth, to seeing things as they really are, than he was then. I will merely ask you to consider with me for a few minutes how far and in what ways man's laying hold of that aspect of or part of truth which we call natural knowledge, or sometimes science, differed in 1799 from what it is to-day, and whether that change must not be accounted a real advance, a real improvement in man.

I do not propose to weary you by what in my hands would be the rash effort of attempting a survey of all the scientific results of the nineteenth century. It will be enough if for a little while I dwell on some few of the salient features distinguishing the way in which we nowadays look upon, and during the coming week shall speak of, the works of nature around us—though those works themselves, save for the slight shifting involved in a secular change, remain exactly the same—from the way in which they were looked upon and might have been spoken of at a gathering of philosophers at Dover in 1799, and I ask your leave to do so.

In the philosophy of the ancients earth, fire, air, and water were called "the elements." It was thought, and rightly thought, that a knowledge of them and of their attributes was a necessary basis of a knowledge of the ways of nature. Translated into modern language, a knowledge of these "elements" of old means a knowledge of the composition of the atmosphere, of water, and of all the other things which we call matter, as well as a knowledge of the general properties of gases, liquids, and solids, and of the nature and effects of combustion. Of all these things our knowledge to-day is large and exact, and, though ever enlarging, in some respects complete. When did that knowledge begin to become exact?

To-day the children in our schools know that the air which wraps round the globe is not a single thing, but is made up of two things, oxygen and nitrogen,[1] mingled together. They know, again, that water is not a single thing, but the product of two things, oxygen and hydrogen, joined together. They know that when the air makes the fire burn and gives the animal life, it is the oxygen in it which does the work. They know that all round them things are undergoing that union with oxygen which we call oxidation, and that oxidation is the ordinary source of heat and light. Let me ask you to picture to yourselves what confusion there would be to-morrow, not only in the discussions at the sectional meetings of our association, but in the world at large, if it should happen that in the coming night some destroying touch should wither up certain tender structures in all our brains and wipe out from our memories all traces of the ideas which cluster in our minds around the verbal tokens, oxygen and oxidation. How could any of us, not the so-called man of science alone, but even the man of business and the man of pleasure, go about his ways lacking those ideas? Yet those ideas were in 1799 lacking to all but a few.

Although in the third quarter of the seventeenth century the light of truth about oxidation and combustion had flashed out in the writings of John Mayow, it came as a flash only, and died away as soon as it had come. For the rest of that century, and for the greater part of the next, philosophers stumbled about in darkness, misled for the most of the time by the phantom conception which they called phlogiston. It was not until the end of the third quarter of the eighteenth century that the new light, which has burned steadily ever since, lit up the minds of the men of science. The light came at nearly the same time from England and from France. Rounding off the sharp corners of controversy, and joining, as we may fitly do to-day, the two countries as twin bearers of a common crown, we may say that we owe the truth to Priestley, to Lavoisier, and to Cavendish. If it was Priestley who was the first to demonstrate the existence of what we now call oxygen, it is to Lavoisier we owe the true conception of the nature of oxidation

[1] Some may already know that there is at least a third thing, argon.

and the clear exposition of the full meaning of Priestley's discovery, while the knowledge of the composition of water, the necessary complement of the knowledge of oxygen, came to us through Cavendish and, we may perhaps add, through Watt.

The date of Priestley's discovery of oxygen is 1774, Lavoisier's classic memoir "On the nature of the principle which enters into combination with metals during calcination" appeared in 1775, and Cavendish's paper on the composition of water did not see the light until 1784.

During the last quarter of the eighteenth century this new idea of oxygen and oxidation was struggling into existence. How new was the idea is illustrated by the fact that Lavoisier himself at first spoke of that which he was afterwards, namely, in 1778, led to call oxygen, the name by which it has since been known, as "the principle which enters into combination." What difficulties its acceptance met with is illustrated by the fact that Priestley himself refused to the end of his life to grasp the true bearings of the discovery which he had made. In the year 1799 the knowledge of oxygen, of the nature of water and of air, and indeed the true conception of chemical composition and chemical change, was hardly more than beginning to be, and the century had to pass wholly away before the next great chemical idea, which we know by the name of the atomic theory of John Dalton, was made known. We have only to read the scientific literature of the time to recognize that a truth which is now not only woven as a master thread into all our scientific conceptions, but even enters largely into the everyday talk and thoughts of educated people, was a hundred years ago struggling into existence among the philosophers themselves. It was all but absolutely unknown to the large world outside those select few.

If there be one word of science which is writ large on the life of the present time, it is the word "electricity." It is, I take it, writ larger than any other word. The knowledge which it denotes has carried its practical results far and wide into our daily life, while the theoretical conceptions which it signifies pierce deep into the nature of things. We are to-day proud, and justly proud, both of the material triumphs and of the intellectual gains which it has brought us, and we are full of even larger hopes of it in the future.

At what time did this bright child of the nineteenth century have its birth?

He who listened to the small group of philosophers of Dover, who in 1799 might have discoursed of natural knowledge, would perhaps have heard much of electric machines, of electric sparks, of the electric fluid, and even of positive and negative electricity; for frictional electricity had long been known and even carefully studied. Probably one or more of the group, dwelling on the observations which Galvani, an Italian, had made known some twenty years before, developed views on

the connection of electricity with the phenomena of living bodies. Possibly one of them was exciting the rest by telling how he had just heard that a professor at Pavia, one Volta, had discovered that electricity could be produced not only by rubbing together particular bodies, but by the simple contact of two metals, and had thereby explained Galvani's remarkable results. For, indeed, as we shall hear from Professor Fleming, it was in that very year, 1799, that electricity as we now know it took its birth. It was then that Volta brought to light the apparently simple truths out of which so much has sprung. The world, it is true, had to wait for yet some twenty years before both the practical and theoretic worth of Volta's discovery became truly pregnant under the fertilizing influence of another discovery. The loadstone and magnetic virtues had, like the electrifying power of rubbed amber, long been an old story. But, save for the compass, not much had come from it. And even Volta's discovery might have long remained relatively barren had it been left to itself. When, however, in 1819, Oersted made known his remarkable observations on the relations of electricity to magnetism, he made the contact needed for the flow of a new current of ideas. And it is perhaps not too much to say that those ideas, developing during the years of the rest of the century with an ever-accelerating swiftness, have wholly changed man's material relations to the circumstances of life, and at the same time carried him far in his knowledge of the nature of things.

Of all the various branches of science, none perhaps is to-day, none for these many years past has been, so well known to, even if not understood by, most people as that of geology. Its practical lessons have brought wealth to many; its fairy tales have brought delight to more; and round it hovers the charm of danger, for the conclusions to which it needs touch on the nature of man's beginning.

In 1799 the science of geology, as we now know it, was struggling into birth. There had been from of old cosmogonies, theories as to how the world had taken shape out of primeval chaos. In that fresh spirit which marked the zealous search after natural knowledge pursued in the middle and latter part of the seventeenth century, the brilliant Stenson, in Italy, and Hooke, in our own country, had laid hold of some of the problems presented by fossil remains, and Woodward, with others, had labored in the same field. In the eighteenth century, especially in its latter half, men's minds were busy about the physical agencies determining or modifying the features of the earth's crust; water and fire, subsidence from a primeval ocean and transformation by outbursts of the central heat, Neptune and Pluto, were being appealed to, by Werner on the one hand and by Desmarest on the other, in explanation of the earth's phenomena. The way was being prepared, theories and views were abundant, and many sound observations had been made; and yet the science of geology, properly so

called, the exact and proved knowledge of the successive phases of the world's life, may be said to date from the closing years of the eighteenth century.

In 1783 James Hutton put forward in a brief memoir his Theory of the Earth, which, in 1795, two years before his death, he expanded into a book; but his ideas failed to lay hold of men's minds until the century had passed away, when, in 1802, they found an able expositor in John Playfair. The very same year that Hutton published his theory, Cuvier came to Paris and almost forthwith began, with Brongniart, his immortal researches into the fossils of Paris and its neighborhood. And four years later, in the year 1799 itself, William Smith's tabular list of strata and fossils saw the light. It is, I believe, not too much to say that out of these geology, as we now know it, sprang. It was thus in the closing years of the eighteenth century that was begun the work which the nineteenth century has carried forward to such great results, but at this time only the select few had grasped the truth, and even they only the beginning of it. Outside a narrow circle the thoughts even of the educated about the history of the globe were bounded by the story of the deluge—though the story was often told in a strange fashion—or were guided by fantastic views of the plastic forces of a sportive nature.

In another branch of science, in that which deals with the problems presented by living beings, the thoughts of men in 1799 were also very different from the thoughts of men to-day. It is a very old quest, the quest after the knowledge of the nature of living beings, one of the earliest on which man set out; for it promised to lead him to a knowledge of himself, a promise which perhaps is still before us, but the fulfillment of which is yet far off. As time has gone on, the pursuit of natural knowledge has seemed to lead man away from himself into the furthermost parts of the universe, and into secret workings of Nature in which he appears to be of little or no account; and his knowledge of the nature of living things, and so of his own nature, has advanced slowly, waiting till the progress of other branches of natural knowledge can bring it aid. Yet in the past hundred years the biologic sciences, as we now call them, have marched rapidly onward.

We may look upon a living body as a machine doing work in accordance with certain laws, and may seek to trace out the working of the inner wheels, how these raise up the lifeless dust into living matter, and let the living matter fall away again into dust, giving out movement and heat. Or we may look upon the individual life as a link in a long chain, joining something which went before to something about to come, a chain whose beginning lies hid in the farthest past, and may seek to know the ties which bind one life to another. As we call up to view the long series of living forms, living now or flitting like shadows on the screen of the past, we may strive to lay hold of the

influences which fashion the garment of life. Whether the problems of life are looked upon from the one point of view or the other, we to-day, not biologists only but all of us, have gained a knowledge hidden even from the philosophers a hundred years ago.

Of the problems presented by the living body viewed as a machine, some may be spoken of as mechanical, others as physical, and yet others as chemical, while some are, apparently at least, none of these. In the seventeenth century William Harvey, laying hold of the central mechanism of the blood stream, opened up a path of inquiry which his own age and the century which followed trod with marked success. The knowledge of the mechanics of the animal and of the plant advanced apace, but the physical and chemical problems had yet to wait. The eighteenth century, it is true, had its physics and its chemistry; but, in relation at least to the problems of the living being, a chemistry which knew not oxygen and a physics which knew not the electricity of chemical action were of little avail. The philosopher of 1799, when he discussed the functions of the animal or of the plant involving chemical changes, was fain for the most part, as were his predecessors in the century before, to have recourse to such vague terms as "fermentation" and the like; to-day our treatises on physiology are largely made up of precise and exact expositions of the play of physical agencies and chemical bodies in the living organisms. He made use of the words "vital force" or "vital principle" not as an occasional, but as a common explanation of the phenomena of the living body. During the present century, especially during its latter half, the idea embodied in those words has been driven away from one seat after another; if we use it now when we are dealing with the chemical and physical events of life, we use it with reluctance, as a *deus ex machina* to be appealed to only when everything else has failed.

Some of the problems—and those, perhaps, the chief problems—of the living body have to be solved neither by physical nor chemical methods, but by methods of their own. Such are the problems of the nervous system. In respect to these the men of 1799 were on the threshold of a pregnant discovery. During the latter part of the present century, and especially during its last quarter, the analysis of the mysterious processes in the nervous system, and especially in the brain, which issue as feeling, thought, and the power to move, has been pushed forward with a success conspicuous in its practical, and full of promise in its theoretical, gains. That analysis may be briefly described as a following up of threads. We now know that what takes place along a tiny thread which we call a nerve fiber differs from that which takes place along its fellow-threads, that differing nervous impulses travel along different nervous fibers, and that nervous and psychical events are the outcome of the clashing of nervous impulses as they sweep along the closely woven web of living threads

of which the brain is made. We have learned by experiment and by observation that the pattern of the web determines the play of the impulses, and we can already explain many of the obscure problems not only of nervous disease, but of nervous life, by an analysis which is a tracking out the devious and linked path of nervous threads. The very beginning of this analysis was unknown in 1799. Men knew that nerves were the agents of feeling and of the movements of muscles; they had learned much about what this part or that part of the brain could do; but they did not know that one nerve fiber differed from another in the very essence of its work. It was just about the end of the past century, or the beginning of the present one, that an English surgeon began to ponder over a conception which, however, he did not make known until some years later, and which did not gain complete demonstration and full acceptance until still more years had passed away. It was in 1811, in a tiny pamphlet published privately, that Charles Bell put forth his New Idea that the nervous system was constructed on the principle that "the nerves are not single nerves possessing various powers, but bundles of different nerves whose filaments are united for the convenience of distribution, but which are distinct in office as they are in origin from the brain."

Our present knowledge of the nervous system is to a large extent only an exemplification and expansion of Charles Bell's New Idea, and has its origin in that.

If we pass from the problems of the living organism viewed as a machine to those presented by the varied features of the different creatures who have lived or who still live on the earth, we at once call to mind that the middle years of the present century mark an epoch in biologic thought such as never came before, for it was then that Charles Darwin gave to the world the Origin of Species.

That work, however, with all the far-reaching effects which it has had, could have had little or no effect, or, rather, could not have come into existence, had not the earlier half of the century been in travail preparing for its coming. For the germinal idea of Darwin appeals, as to witnesses, to the results of two lines of biologic investigation which were almost unknown to the men of the eighteenth century.

To one of these lines I have already referred. Darwin, as we know, appealed to the geological record; and we also know how that record, imperfect as it was then, and imperfect as it must always remain, has since his time yielded the most striking proofs of at least one part of his general conception. In 1799 there was, as we have seen, no geological record at all.

Of the other line I must say a few words.

To-day the merest beginner in biologic study, or even that exemplar of acquaintance without knowledge, the general reader, is aware that every living being, even man himself, begins its independent existence

as a tiny ball, of which we can, even acknowledging to the full the limits of the optical analysis at our command, assert with confidence that in structure, using that word in its ordinary sense, it is in all cases absolutely simple. It is equally well known that the features of form which supply the characters of a grown-up living being, all the many and varied features of even the most complex organism, are reached as the goal of a road, at times a long road, of successive changes; that the life of every being, from the ovum to its full estate, is a series of shifting scenes, which come and go, sometimes changing abruptly, sometimes melting the one into the other, like dissolving views, all so ordained that often the final shape with which the creature seems to begin, or is said to begin, its life in the world is the outcome of many shapes, clothed with which it in turn has lived many lives before its seeming birth.

All or nearly all the exact knowledge of the labored way in which each living creature puts on its proper shape and structure is the heritage of the present century. Although the way in which the chick is molded in the egg was not wholly unknown even to the ancients, and in later years had been told, first in the sixteenth century by Fabricius, then in the seventeenth century in a more clear and striking manner by the great Italian naturalist, Malpighi, the teaching thus offered had been neglected or misinterpreted. At the close of the eighteenth century the dominant view was that in the making of a creature out of the egg there was no putting on of wholly new parts, no epigenesis. It was taught that the entire creature lay hidden in the egg, hidden by reason of the very transparency of its substance, lay ready-made, but folded up, as it were, and that the process of development within the egg or within the womb was a mere unfolding, a simple evolution. Nor did men shrink from accepting the logical outcome of such a view—namely, that within the unborn creature itself lay in like manner, hidden and folded up, its offspring also, and within that again its offspring in turn, after the fashion of a cluster of ivory balls carved by Chinese hands, one within the other. This was no fantastic view put forward by an imaginative dreamer; it was seriously held by sober men, even by men like the illustrious Haller, in spite of their recognizing that as the chick grew in the egg some changes of form took place. Though so early as the middle of the eighteenth century Friedrich Casper Wolff and, later on, others had strenuously opposed such a view, it held its own not only to the close of the century, but far on into the next. It was not until a quarter of the present century had been added to the past that Von Baer made known the results of researches which once and for all swept away the old view. He and others working after him made it clear that each individual puts on its final form and structure not by an unfolding of preexisting hidden features, but by the formation of new parts through

the continued differentiation of a primitively simple material. It was also made clear that the successive changes which the embryo undergoes in its progress from the ovum to maturity are the expression of morphologic laws, that the progress is one from the general to the special, and that the shifting scenes of embryonic life are hints and tokens of lives lived by ancestors in times long past.

If we wish to measure how far off in biologic thought the end of the last century stands, not only from the end, but even from the middle of this one, we may imagine Darwin striving to write the Origin of Species in 1799. We may fancy him being told by philosophers explaining how one group of living beings differed from another group because all its members and all their ancestors came into existence at one stroke when the first-born progenitor of the race, within which all the rest were folded up, stood forth as the result of a creative act. We may fancy him listening to a debate between the philosopher who maintained that all the fossils strewn in the earth were the remains of animals or plants churned up in the turmoil of a violent universal flood, and dropped in their places as the waters went away, and him who argued that such were not really the "spoils of living creatures," but the products of some playful plastic power which out of the superabundance of its energy fashioned here and there the lifeless earth into forms which imitated, but only imitated, those of living things. Could he amid such surroundings, by any flight of genius have beat his way to the conception for which his name will ever be known?

Here I may well turn away from the past. It is not my purpose, nor, as I have said, am I fitted, nor is this perhaps the place, to tell even in outline the tale of the work of science in the nineteenth century. I am content to have pointed out that the two great sciences of chemistry and geology took their birth, or at least began to stand alone, at the close of the last century, and have grown to be what we know them now within about a hundred years, and that the study of living beings has within the same time been so transformed as to be to-day something wholly different from what it was in 1799. And, indeed, to say more would be to repeat almost the same story about other things. If our present knowledge of electricity is essentially the child of the nineteenth century, so also is our present knowledge of many other branches of physics. And those most ancient forms of exact knowledge, the knowledge of numbers and of the heavens, whose beginning is lost in the remote past, have, with all other kinds of natural knowledge, moved onward during the whole of the hundred years with a speed which is ever increasing. I have said, I trust, enough to justify the statement that in respect to natural knowledge a great gulf lies between 1799 and 1899. That gulf, moreover, is a twofold one: Not only has natural knowledge been increased, but men have run to and fro spreading it as they go. Not only have the few

driven far back round the full circle of natural knowledge the dark clouds of the unknown which wrap us all about, but also the many walk in the zone of light thus increasingly gained. If it be true that the few to-day are, in respect to natural knowledge, far removed from the few of those days, it is also true that nearly all which the few alone knew then, and much which they did not know, has now become the common knowledge of the many.

What, however, I may venture to insist upon here is that the difference in respect to natural knowledge, whatever be the case with other differences between then and now, is undoubtedly a difference which means progress. The span between the science of that time and the science of to-day is beyond all question a great stride onward.

We may say this, but we must say it without boasting. For the very story of the past which tells of the triumphs of science bids the man of science put away from him all thoughts of vainglory, and that by many tokens.

Whoever, working at any scientific problem, has occasion to study the inquiries into the same problem made by some fellow-worker in the years long gone by, comes away from that study humbled by one or other of two different thoughts. On the one hand, he may find, when he has translated the language of the past into the phraseology of to-day, how near was his forerunner of old to the conception which he thought, with pride, was all his own, not only so true but so new. On the other hand, if the ideas of the investigator of old, viewed in the light of modern knowledge, are found to be so wide of the mark as to seem absurd, the smile which begins to play upon the lips of the modern is checked by the thought, Will the ideas which I am now putting forth, and which I think explain so clearly, so fully, the problem in hand, seem to some worker in the far future as wrong and as fantastic as do these of my forerunner to me? In either case his personal pride is checked. Further, there is written clearly on each page of the history of science, in characters which can not be overlooked, the lesson that no scientific truth is born anew, coming by itself and of itself. Each new truth is always the offspring of something which has gone before, becoming in turn the parent of something coming after. In this aspect the man of science is unlike, or seems to be unlike, the poet and the artist. The poet is born, not made; he rises up, no man knowing his beginnings; when he goes away, though men after him may sing his songs for centuries, he himself goes away wholly, having taken with him his mantle, for this he can give to none other. The man of science is not thus creative; he is created. His work, however great it be, is not wholly his own; it is in part the outcome of the work of men who have gone before. Again and again a conception which has made a name great has come not so much by the man's own effort as out of the fullness of time.

Again and again we may read in the words of some man of old the outlines of an idea which in later days has shone forth as a great acknowledged truth. From the mouth of the man of old the idea dropped barren, fruitless; the world was not ready for it, and heeded it not; the concomitant and abutting truths which could give it power to work were wanting. Coming back again in later days, the same idea found the world awaiting it; things were in travail preparing for it, and some one, seizing the right moment to put it forth again, leaped into fame. It is not so much the men of science who make science as some spirit which, born of the truths already won, drives the man of science onward and uses him to win new truths in turn.

It is because each man of science is not his own master, but one of many obedient servants of an impulse which was at work long before him, and will work long after him, that in science there is no falling back. In respect to other things there may be times of darkness and times of light; there may be risings, decadences, and revivals. In science there is only progress. The path may not be always a straight line; there may be swerving to this side and to that; ideas may seem to return again and again to the same point of the intellectual compass; but it will always be found that they have reached a higher level—they have moved, not in a circle, but in a spiral. Moreover, science is not fashioned as is a house, by putting brick to brick, that which is once put remaining as it was put to the end. The growth of science is that of a living being. As in the embryo, phase follows phase, and each member or body puts on in succession different appearances, though all the while the same member, so a scientific conception of one age seems to differ from that of a following age, though it is the same one in the process of being made; and as the dim outlines of the early embryo become, as the being grows more distinct and sharp, like a picture on a screen brought more and more into focus, so the dim gropings and searchings of the men of science of old are by repeated approximations wrought into the clear and exact conclusions of later times.

The story of natural knowledge, of science, in the nineteenth century, as, indeed, in preceding centuries, is, I repeat, a story of continued progress. There is in it not so much as a hint of falling back, not even of standing still. What is gained by scientific inquiry is gained forever; it may be added to, it may seem to be covered up, but it can never be taken away. Confident that the progress will go on, we can not help peering into the years to come and straining our eyes to foresee what science will become and what it will do as they roll on. While we do so, the thought must come to us, Will all the increasing knowledge of nature avail only to change the ways of man; will it have no effect on man himself?

The material good which mankind has gained and is gaining through

the advance of science is so imposing as to be obvious to everyone, and the praises of this aspect of science are to be found in the mouths of all. Beyond all doubt science has greatly lessened and has markedly narrowed hardship and suffering; beyond all doubt science has largely increased and has widely diffused ease and comfort. The appliances of science have, as it were, covered with a soft cushion the rough places of life, and that not for the rich only, but also for the poor. So abundant and so prominent are the material benefits of science that in the eyes of many these seem to be the only benefits which she brings. She is often spoken of as if she were useful and nothing more, as if her work were only to administer to the material wants of man.

Is this so?

We may begin to doubt it when we reflect that the triumphs of science which bring these material advantages are in their very nature intellectual triumphs. The increasing benefits brought by science are the results of man's increasing mastery over nature, and that mastery is increasingly a mastery of mind; it is an increasing power to use the forces of what we call inanimate nature in place of the force of his own or other creatures' bodies; it is an increasing use of mind in place of muscle.

Is it to be thought that that which has brought the mind so greatly into play has had no effect on the mind itself? Is that part of the mind which works out scientific truths a mere slavish machine, producing results it knows not how, having no part in the good which in its workings it brings forth?

What are the qualities, the features, of that scientific mind which has wrought, and is working, such great changes in man's relation to nature? In seeking an answer to this question we have not to inquire into the attributes of genius. Though much of the progress of science seems to take on the form of a series of great steps, each made by some great man, the distinction in science between the great discoverer and the humble worker is one of degree only, not of kind. As I was urging just now, the greatness of many great names in science is often, in large part, the greatness of occasion, not of absolute power. The qualities which guide one man to a small truth silently taking its place among its fellows, as these go to make up progress, are at bottom the same as those by which another man is led to something of which the whole world rings.

The features of the fruitful scientific mind are in the main three.

In the first place, above all other things, his nature must be one which vibrates in unison with that of which he is in search; the seeker after truth must himself be truthful, truthful with the truthfulness of nature. For the truthfulness of nature is not wholly the same as that which man sometimes calls truthfulness. It is far more imperious, far more exacting. Man, unscientific man, is often content with "the

nearly" and "the almost." Nature never is. It is not her way to call the same two things which differ, though the difference may be measured by less than a thousandth of a milligram or of a millimeter, or by any other like standard of minuteness. And the man who, carrying the ways of the world into the domain of science, thinks that he may treat nature's differences in any other way than she treats them herself, will find that she resents his conduct; if he, in carelessness or in disdain, overlooks the minute difference which she holds out to him as a signet to guide him in his search, the projecting tip, as it were, of some buried treasure, he is bound to go astray, and the more strenuously he struggles on the farther he will find himself from his true goal.

In the second place, he must be alert of mind. Nature is ever making signs to us; she is ever whispering to us the beginnings of her secrets; the scientific man must be ever on the watch, ready at once to lay hold of nature's hint, however small; to listen to her whisper, however low.

In the third place, scientific inquiry, though it be preeminently an intellectual effort, has need of the moral quality or courage—not so much the courage which helps a man to face a sudden difficulty as the courage of steadfast endurance. Almost every inquiry, certainly every prolonged inquiry, sooner or later goes wrong. The path, at first so straight and clear, grows crooked and gets blocked; the hope and enthusiasm, or even the jaunty ease, with which the inquirer set out, leave him, and he falls into a slough of despond. That is the critical moment calling for courage. Struggling through the slough, he will find on the other side the wicket gate opening up the real path; losing heart, he will turn back and add one more stone to the great cairn of the unaccomplished.

But, I hear some one say, these qualities are not the peculiar attributes of the man of science; they may be recognized as belonging to almost everyone who has commanded or deserved success, whatever may have been his walk of life. That is so. That is exactly what I would desire to insist, that the men of science have no peculiar virtues, no special powers. They are ordinary men, their characters are common, even commonplace. Science, as Huxley said, is organized common sense, and men of science are common men drilled in the ways of common sense. For their life has this feature. Though in themselves they are no stronger, no better than other men, they possess a strength which, as I just now urged, is not their own, but is that of the science whose servants they are. Even in his apprenticeship the scientific inquirer, while learning what has been done before his time, if he learns it aright, so learns it that what is known may serve him not only as a vantage ground whence to push off into the unknown, but also as a compass to guide him in his course. And when fitted for his work he enters on inquiry itself, what a zealous, anxious guide,

what a strict and, because strict, helpful schoolmistress does Nature make herself to him! Under her care every inquiry, whether it bring the inquirer to a happy issue or seem to end in naught, trains him for the next effort. She so orders her ways that each act of obedience to her makes the next act easier for him, and step by step she leads him on toward that perfect obedience which is complete mastery.

Indeed, when we reflect on the potency of the discipline of scientific inquiry we cease to wonder at the progress of scientific knowledge. The results actually gained seem to fall so far short of what under such guidance might have been expected to have been gathered in that we are fain to conclude that science has called to follow her, for the most part, the poor in intellect and the wayward in spirit. Had she called to her service the many acute minds who have wasted their strength struggling in vain to solve hopeless problems, or who have turned their energies to things other than the increase of knowledge; had she called to her service the many just men who have walked straight without the need of a rod to guide them, how much greater than it has been would have been the progress of science, and how many false teachings would the world have been spared! To men of science themselves, when they consider their favored lot, the achievements of the past should serve not as a boast, but as a reproach.

If there be any truth in what I have been urging, that the pursuit of scientific inquiry is itself a training of special potency, giving strength to the feeble and keeping in the path those who are inclined to stray, it is obvious that the material gains of science, great as they may be, do not make up all the good which science brings or may bring to man. We especially, perhaps, in these later days, through the rapid development of the physical sciences, are too apt to dwell on the material gains alone. As a child in its infancy looks upon its mother only as a giver of good things, and does not learn till in after days how she was also showing her love by carefully training it in the way it should go, so we, too, have thought too much of the gifts of science, overlooking her power to guide.

Man does not live by bread alone, and science brings him more than bread. It is a great thing to make two blades of grass grow where before one alone grew; but it is no less great a thing to help a man to come to a just conclusion on the questions with which he has to deal. We may claim for science that while she is doing the one she may be so used as to do the other also. The dictum just quoted, that science is organized common sense, may be read as meaning that the common problems of life which common people have to solve are to be solved by the same methods by which the man of science solves his special problems. It follows that the training which does so much for him may be looked to as promising to do much for them. Such aid can come from science on two conditions only. In the first place, this her influence must be acknowledged; she must be duly recognized as a

teacher no less than as a hewer of wood and a drawer of water. And the pursuit of science must be followed, not by the professional few only, but at least in such measure as will insure the influence of example by the many. But this latter point I need not urge before this great association, whose chief object during more than half a century has been to bring within the fold of science all who would answer to the call. In the second place, it must be understood that the training to be looked for from science is the outcome, not of the accumulation of scientific knowledge, but of the practice of scientific inquiry. Man may have at his fingers' ends all the accomplished results and all the current opinions of any one or of all the branches of science, and yet remain wholly unscientific in mind; but no one can have carried out even the humblest research without the spirit of science in some measure resting upon him. And that spirit may in part be caught even without entering upon an actual investigation in search of a new truth. The learner may be led to old truths, even the oldest, in more ways than one. He may be brought abruptly to a truth in its finished form, coming straight to it like a thief climbing over the wall; and the hurry and press of modern life tempt many to adopt this quicker way. Or he may be more slowly guided along the path by which the truth was reached by him who first laid hold of it. It is by this latter way of learning the truth, and by this alone, that the learner may hope to catch something at least of the spirit of the scientific inquirer.

This is not the place, nor have I the wish, to plunge into the turmoil of controversy; but if there be any truth in what I have been urging, then they are wrong who think that in the schooling of the young science can be used with profit only to train those for whom science will be the means of earning their bread. It may be that from the point of view of pedagogic art the experience of generations has fashioned out of the older studies of literature an instrument of discipline of unusual power, and that the teaching of science is as yet but a rough tool in unpracticed hands. That, however, is not an adequate reason why scope should not be given for science to show the value which we claim for it as an intellectual training fitted for all sorts and conditions of men. Nor need the studies of humanity and literature fear her presence in the schools, for if her friends maintain that the teaching is one-sided, and therefore misleading, which deals with the doings of man only, and is silent about the works of nature, in the sight of which he and his doings shrink almost to nothing, she herself would be the first to admit that that teaching is equally wrong which deals only with the works of nature and says nothing about the doings of man, who is, to us at least, nature's center.

There is yet another general aspect of science on which I would crave leave to say a word. In that broad field of human life which we call politics, in the struggle not of man with man, but of race with race, science works for good. If we look only on the surface it may

at first sight seem otherwise. In no branch of science has there during these later years been greater activity and more rapid progress than in that which furnishes the means by which man brings death, suffering, and disaster on his fellowmen. If the healer can look with pride on the increased power which science has given him to alleviate human suffering and ward off the miseries of disease, the destroyer can look with still greater pride on the power which science has given him to sweep away lives and to work desolation and ruin; while the one has slowly been learning to save units, the other has quickly learned to slay thousands. But, happily, the very greatness of the modern power of destruction is already becoming a bar to its use, and bids fair—may we hope before long—wholly to put an end to it; in the words of Tacitus, though in another sense, the very preparations for war, through the character which science gives them, make for peace.

Moreover, not in one branch of science only, but in all, there is a deep undercurrent of influence sapping the very foundations of all war. As I have already urged, no feature of scientific inquiry is more marked than the dependence of each step forward on other steps which have been made before. The man of science can not sit by himself in his own cave weaving out results by his own efforts, unaided by others, heedless of what others have done and are doing. He is but a bit of a great system, a joint in a great machine, and he can only work aright when he is in due touch with his fellow workers. If his labor is to be what it ought to be, and is to have the weight which it ought to have, he must know what is being done, not by himself, but by others, and by others not of his own land and speaking his tongue only, but also of other lands and of other speech. Hence it comes about that to the man of science the barriers of manners and of speech which pen men into nations become more and more unreal and indistinct. He recognizes his fellow-worker, wherever he may live, and whatever tongne he may speak, as one who is pushing forward shoulder to shoulder with him toward a common goal, as one whom he is helping and who is helping him. The touch of science makes the whole world kin.

The history of the past gives us many examples of this brotherhood of science. In the revival of learing throughout the sixteenth and seventeenth centuries, and some way on into the eighteenth century, the common use of the Latin tongue made intercourse easy. In some respects in those earlier days science was more cosmopolitan than it afterwards became. In spite of the difficulties and hardships of travel, the men of science of different lands again and again met each other face to face, heard with their ears, and saw with their eyes what their brethren had to say or show. The Englishman took the long journey to Italy to study there; the Italian, the Frenchman, and the German wandered from one seat of learning to another; and many a man held a chair in a country not his own. There was help, too, as well as intercourse. The Royal Society of London took upon itself the task of

publishing nearly all the works of the great Italian, Malpighi, and the brilliant Lavoisier, two years before his own countrymen in their blind fury slew him, received from the same body the highest token which it could give of its esteem.

In these closing years of the nineteenth century this great need of mutual knowledge and of common action felt by men of science of different lands is being manifested in a special way. Though nowadays what is done anywhere is soon known everywhere, the news of a discovery being often flashed over the globe by telegraph, there is an increasing activity in the direction of organization to promote international meetings and international cooperation. In almost every science inquirers from many lands now gather together at stated intervals in international congresses to discuss matters which they have in common at heart, and go away each one feeling strengthened by having met his brother. The desire that in the struggle to lay bare the secrets of nature the least waste of human energy should be incurred is leading more and more to the concerted action of nations combining to attack problems the solution of which is difficult and costly. The determination of standards of measurement, magnetic surveys, the solution of great geodetic problems, the mapping of the heavens and of the earth—all these are being carried on by international organizations.

In this and in other countries men's minds have this long while past been greatly moved by the desire to make fresh efforts to pierce the dark secrets of the forbidding Antarctic regions. Belgium has just made a brave single-handed attempt; a private enterprise sailing from these shores is struggling there now, lost for the present to our view; and this year we in England and our brethren in Germany are, thanks to the promised aid of the respective governments, and no less to private liberality, in which this association takes its share, able to begin the preparation of carefully organized expeditions. That international amity of which I am speaking is illustrated by the fact that in this country and in that there is not only a great desire but a firm purpose to secure the fullest cooperation between the expeditions which will leave the two shores. If in this momentous attempt any rivalry be shown between the two nations, it will be for each a rivalry, not in forestalling, but in assisting the other. May I add that if the story of the past may seem to give our nation some claim to the seas as more peculiarly our own, that claim bespeaks a duty likewise peculiarly our own, to leave no effort untried by which we may plumb the seas' yet unknown depths and trace their yet unknown shores? That claim, if it means anything, means that when nations are joining hands in the dangerous work of exploring the unknown South, the larger burden of the task should fall to Britain's share; it means that we in this country should see to it, and see to it at once, that the concerted Antarctic expedition which in some two years or so will leave the shores of Germany, of England, and perhaps of other lands, should, so far as

we are concerned, be so equipped and so sustained that the risk of failure and disaster may be made as small, and the hope of being able not merely to snatch a hurried glimpse of lands not yet seen, but to gather in with full hands a rich harvest of the facts which men not of one science only, but of many, long to know, as great as possible.

Another international scientific effort demands a word of notice. The need which every inquirer in science feels to know, and to know quickly, what his fellow-worker, wherever on the globe he may be carrying on his work or making known his results, has done or is doing, led some four years back to a proposal for carrying out by international cooperation a complete current index, issued promptly, of the scientific literature of the world. Though much labor in many lands has been spent upon the undertaking, the project is not yet an accomplished fact. Nor can this, perhaps, be wondered at, when the difficulties of the task are weighed. Difficulties of language, difficulties of driving in one team all the several sciences which, like young horses, wish each to have its head free with leave to go its own way, difficulties mechanical and financial, of press and post, difficulties raised by existing interests—these and yet other difficulties are obstacles not easy to be overcome. The most striking and the most encouraging features of the deliberations which have now been going on for three years have been the repeated expressions, coming not from this or that quarter only, but from almost all quarters, of an earnest desire that the effort should succeed, of a sincere belief in the good of international cooperation, and of a willingness to sink as far as possible individual interests for the sake of the common cause. In the face of such a spirit we may surely hope that the many difficulties will ultimately pass out of sight.

Perhaps, however, not the least notable fact of international cooperation in science is the proposal which has been made within the last two years that the leading academies of the world should, by representatives, meet at intervals to discuss questions in which the learned of all lands are interested. A month hence a preliminary meeting of this kind will be held at Wiesbaden; and it is at least probable that the closing year of that nineteenth century in which science has played so great a part may at Paris during the great World's Fair—which every friend, not of science only, but of humanity, trusts may not be put aside or even injured through any untoward event, and which promises to be an occasion not of pleasurable sight-seeing only, but also, by its many international congresses, of international communing in the search for truth—witness the first select Witenagemote of the science of the world.

I make no apology for having thus touched on international cooperation. I should have been wanting had I not done so on the memorable occasion of this meeting. A hundred years ago two great nations were grappling with each other in a fierce struggle which had

lasted, with pauses, for many years, and which was to last for many years to come; war was on every lip and in almost every heart. To-day this meeting has, by a common wish, been so arranged that those two nations should, in the persons of their men of science, draw as near together as they can, with nothing but the narrow streak of the channel between them, in order that they may take counsel together on matters in which they have one interest and a common hope. May we not look upon this brotherly meeting as one of many signs that science, though she works in a silent manner and in ways unseen by many, is steadily making for peace?

Looking back, then, in this last year of the eighteen hundreds, on the century which is drawing to a close, while we may see in the history of scientific inquiry much which, telling the man of science of his shortcomings and his weakness, bids him be humble, we also see much, perhaps more, which gives him hope. Hope is, indeed, one of the watchwords of science. In the latter-day writings of some who know not science much may be read which shows that the writer is losing or has lost hope in the future of mankind. There are not a few of these; their repeated utterances make a sign of the times. Seeing in matters lying outside science few marks of progress and many tokens of decline or decay, recognizing in science its material benefits only, such men have thoughts of despair when they look forward to the times to come. But if there be any truth in what I have attempted to urge to-night, if the intellectual, if the moral influences of science are no less marked than her material benefits, if, moreover, that which she has done is but the earnest of that which she shall do, such men may pluck up courage and gather strength by laying hold of her garment. We men of science at least need not share their views or their fears. Our feet are set, not on the shifting sands of the opinions and of the fancies of the day, but on a solid foundation of verified truth, which by the labors of each succeeding age is made broader and more firm. To us the past is a thing to look back upon, not with regret, not as something which has been lost never to be regained, but with content, as something whose influence is with us still, helping us on our further way. With us, indeed, the past points not to itself, but to the future; the golden age is in front of us, not behind us; that which we do know is a lamp whose brightest beams are shed into the unknown before us, showing us how much there is in front and lighting up the way to reach it. We are confident in the advance because, as each one of us feels that any step forward which he may make is not ordered by himself alone and is not the result of his own sole efforts in the present, but is, and that in large measure, the outcome of the labors of others in the past, so each one of us has the sure and certain hope that as the past has helped him, so his efforts, be they great or be they small, will be a help to those to come.

SIR WILLIAM CROOKES ON PSYCHICAL RESEARCH.

The articles in the General Appendix of the Smithsonian Report are intended as a rule to set forth accounts of known and admitted scientific facts and not of speculations.

The following two articles, forming portions of addresses to the British Association for the Advancement of Science and to the Society for Psychical Research, delivered in each case by their president, Prof. William Crookes, contain, however, speculations so weighty and ingeniously illustrated that an exception is here made in their favor, but it is to be repeated that they are not presented as demonstrated fact.

S. P. LANGLEY, *Secretary.*

I. EXTRACT FROM ADDRESS BEFORE THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, 1898.[1]

* * * No incident in my scientific career is more widely known than the part I took many years ago in certain psychic researches. Thirty years have passed since I published an account of experiments tending to show that outside our scientific knowledge there exists a Force exercised by intelligence differing from the ordinary intelligence common to mortals. This fact in my life is, of course, well understood by those who honored me with the invitation to become your president. Perhaps among my audience some may feel curious as to whether I shall speak out or be silent. I elect to speak, although briefly. To enter at length on a still debatable subject would be unduly to insist on a topic which—as Wallace, Lodge, and Barrett have already shown—though not unfitted for discussion at these meetings, does not yet enlist the interest of the majority of my scientific brethren. To ignore the subject would be an act of cowardice—an act of cowardice I feel no temptation to commit.

To stop short in any research that bids fair to widen the gates of knowledge, to recoil from fear of difficulty or adverse criticism, is to bring reproach on science. There is nothing for the investigator to do but to go straight on; "to explore up and down, inch by inch, with the taper his reason;" to follow the light wherever it may lead, even should it at times resemble a will-o'-the-wisp. I have nothing to

[1] From Report of the British Association for the Advancement of Science, 1898. Bristol meeting.

retract. I adhere to my already published statements. Indeed, I might add much thereto. I regret only a certain crudity in those early expositions which, no doubt justly, militated against their acceptance by the scientific world. My own knowledge at that time scarcely extended beyond the fact that certain phenomena new to science had assuredly occurred, and were attested by my own sober senses and, better still, by automatic record. I was like some two-dimensional being who might stand at the singular point of a Riemann's surface, and thus find himself in infinitesimal and inexplicable contact with a plane of existence not his own.

I think I see a little farther now. I have glimpses of something like coherence among the strange elusive phenomena; of something like continuity between those unexplained forces and laws already known. This advance is largely due to the labors of another association, of which I have also this year the honor to be president—the Society for Psychical Research. And were I now introducing for the first time these inquiries to the world of science I should choose a starting point different from that of old. It would be well to begin with *telepathy;* with the fundamental law, as I believe it to be, that thoughts and images may be transferred from one mind to another without the agency of the recognized organs of sense—that knowledge may enter the human mind without being communicated in any hitherto known or recognized ways.

Although the inquiry has elicited important facts with reference to the mind, it has not yet reached the scientific stage of certainty which would entitle it to be usefully brought before one of our sections. I will therefore confine myself to pointing out the direction in which scientific investigation can legitimately advance. If telepathy take place we have two physical facts—the physical change in the brain of A, the suggester, and the analogous physical change in the brain of B, the recipient of the suggestion. Between these two physical events there must exist a train of physical causes. Whenever the connecting sequence of intermediate causes begins to be revealed, the inquiry will then come within the range of one of the sections of the British association. Such a sequence can only occur through an intervening medium. All the phenomena of the universe are presumably in some way continuous, and it is unscientific to call in the aid of mysterious agencies when, with every fresh advance in knowledge, it is shown that ether vibrations have powers and attributes abundantly equal to any demand—even to the transmission of thought. It is supposed by some physiologists that the essential cells of nerves do not actually touch, but are separated by a narrow gap which widens in sleep, while it narrows almost to extinction during mental activity. This condition is so singularly like that of a Branly or Lodge coherer as to suggest a further analogy. The structure of brain and nerve being similar, it is conceivable there may be present masses of such nerve coherers in the

brain whose special function it may be to receive impulses brought from without through the connecting sequence of ether waves of appropriate order of magnitude. Röntgen has familiarized us with an order of vibrations of extreme minuteness compared with the smallest waves with which we have hitherto been acquainted, and of dimensions comparable with the distances between the centers of the atoms of which the material universe is built up; and there is no reason to suppose that we have here reached the limit of frequency. It is known that the action of thought is accompanied by certain molecular movements in the brain, and here we have physical vibrations capable, from their extreme minuteness, of acting direct on individual molecules, while their rapidity approaches that of the internal and external movements of the atoms themselves.

Confirmation of telepathic phenomena is afforded by many converging experiments and by many spontaneous occurrences only thus intelligible. The most varied proof, perhaps, is drawn from analysis of the subconscious workings of the mind, when these, whether by accident or design, are brought into conscious survey. Evidence of a region below the threshold of consciousness has been presented, since its first inception, in the Proceedings of the Society for Psychical Research, and its various aspects are being interpreted and welded into a comprehensive whole by the pertinacious genius of F. W. H. Myers. Concurrently, our knowledge of the facts in this obscure region has received valuable additions at the hands of laborers in other countries. To mention a few names out of many, the observations of Richet, Pierre Janet, and Binet (in France), of Breuer and Freud (in Austria), of William James (in America), have strikingly illustrated the extent to which patient experimentation can probe subliminal processes, and can thus learn the lessons of alternating personalities and abnormal states. While it is clear that our knowledge of subconscious mentation is still to be developed, we must beware of rashly assuming that all variations from the normal waking condition are necessarily morbid. The human race has reached no fixed or changeless ideal. In every direction there is evolution as well as disintegration. It would be hard to find instances of more rapid progress, moral and physical, than in certain important cases of cure by suggestion—again to cite a few names out of many—by Liébeault, Bernheim, the late Auguste Voisin, Bérillon (in France), Schrenck-Notzing (in Germany), Forel (in Switzerland), van Eeden (in Holland), Wetterstrand (in Sweden), Milne-Bramwell and Lloyd Tuckey (in England). This is not the place for details, but the vis medicatrix thus evoked, as it were, from the depths of the organism, is of good omen for the upward evolution of mankind.

A formidable range of phenomena must be scientifically sifted before we effectually grasp a faculty so strange, so bewildering, and for ages so inscrutable as the direct action of mind on mind. This delicate

task needs a rigorous employment of the method of exclusion—a constant setting aside of irrelevant phenomena that could be explained by known causes, including those far too familiar causes, conscious and unconscious fraud. The inquiry unites the difficulties inherent in all experimentation connected with mind, with tangled human temperaments, and with observations dependent less on automatic record than on personal testimony. But difficulties are things to be overcome even in the elusory branch of research known as experimental psychology. It has been characteristic of the leaders among the group of inquirers constituting the Society for Psychical Research to combine critical and negative work with work leading to positive discovery. To the penetration and scrupulous fair-mindedness of Prof. Henry Sidgwick and of the late Edmund Gurney is largely due the establishment of canons of evidence in psychical research, which strengthen while they narrow the path of subsequent explorers. To the detective genius of Dr. Richard Hodgson we owe a convincing demonstration of the narrow limits of human continuous observation.

It has been said that "Nothing worth the proving can be proved, nor yet disproved." True though this may have been in the past, it is true no longer. The science of our century has forged weapons of observation and analysis by which the veriest tyro may profit. Science has trained and fashioned the average mind into habits of exactitude and disciplined perception, and in so doing has fortified itself for tasks higher, wider, and incomparably more wonderful than even the wisest among our ancestors imagined. Like the souls in Plato's myth that follow the chariot of Zeus, it has ascended to a point of vision far above the earth. It is henceforth open to science to transcend all we now think we know of matter and to gain new glimpses of a profounder scheme of Cosmic law.

An eminent predecessor in this chair declared that "by an intellectual necessity he crossed the boundary of experimental evidence, and discerned in that matter, which we in our ignorance of its latent powers, and notwithstanding our professed reverence for its Creator, have hitherto covered with opprobrium, the potency and promise of all terrestrial life." I should prefer to reverse the apothegm, and to say that in life I see the promise and potency of all forms of matter.

In old Egyptian days a well-known inscription was carved over the portal of the temple of Isis: "I am whatever hath been, is, or ever will be; and my veil no man hath yet lifted." Not thus do modern seekers after truth confront nature—the word that stands for the baffling mysteries of the universe. Steadily, unflinchingly, we strive to pierce the inmost heart of Nature, from what she is to reconstruct what she has been, and to prophesy what she yet shall be. Veil after veil we have lifted, and her face grows more beautiful, august, and wonderful with every barrier that is withdrawn.

II.—ADDRESS BEFORE THE SOCIETY FOR PSYCHICAL RESEARCH.[1]

The task I am called upon to perform to-day is to my thinking by no means a merely formal or easy matter. It fills me with deep concern to give an address, with such authority as a president's chair confers, upon a science which, though still in a purely nascent stage, seems to me at least as important as any other science whatever. Psychical science, as we here try to pursue it, is the embryo of something which in time may dominate the whole world of thought. This possibility—nay, probability—does not make it the easier to me now. Embryonic development is apt to be both rapid and interesting; yet the prudent man shrinks from dogmatising on the egg until he has seen the chicken.

Nevertheless, I desire, if I can, to say a helpful word. And I ask myself what kind of helpful word. Is there any connection between my old-standing interest in psychical problems and such original work as I may have been able to do in other branches of science?

I think there is such a connection—that the most helpful quality which has aided me in psychical problems and has made me lucky in physical discoveries (sometimes of rather unexpected kinds) has simply been my knowledge—my vital knowledge, if I may so term it—of my own ignorance.

Most students of nature sooner or later pass through a process of writing off a large percentage of their supposed capital of knowledge as a merely illusory asset. As we trace more accurately certain familiar sequences of phenomena we begin to realize how closely these sequences, or laws, as we call them, are hemmed round by still other laws of which we can form no notion. With myself this writing off of illusory assets has gone rather far and the cobweb of supposed knowledge has been pinched (as some one has phrased) into a particularly small pill.

I am not disposed to bewail the limitations imposed by human ignorance. On the contrary, I feel ignorance is a healthful stimulant; and my enforced conviction that neither I nor anyone can possibly lay down beforehand what does not exist in the universe, or even what is not going on all round us every day of our lives, leaves me with a cheerful hope that something very new and very arresting may turn up anywhere at any minute.

Well, it was this attitude of a mind "to let" which first brought me across Mr. D. D. Home, and which led to my getting a glimpse of

[1] Address by the president, William Crookes, to the Society for Psychical Research, January 29, 1897. Reprinted from Proceedings of the Society for Psychical Research, London, Vol. XII, March, 1897, pp. 338-355.

some important laws of matter and energy of which I fear many of my fellow physicists still prefer to be uncognizant. It is this same accessible temper of mind which leads me to follow the problems of the Society for Psychical Research with an interest which, if somewhat calmed by advancing years, and by a perception of the inevitable slowness of discovery, is still as deep a feeling as any which life has left me. And I shall try to utilize this temper of mind to-day by clearing away, so far as I can, certain presuppositions, on one side or on the other, which seem to me to depend upon a too hasty assumption that we know more about the universe than as yet we really can know.

I will take the most essential part first, and address myself to those who believe with me in the survival of man's individuality after death. I will point out a curious, inveterate, and widespread illusion—the illusion that our earthly bodies are a kind of norm of humanity, so that ethereal bodies, if such there be, must correspond to them in shape and size.

When we take a physical view of a human being in his highest form of development, he is seen to consist essentially of a thinking brain, the brain itself, among its manifold functions, being a transformer whereby intelligent will power is enabled to react on matter. To communicate with the external world, the brain requires organs by which it can be transported from place to place, and other organs by means of which energy is supplied to replace that expended in the exercise of its own special functions. Again, waste of tissue and reparation have to be provided for; hence the necessity for organs of digestion, assimilation, circulation, respiration, etc., to carry on these processes effectually; and when we consider that this highly complex organ is fitted to undergo active work for the best part of a century, we can not but marvel that it can keep in tune so long. The human creature represents the most perfect thinking and acting machine yet evolved on this earth, developing through countless ages in strict harmony with the surrounding conditions of temperature, atmosphere, light, and gravitation. The profound modifications in the human frame, which any important alteration in either of these factors would occasion, are strangely unconsidered. It is true there have been questionings as to the effects that might be occasioned by changes in temperature and atmospheric composition, but possible variations in gravitation seem almost to have escaped notice. The human body, which long experience and habit have taught us to consider in its highest development as the perfection of beauty and grace—"formed in the image of God"—is entirely conditioned by the strength of gravitation on this globe. So far as has been possible to ascertain, the intensity of gravity has not varied appreciably within those geologic ages covering the existence of animated thinking beings. The human

race, therefore, has passed through all its periods of evolution and development in strict conformity with and submission to this dominant power until it is difficult to conceive any great departure from the narrow limits imposed on the proportions of the human frame.

In the first place, I wish to consider what transformation in our appearance would be produced by a change in the force of gravitation. Let us take extreme cases. Say that the power of gravitation were to be doubled. In that case we should have to exert a vastly increased strength to support ourselves in any other than the prone or dorsal position, it would be hard to rise from the ground, to run, leap, climb, to drag or carry any object. Our muscles would necessarily be more powerful, and the skeleton to which they are attached would need corresponding modification. To work such limbs a more rapid transformation of matter would be required; hence the supply of nutriment must be greater, involving enlarged digestive organs, and a larger respiratory apparatus to allow of the perfect aeration of the increased mass of the blood. To keep up the circulation with the necessary force, either the heart would have to be more powerful or the distance through which the blood would require to be impelled must be reduced. The increased amount of nourishment demanded would involve a corresponding increase in the difficulty of its collection, and the struggle for existence would be intensified. More food being required day by day, the jaws would have to be enlarged and the muscles strengthened. The teeth also must be adapted for extra tearing and grinding.

These considerations involve marked changes in the structure of human beings. To accord with thickened bones, bulging muscles, and larger respiratory and digestive apparatus, the body would be heavier and more massive. The necessity for such alterations in structure would be increased by the liability to fall. The necessity of keeping the center of gravity low, and the great demands made on the system in other respects, must conspire to reduce the size of head and brain. With increase of gravitation the bipedal form would be beset by drawbacks. Assuming that the human race, under the altered circumstances, remained bipedal, it is highly probable that a large increase in the quadruped, hexapod, or octopod structure would prevail in the animal kingdom. The majority of animals would be of the saurian class, with very short legs allowing the trunk to rest easily on the ground, and the serpent type would probably be in the ascendant. Winged creatures would suffer severely, and small birds and insects would be dragged to earth by a force hard to resist; although this might be more or less compensated by the increased density of the air. Humming birds, dragon flies, butterflies, and bees, all of which spend a large portion of their time in the air, would, in the struggle for existence, be rare visitants. Hence the fertilization of flowers by the inter-

vention of insects must be thwarted, and this would lead to the extinction, or at all events to a scarcity, of entomophilous plants, i. e., all those with the showiest blossoms—a gloomy result to follow from a mere increase of the earth's attraction.

But having known no other type of human form, it is allowable to think that, under these different conditions, man would still consider woman—though stunted, thick limbed, flat-footed, with enormous jaws underlying a diminutive skull—as the highest type of beauty!

Decreased attraction of the earth might be attended with another set of changes scarcely less remarkable. With the same expenditure of vital energy as at present, and with the same quantity of transformation of matter, we should be able to lift heavier weights, to take longer bounds, to move with greater swiftness, and to undergo prolonged muscular exertion with less fatigue—possibly to fly. Hence the transformation of matter required to keep up animal heat and to restore the waste of energy and tissue would be smaller for the same amount of duty done. A less volume of blood, reduced lungs and digestive organs would be required. Thus we might expect a set of structural changes of an inverse nature to those resulting from intensified gravitation. All parts of the body might safely be constructed upon a less massive plan—a slighter skeleton, smaller muscles, and slenderer trunk. These modifications, in a less degree than we are contemplating, tend in the present to beauty of form, and it is easy to imagine our æsthetic feelings would naturally keep pace with further developments in the direction of grace, slenderness, symmetry, and tall figures.

It is curious that the popular conceptions of evil and malignant beings are of the type that would be produced by increased gravitation—toads, reptiles, and noisome creeping things—while the arch fiend himself is represented as perhaps the ultimate form which could be assumed by a thinking brain and its necessary machinery were the power of gravitation to be increased to the highest point compatible with existence—a serpent crawling along the ground. On the other hand, our highest types of beauty are those which would be common under decreased gravitation.

The "daughter of the gods, divinely tall," and the leaping athlete, please us by the slight triumph over the earthward pull which their stature or spring implies. It is true we do not correspondingly admire the flea, whose triumph over gravitation, unaided by wings, is so striking. Marvellous as is the flea, its body, like ours, is strictly conditioned by gravitation.

But popular imagination presupposes spiritual beings to be utterly independent of gravitation, while retaining shapes and proportions which gravitation originally determined, and only gravitation seems likely to maintain.

When and if spiritual beings make themselves visible either to our bodily eyes or to our inward vision, their object would be thwarted were they not to appear in a recognizable form; so that their appearance would take the shape of the body and clothing to which we have been accustomed. Materiality, form, and space, I am constrained to believe, are temporary conditions of our present existence. It is difficult to conceive the idea of a spiritual being having a body like ours, conditioned by the exact gravitating force exerted by the earth, and with organs which presuppose the need for food and necessity for the removal of waste products. It is equally difficult, hemmed in and bound round as we are by materialistic ideas, to think of intelligence, thought, and will existing without form or matter and untrammeled by gravitation or space.

Men of science before now have had to face a similar problem. In some speculations on the nature of matter, Faraday [1] expressed himself in language which, mutatis mutandis, applies to my present surmises. This earnest philosopher was speculating on the ultimate nature of

[1] "If we must assume at all, as indeed in a branch of knowledge like the present we can hardly help it, then the safest course appears to be to assume as little as possible, and in that respect the atoms of Boscovich appear to me to have a great advantage over the more usual notion. His atoms are mere centers of forces or powers, not particles of matter in which the powers themselves reside.

"If in the ordinary view of atoms we call the particle of matter away from the powers *a*, and the system of powers or forces in and around it *m*, then in Boscovich's theory *a* disappears, or is a mere mathematical point, while in the usual notion it is a little unchangeable, impenetrable piece of matter, and *m* is an atmosphere of force grouped around it.

"To my mind, therefore, the *a* or nucleus vanishes, and the substance consists of the powers, or *m*; and indeed, what notion can we form of the nucleus independent of its powers? All our perception and knowledge of the atom, and even our fancy, is limited to ideas of its powers. What thought remains on which to hang the imagination of an *a* independent of the acknowledged forces?

"A mind just entering on the subject may consider it difficult to think of the powers of matter independent of a separate something to be called 'the matter;' but it is certainly far more difficult, and indeed impossible, to think of or imagine that matter independent of the powers. Now, the powers we know and recognize in every phenomenon of the creation, the abstract matter in none; why, then, assume the existence of that of which we are ignorant, which we can not conceive, and for which there is no philosophical necessity?

"If an atom be conceived to be a center of power, that which is ordinarily referred to under the term 'shape' would be now referred to the disposition and relative intensity of the forces. * * * Nothing can be supposed of the disposition of forces in and about a solid nucleus of matter which can not be equally conceived with respect to a center.

"The view now stated of the constitution of matter would seem to involve necessarily the conclusion that matter fills all space. * * * In that view matter is not merely mutually penetrable, but each atom extends, so to say, throughout the whole of the solar system, yet always retaining its own center of force." (Faraday, "On the nature of matter," *Phil. Mag.*, 1844, Vol. XXIV, p. 136.)

matter; and, thinking of the little, hard, impenetrable atom of Lucretius, and the forces or forms of energy appertaining to it, he felt himself impelled to reject the idea of the existence of the nucleus altogether, and to think only of the forces and forms of energy usually associated therewith. He was led to the conclusion that this view necessarily involved the surmise that the atoms are not merely mutually penetrable, but that each atom, so to say, extends throughout all space, yet always retaining its own center of force.[1]

A view of the constitution of matter which recommended itself to Faraday as preferable to the one ordinarily held appears to me to be exactly the view I endeavor to picture as the constitution of spiritual beings. Centers of intellect, will, energy, and power, each mutually penetrable, while at the same time permeating what we call space, but each center retaining its own individuality, persistence of self, and memory. Whether these intelligent centers of the various spiritual forces which in their aggregate go to make up man's character or karma are also associated in any way with the forms of energy which, centered, form the material atom—whether these spiritual entities are material, not in the crude, gross sense of Lucretius, but material as sublimated through the piercing intellect of Faraday—is one of those mysteries which to us mortals will perhaps ever remain an unsolved problem.

My next speculation is more difficult, and is addressed to those who not only take too terrestrial a view, but who deny the plausibility—nay, the possibility—of the existence of an unseen world at all. I reply we are demonstrably standing on the brink, at any rate, of one unseen world. I do not here speak of a spiritual or immaterial world. I speak of the world of the infinitely little, which must be still called a material world, although matter as therein existing or perceptible is something which our limited faculties do not enable us to conceive. It is the world—I do not say of molecular forces as opposed to molar, but of forces whose action lies mainly outside the limit of human perception, as opposed to forces evident to the gross perception of human organisms. I hardly know how to make clear to myself or to you the difference in the apparent laws of the universe which would follow upon a mere difference of bulk in the observer. Such an observer I must needs imagine as best I can. I shall not attempt to rival the vividness of the great satirist who, from a postulated difference of size far less considerable, deduced in Gulliver's Travels the absurdity, and the mere relativity, of so much in human morals, politics, society. But I shall take courage from the example of my predecessor in this chair, Prof. William James, of Harvard, from whom later I shall cite a most striking parable of precisely the type I seek.

You must permit me, then, an homunculus on whom to hang my

[1] I may say, in passing, that the modern vortex atom also fulfills these conditions.

speculation.[1] I can not place him actually amid the interplay of molecules, for lack of power to imagine his environment; but I shall make him of such microscopic size that molecular forces which in common life we hardly notice—such as surface tension, capillarity, the Brownian movements—become for him so conspicuous and dominant that he can hardly believe, let us say, in the universality of gravitation, which we may suppose to have been revealed to him by ourselves, his creators.

Let us place him on a cabbage leaf and let him start for himself.

The area of the cabbage leaf appears to him as a boundless plain many square miles in extent. To this minimized creature the leaf is studded with huge glittering transparent globes, resting motionless on the surface of the leaf, each globe vastly exceeding in height the towering pyramids. Each of these spheres appears to emit from one of its sides a dazzling light. Urged by curiosity he approaches and touches one of the orbs. It resists pressure like an india-rubber ball, until accidentally he fractures the surface, when suddenly he feels himself seized and whirled and brought somewhere to an equilibrium, where he remains suspended in the surface of the sphere utterly unable to extricate himself. In the course of an hour or two he finds the globe diminishing, and ultimately it disappears, leaving him at liberty to pursue his travels. Quitting the cabbage leaf, he strays over the surface of the soil, finding it exceeding rocky and mountainous, until he sees before him a broad surface akin to the kind of matter which formed the globes on the cabbage leaf. Instead, however, of rising upward from its support, it now slopes downward in a vast curve from the brink, and ultimately becomes apparently level, though, as this is at a considerable distance from the shore, he can not be absolutely certain. Let us now suppose that he holds in his hand a vessel bearing the same proportion to his minimized frame that a pint measure does to that of a man as he is, and that by adroit manipulation he contrives to fill it with water. If he inverts the vessel he finds that the liquid will not flow and can only be dislodged by violent shocks. Wearied by his exertions to empty the vessel of water, he sits on the shore and idly amuses himself by throwing stones and other objects into the water. As a rule the stones and other wet bodies sink, although when dry they obstinately refuse to go to the bottom, but float on the surface. He tries other substances. A rod of polished steel, a silver pencil case, some platinum wire, and a steel pen, objects two or three times the density of the stones, refuse to sink at all, and float on the surface like so many bits of cork. Nay, if he and his friends manage to throw into the water one of those enormous steel bars which we call needles,

[1] I need hardly say that in this fanciful sketch, composed only for an illustrative purpose, all kinds of problems (as of the homunculus's own structure and powers) are left untouched, and various points which would really need to be mathematically worked out are left intentionally vague.

this also makes a sort of concave trough for itself on the surface and floats tranquilly. After these and a few more observations he theorizes on the properties of water and of liquids in general. Will he come to the conclusion that liquids seek their own level, that their surfaces when at rest are horizontal, and that solids when placed in a liquid sink or float according to their higher or lower specific gravity? No; he will feel justified in inferring that liquids at rest assume spherical, or at least curvilinear forms, whether convex or concave, depending upon circumstances not easily ascertained; that they can not be poured from one vessel to another and resist the force of gravitation, which is consequently not universal, and that such bodies as he can manipulate generally refuse to sink in liquids, whether their specific gravity be high or low. From the behavior of a body placed in contact with a dewdrop he will even derive plausible reasons for doubting the inertia of matter.

Already he has been somewhat puzzled by the constant and capricious bombardment of cumbrous objects like portmanteaus flying in the air; for the gay motes that people the sunbeams will dance somewhat unpleasantly for a microscopic homunculus who can never tell where they are coming. Nay, what he has understood to be the difficulty experienced by living creatures in rising from the earth, except with wings, will soon seem absurdly exaggerated; for he will discern a terrific creature, a behemoth "in plated mail," leaping through the skies in frenzied search for prey, and for the first time due homage will be rendered to the majesty of the common flea.

Perturbed by doubts, he will gaze at night into some absolutely tranquil pool. There, with no wind to ruffle, nor access of heat to cause currents or change surface tension, he perceives small inanimate objects immersed and still. But are they still? No. One of them moves; another is moving. Gradually it is borne in upon him that whenever any object is small enough it is always in motion. Perhaps our homunculus might be better able than we are to explain these so-called Brownian movements; or the guess might be forced upon him that he who sees this sight is getting dim glimpses of the ultimate structure of matter, and that these movements are residual, the result of the inward molecular turmoil which has not canceled itself out into nullity, as it must needs do in aggregations of matter of more than the smallest microscopic dimensions.

Things still more tormentingly perplexing our homunculus would doubtless encounter. And these changes in his interpretation of phenomena would arise not from his becoming aware of any forces hitherto overlooked, still less from the disappearance of laws now recognized, but simply from the fact that his supposed decrease in bodily size brings capillarity, surface tension, etc., into a relative prominence they do not now possess. To full-grown rational beings the effects of these

forces rank among residual phenomena, which attract attention only when science has made a certain progress. To homunculi such as we have imagined the same effects would be of capital importance, and would be rightly interpreted not as something supplementary to those of general gravitation, but as due to an independent and possibly antagonistic force.

The physics of these homunculi would differ most remarkably from our own. In the study of heat they would encounter difficulties probably insuperable. In this branch of physical investigation little can be done unless we have the power at pleasure of raising and lowering the temperature of bodies. This requires the command of fire. Actual man, in a rudimentary state of civilization, can heat and ignite certain kinds of matter by friction, percussion, concentrating the sun's rays, etc.; but before these operations produce actual fire they must be performed upon a considerable mass of matter, otherwise the heat is conducted or radiated away as rapidly as produced and the point of ignition seldom reached.

Nor could it be otherwise with the chemistry of the little people, if, indeed, such a science be conceived as at all possible for them.

It can scarcely be denied that the fundamental phenomena which first led mankind into chemical inquiries are those of combustion. But, as we have just seen, minimized beings would be unable to produce fire at will, except by certain chemical reactions, and would have little opportunity of examining its nature. They might occasionally witness forest fires, volcanic eruptions, etc.; but such grand and catastrophic phenomena, though serving to reveal to our supposed Lilliputians the existence of combustion, would be ill suited for quiet investigation into its conditions and products. Moreover, considering the impossibility they would experience of pouring water from one test tube to another, the ordinary operations of analytical chemistry and of all manipulations depending on the use of the pneumatic trough would remain forever a sealed book.

Let us for a moment go to the opposite extreme and consider how Nature would present itself to human beings of enormous magnitude. Their difficulties and misconstructions would be of an opposite nature to those experienced by pigmies. Capillary attraction and the cohesion of liquids, surface tension, and the curvature of liquid surfaces near their boundary, the dewdrop and the behavior of minute bodies on a globule of water, the flotation of metals on the surface of water, and many other familiar phenomena, would be either ignored or unknown. The homunculus able to communicate but a small momentum would find all objects much harder than they appear to us, while to a race of colossals granite rocks would be but a feeble impediment.

There would be another most remarkable difference between such enormous beings and ourselves. If we stoop and take up a pinch of

earth between fingers and thumb, moving those members, say, through the space of a few inches in a second of time, we experience nothing remarkable. The earth offers a little resistance, more or less, according to its greater or less tenacity, but no other perceptible reaction follows.

Let us suppose the same action performed by a gigantic being, able to move finger and thumb in a second's space through some miles of soil in the same lapse of time, and he would experience a very decided reaction. The mass of sand, earth, stones, and the like, hurled together in such quantities and at such speed, would become intensely hot. Just as the homunculus would fail to bring about ignition when he desired, so the colossus could scarcely move without causing the liberation of a highly inconvenient degree of heat, literally making everything too hot to hold. He would naturally ascribe to granite rocks and the other constituents of the earth's surface such properties as we attribute to phosphorus—of combustion on being a little roughly handled.

Need I do more than point the obvious lesson? If a possible—nay, reasonable—variation in only one of the forces conditioning the human race, that of gravitation, could so modify our outward form, appearance, and proportions as to make us to all intents and purposes a different race of beings; if mere differences of size can cause some of the most simple facts in chemistry and physics to take so widely different a guise; if beings microscopically small and prodigiously large would simply as such be subject to the hallucinations I have pointed out, and to others I might enlarge upon, is it not possible that we, in turn, though occupying, as it seems to us, the golden mean, may also by the mere virtue of our size and weight fall into misinterpretations of phenomena from which we should escape were we or the globe we inhabit either larger or smaller, heavier or lighter? May not our boasted knowledge be simply conditioned by accidental environments, and thus be liable to a large element of subjectivity hitherto unsuspected and scarcely possible to eliminate?

Here I will introduce Professor James's speculation, to which I have already alluded. It deals with a possible alteration of the time scale due to a difference in rapidity of sensation on the part of a being presumably on a larger scale than ourselves:

"We have every reason to think that creatures may possibly differ enormously in the amounts of duration which they intuitively feel, and in the fineness of the events that may fill it. Von Baer has indulged in some interesting computations of the effect of such differences in changing the aspect of nature. Suppose we were able, within the length of a second, to note distinctly 10,000 events, instead of barely 10, as now; if our life were then destined to hold the same number of impressions, it might be 1,000 times as short. We should live less than a month, and personally know nothing of the change of

seasons. If born in winter, we should believe in summer as we now believe in the heats of the Carboniferous era. The motions of organic beings would be so slow to our senses as to be inferred, not seen. The sun would stand still in the sky, the moon be almost free from change, and so on. But now reverse the hypothesis, and suppose a being to get only one one-thousandth part of the sensations that we get in a given time, and consequently to live 1,000 times as long. Winters and summers will be to him like quarters of an hour. Mushrooms and the swifter-growing plants will shoot into being so rapidly as to appear instantaneous creations; annual shrubs will rise and fall from the earth like restlessly boiling water springs; the motions of animals will be as invisible as are to us the movements of bullets and cannon balls; the sun will scour through the sky like a meteor, leaving a fiery trail behind him, etc. That such imaginary cases (barring the superhuman longevity) may be realized somewhere in the animal kingdom it would be rash to deny." (James's Principles of Psychology, Vol. I, p. 639.)

And now let me specially apply this general conception of the impossibility of predicting what secrets the universe may still hold, what agencies undivined may habitually be at work around us.

Telepathy, the transmission of thought and images directly from one mind to another without the agency of the recognized organs of sense, is a conception new and strange to science. To judge from the comparative slowness with which the accumulated evidence of our society penetrates the scientific world, it is, I think, a conception even scientifically repulsive to many minds. We have supplied striking experimental evidence; but few have been found to repeat our experiments. We have offered good evidence in the observation of spontaneous cases, as apparitions at the moment of death and the like, but this evidence has failed to impress the scientific world in the same way as evidence less careful and less coherent has often done before. Our evidence is not confronted and refuted; it is shirked and evaded as though there were some great a priori improbability which absolved the world of science from considering it. I at least see no a priori improbability whatever. Our alleged facts might be true in all kinds of ways without contradicting any truth already known. I will dwell now on only one possible line of explanation, not that I see any way of elucidating all the new phenomena I regard as genuine, but because it seems probable I may shed a light on some of those phenomena.

All the phenomena of the universe are presumably in some way continuous; and certain facts, plucked as it were from the very heart of nature, are likely to be of use in our gradual discovery of facts which lie deeper still.

Let us, then, consider the vibrations we trace, not only in solid bodies, but in the air, and in a still more remarkable manner in the ether.

These vibrations differ in their velocity and in their frequency. That they exist, extending from one vibration to two thousand millions

of millions vibrations per second, we have good evidence. That they subserve the purpose of conveying impressions from outside sources of whatever kind to living organisms may be fully recognized.

As a starting point I will take a pendulum beating seconds in air. If I keep on doubling I will get a series of steps as follows:

Starting point.	The seconds pendulum.
Step 1....	2 vibrations per second.
2....	4
3....	8
4....	16
5....	32
6....	64
7....	128
8....	256
9....	512
10....	1024
15....	32768
20....	1,048576
25....	33,554432
30....	1073,741825
35....	34359,738368
40....	1,099511,627776
45....	35,184372,088832
50....	1125,899906,842624
55....	36028,707018,963968
56....	72057,594037,927936
57....	144115,188075,855872
58....	288220,376151,711744
59....	576440,752303,423488
60....	1,152881,504606,846976
61....	2,305763,009213,693952
62....	4,611526,018427,387904
63....	9,223052,036854,775808

At the fifth step from unity, at 32 vibrations per second, we reach the region where atmospheric vibration reveals itself to us as sound. Here we have the lowest musical note. In the next ten steps the vibrations per second rise from 32 to 32,768, and here, to the average human ear, the region of sound ends. But certain more highly endowed animals probably hear sounds too acute for our organs; that is, sounds which vibrate at a higher rate.

We next enter a region in which the vibrations rise rapidly, and the vibrating medium is no longer the gross atmosphere, but a highly attenuated medium, "a diviner air," called the ether. From the sixteenth to the thirty-fifth step the vibrations rise from 32,768 to 34359,738368 a second, such vibrations appearing to our means of observation as electrical rays.

We next reach a region extending from the thirty-fifth to the forty-fifth step, including from 34359,738368 to 35,184372,088832 vibrations per second. This region may be considered as unknown, because we

are as yet ignorant what are the functions of vibrations of the rates just mentioned. But that they have some function it is fair to suppose.

Now we approach the region of light, the steps extending from the forty-fifth to between the fiftieth and the fifty-first, and the vibrations extending from 35,184372,088832 per second (heat rays) to 1875,000000,000000 per second, the highest recorded rays of the spectrum. The actual sensation of light, and therefore the vibrations which transmit visible signs, being comprised between the narrow limits of about 450,000000,000000 (red light) and 750,000000,000000 (violet light)—less than one step.

Leaving the region of visible light we arrive at what is, for our existing senses and our means of research, another unknown region, the functions of which we are beginning to suspect. It is not unlikely that the X-rays of Professor Röntgen will be found to lie between the fifty-eighth and the sixty-first step, having vibrations extending from 288220,576151,711744 to 2,305763,009213,693952 per second, or even higher.

In this series it will be seen there are two great gaps, or unknown regions, concerning which we must own our entire ignorance as to the part they play in the economy of creation. Further, whether any vibrations exist having a greater number per second than those classes mentioned we do not presume to decide.

But is it premature to ask in what way are vibrations connected with thought or its transmission? We might speculate that the increasing rapidity or frequency of the vibrations would accompany a rise in the importance of the functions of such vibrations. That high frequency deprives the rays of many attributes that might seem incompatible with "brain waves" is undoubted. Thus, rays about the sixty-second step are so minute as to cease to be refracted, reflected, or polarized; they pass through many so-called opaque bodies, and research begins to show that the most rapid are just those which pass most easily through dense substances. It does not require much stretch of the scientific imagination to conceive that at the sixty-second or sixty-third step the trammels from which rays at the sixty-first step were struggling to free themselves have ceased to influence rays having so enormous a rate of vibration as 9,223052,036854,775808 per second, and that these rays pierce the densest medium with scarcely any diminution of intensity, and pass almost unrefracted and unreflected along their path with the velocity of light.

Ordinarily we communicate intelligence to each other by speech. I first call up in my own brain a picture of a scene I wish to describe, and then, by means of an orderly transmission of wave vibrations set in motion by my vocal chords through the material atmosphere, a corresponding picture is implanted in the brain of anyone whose ear is

capable of receiving such vibrations. If the scene I wish to impress on the brain of the recipient is of a complicated character, or if the picture of it in my own brain is not definite, the transmission will be more or less imperfect; but if I wish to get my audience to picture to themselves some very simple object, such as a triangle or a circle, the transmission of ideas will be well-nigh perfect, and equally clear to the brains of both transmitter and recipient. Here we use the vibrations of the material molecules of the atmosphere to transmit intelligence from one brain to another.

In the newly discovered Röntgen rays we are introduced to an order of vibrations of extremest minuteness as compared with the most minute waves with which we have hitherto been acquainted, and of dimensions comparable with the distances between the centers of the atoms of which the material universe is built up; and there is no reason to suppose that we have here reached the limit of frequency. Waves of this character cease to have many of the properties associated with light waves. They are produced in the same ethereal medium, and are probably propagated with the same velocity as light, but here the similarity ends. They can not be regularly reflected from polished surfaces; they have not been polarized; they are not refracted on passing from one medium to another of different density, and they penetrate considerable thicknesses of substances opaque to light with the same ease with which light passes through glass. It is also demonstrated that these rays, as generated in the vacuum tube, are not homogeneous, but consist of bundles of different wave-lengths, analogous to what would be differences of color could we see them as light. Some pass easily through flesh, but are partially arrested by bone, while others pass with almost equal facility through bone and flesh.

It seems to me that in these rays we may have a possible mode of transmitting intelligence which, with a few reasonable postulates, may supply a key to much that is obscure in psychical research. Let it be assumed that these rays, or rays even of higher frequency, can pass into the brain and act on some nervous center there. Let it be conceived that the brain contains a center which uses these rays as the vocal chords use sound vibrations (both being under the command of intelligence), and sends them out, with the velocity of light, to impinge on the receiving ganglion of another brain. In this way some, at least, of the phenomena of telepathy, and the transmission of intelligence from one sensitive to another through long distances, seem to come into the domain of law and can be grasped. A sensitive may be one who possesses the telepathic transmitting or receiving ganglion in an advanced state of development, or who, by constant practice, is rendered more sensitive to these high-frequency waves. Experience seems to show that the receiving and the transmitting ganglions are not equally developed; one may be active, while the other, like the

pineal eye in man, may be only vestigial. By such an hypothesis no physical laws are violated; neither is it necessary to invoke what is commonly called the supernatural.

To this hypothesis it may be objected that brain waves, like any other waves, must obey physical laws. Therefore, transmission of thought must be easier or more certain the nearer the agent and recipient are to each other, and should die out altogether before great distances are reached. Also it can be urged that if brain waves diffuse in all directions they should affect all sensitives within their radius of action, instead of impressing only one brain. The electric telegraph is not a parallel case, for there a material wire intervenes to conduct and guide the energy to its destination.

These are weighty objections, but not, I think, insurmountable. Far be it from me to say anything disrespectful of the law of inverse squares, but I have already endeavored to show we are dealing with conditions removed from our material and limited conceptions of space, matter, form. Is it inconceivable that intense thought concentrated toward a sensitive with whom the thinker is in close sympathy may induce a telepathic chain of brain waves, along which the message of thought can go straight to its goal without loss of energy due to distance? And is it also inconceivable that our mundane ideas of space and distance may be superseded in these subtile regions of unsubstantial thought, where "near" and "far" may lose their usual meaning?

I repeat that this speculation is strictly provisional. I dare to suggest it. The time may come when it will be possible to submit it to experimental tests.

I am impelled to one further reflection, dealing with the conservation of energy. We say, with truth, that energy is transformed but not destroyed, and that whenever we can trace the transformation we find it quantitatively exact. So far as our very rough exactness goes, this is true for inorganic matter and for mechanical forces. But it is only inferentially true for organized matter and for vital forces. We can not express life in terms of heat or of motion. And thus it happens that just when the exact transformation of energy will be most interesting to watch, we can not really tell whether any fresh energy has been introduced into the system or not. Let us consider this a little more closely.

It has, of course, always been realized by physicists, and has been especially pointed out by Dr. Croll, that there is a wide difference between the production of motion and the direction of it into a particular channel. The production of motion, molar or molecular, is governed by physical laws, which it is the business of the philosopher to find out and correlate. The law of the conservation of energy overrides all laws, and it is a preeminent canon of scientific belief that for every act done a corresponding expenditure of energy must be trans-

formed. No work can be effected without using up a corresponding value in energy of another kind. But to us the other side of the problem is even of more importance. Granted the existence of a certain kind of molecular motion, what is it that determines its direction along one path rather than another? A weight falls to the earth through a distance of 3 feet. I lift it, and let it fall once more. In these movements of the weight a certain amount of energy is expended in its rise and the same amount is liberated in its fall. But instead of letting the weight fall free, suppose I harness it to a complicated system of wheels, and, instead of letting the weight fall in the fraction of a second, I distribute its fall over twenty-four hours. No more energy is expended in raising the weight, and in its slow fall no more or less energy is developed than when it fell free; but I have made it do work of another kind. It now drives a clock, a telescope, or a philosophic instrument, and does what we call useful work. The clock runs down. I lift the weight by exerting the proper amount of energy, and in this action the law of conservation of energy is strictly obeyed. But now I have the choice of either letting the weight fall free in a fraction of a second, or, constrained by the wheelwork, in twenty-four hours. I can do which I like, and whichever way I decide, no more energy is developed in the fall of the weight. I strike a match; I can use it to light a cigarette or to set fire to a house. I write a telegram; it may be simply to say I shall be late for dinner, or it may produce fluctuations on the stock exchange that will ruin thousands. In these cases the actual force required in striking the match or in writing the telegram is governed by the law or conservation of energy; but the vastly more momentous part, which determines the words I use or the material I ignite, is beyond such a law. It is probable that no expenditure of energy need be used in the determination of direction one way more than another. Intelligence and free will here come into play, and these mystic forces are outside the law of conservation of energy as understood by physicists.

The whole universe, as we see it, is the result of molecular movement. Molecular movements strictly obey the law of conservation of energy, but what we call "law" is simply an expression of the direction along which a form of energy acts, not the form of energy itself. We may explain molecular and molar motions, and discover all the physical laws of motion, but we shall be far as ever from a solution of the vastly more important question as to what form of will and intellect is behind the motions of molecules, guiding and constraining them in definite directions along predetermined paths. What is the determining cause in the background? What combination of will and intellect outside our physical laws guides the fortuitous concourse of atoms along ordered paths culminating in the material world in which we live?

In these last sentences I have intentionally used words of wide sig-

nification—have spoken of guidance along ordered paths. It is wisdom to be vague here, for we absolutely can not say whether or when any diversion may be introduced into the existing system of earthly forces by an external power. We can no more be certain that this is not so than I can be certain, in an express train, that no signalman has pressed a handle to direct the train on to this or that line of rails. I may compute exactly how much coal is used per mile, so as to be able to say at any minute how many miles we have traveled, but, unless I actually see the points, I can not tell whether they are shifted before the train passes.

An omnipotent being could rule the course of this world in such a way that none of us should discover the hidden springs of action. He need not make the sun stand still upon Gibeon. He could do all that he wanted by the expenditure of infinitesimal diverting force upon ultramicroscopic modifications of the human germ.

In this address I have not attempted to add any item to the sound knowledge which I believe our society is gradually amassing. I shall be content if I have helped to clear away some of those scientific stumblingblocks, if I may so call them, which tend to prevent many of our possible coadjutors from adventuring themselves on the new illimitable road.

I see no good reason why any man of scientific mind should shut his eyes to our work or deliberately stand aloof from it. Our Proceedings are, of course, not exactly parallel to the Proceedings of a society dealing with a long-established branch of science. In every form of research there must be a beginning. We own to much that is tentative, much that may turn out erroneous. But it is thus, and thus only, that each science in turn takes its stand. I venture to assert that both in actual careful record of new and important facts, and in suggestiveness, our society's work and publications will form no unworthy preface to a profounder science both of man, of nature, and of "worlds not realized" than this planet has yet known.

SURVEY OF THAT PART OF THE RANGE OF NATURE'S OPERATIONS WHICH MAN IS COMPETENT TO STUDY.

By G. Johnstone Stoney, M. A., D. Sc., F. R. S.[1]

PREFACE.

In the year 1860 Prof. Clerk Maxwell published, in the pages of the Philosophical Magazine, a remarkable investigation, aided by which the present writer in that year drew up for his own information the scheme of magnitudes described in the following pages, from the use of which he has ever since derived advantage when studying the operations of nature, whether those carried on upon a large or on a small scale. (See fig. 1.)

At the suggestion of some scientific friends he now publishes the diagram, in the hope that it may prove of equal assistance to others by contributing toward the formation of a correct estimate of what that little is which man can truly know, and of the contrast which necessarily prevails whenever the boundless range both in time and space of each actual operation in nature is considered in its relation to the limits in both directions at which any clear human knowledge concerning it must stop.

DEFINITIONS.

When interpreting nature's work, we are obliged frequently to speak of high numbers and small fractions. To do this conveniently we shall employ the affix -o to signify a decimal multiple. Thus, a uno will mean some decimal multiple of the arithmetical unit; that is, some member of the series 10, 100, 1000, etc. The uno-eighteen is to be understood as the name of the eighteenth of this series; it is accord-

[1]From a separate copy of the Scientific Proceedings of the Royal Dublin Society, Vol. IX, No. 13, communicated by the author. Printed in The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science, Fifth Series, No. 294, November, 1899, pp. 457–474.

ingly the number represented by 1 followed by 18 ciphers. Similarly a metro will mean some decimal multiple of the meter, and the metro-sixteen will mean the sixteenth of this series of metros. In other words, it is a uno-sixteen of meters. So, again, we shall use the syllable -et for decimal submultiple. Thus the sixthet will mean the sixth of these -ets, that is, a unit in the sixth place of decimals. In this nomenclature the tenthet of a meter is the same as the tenth-metret, i. e., the tenth of the series of metrets or decimal submultiples of a meter. Or, it may be spoken of as the tenthet-meter, using this word as an abbreviation for "tenthet of a meter;" just as we may say half ounce or quarter inch.[1]

MAXWELL'S DETERMINATION.

In the year 1860 the late Prof. Clerk Maxwell published the first determination made by man of any actual molecular interval.[2] The principles upon which he proceeded may be described as follows: In accordance with the kinetic theory of gas, a gas consists of an enormous swarm of little missiles, all alike in each kind of gas, though differing from one gas to another. These molecules dart about among one another with almost incredible activity, and are, to use Maxwell's simile, like the individuals of a swarm of bees which furiously make short flights in every direction, while the swarm as a whole is either stationary or quietly sailing along. In a gas each molecule dashes for-

[1] It is as necessary to be able to write the quantities we have to deal with in some convenient form as it is to be able to describe them briefly. The usual plan is to employ positive and negative powers of 10 to express decimal multiples and submultiples. Another contrivance is to represent them by Roman numerals in the way indicated by the following examples:

As specimens of decimal multiples, let XVI (a uno-sixteen) mean 1 followed by sixteen ciphers, and let 4 VII (four uno-sevens) mean 4 followed by seven ciphers. In multiples the Roman numeral indicates the number of ciphers.

Similarly, to represent submultiples, let VIII^t (an eighthet) be used as the symbol for a unit in the eighth place of decimals, and let 6 XIII^t (six thirteenthets) mean 6 in the thirteenth place of decimals. In submultiples the Roman numeral indicates the decimal place.

In manuscript it is more convenient to employ a little curved line, the left-hand half of the letter "o," instead of the letter "t," which has been used in the last paragraph for the convenience of the printer. The small curved line is easily written, and it is appropriate, as it is the symbol in Pitman's Phonography for the group of letters "tht," or "thet."

We may extend the same convention so as to write in a condensed form multiples and submultiples of the meter, etc. Thus m XVI, 15 m X, IX^t m, and 7 VIII^t m will mean a metro-sixteen, fifteen metro-tens, a ninthet-meter (or ninth-metret), and seven eighthet-meters (or seven eighth-metrets).

When once we have got accustomed to this use of the Roman numerals, they will be found to work more conveniently than the positive and negative powers of 10, which are usually employed.

[2] Philosophical Magazine for 1860, Vol. XXI, p. 19, and Vol. XX, p. 21

ward in an almost[1] straight line till it gets close to another molecule. Then an encounter takes place; the molecules struggle together for an excessively brief period, after which they fling asunder in two new directions. The average velocity with which the molecules dart about had been known before Maxwell's investigation. It is about 500 meters per second in the air which we breathe. It was also known that, except in very high vacua, the molecules are so crowded that their journeys between their encounters can be but short, but the length of these journeys was not known. What Professor Maxwell effected was an actual determination in certain gases of the average length of these "free paths." He did this by showing that upon this average depends what is called viscosity in a gas—that property which gradually brings a gas to rest after it has been disturbed and currents established in it. He further showed that the average length of the free paths is what determines the rate at which gases diffuse into one another. Accordingly, from experiments on viscosity made by Sir George Stokes, and from Graham's experiment on diffusion, he was able to ascertain what the average length of the free paths must be to produce the observed amount of effect. He thus found it to be about six eighthets[2] of a meter—that which would be represented arithmetically by 0.00000006 of a meter—in atmospheric air at the temperature and pressure of the experiments, which we may take to have been a barometric pressure of 760 millimeters of mercury and a temperature of about 17° Centigrade. This length is smaller than any interval which the microscope can show, and yet it is a length which must be regarded as very large among molecular magnitudes.

NATURE'S WORK AT CLOSER QUARTERS.

We can, however, extract from Maxwell's determination information about still smaller quantities. In fact, Clausius had previously been able to show[3] that in the more perfect gases, at ordinary temperatures and pressures, the mean length of the free path is about sixty times what the average spacing of the molecules is at any one instant

[1] The gravitation of the molecules toward the earth must bend the free paths, but the curvature is insensible until, near the boundary of the atmosphere, the attenuation of the air far exceeds any that can be reached in artificial vacua. This bending of the free paths keeps the atmosphere that accompanies the earth from extending outward beyond a short distance. It moreover makes the denser constituents of an atmosphere come to an end sooner than the lighter constituents, so that in the upper regions of an atmosphere the law of the equal diffusion of gases no longer holds. See "On the physical constitution of the sun and stars," Royal Society's Proceedings, No. 105, 1868, pp. 13 and 14; or "Of atmospheres upon planets and satellites," Royal Dublin Society's Scientific Transactions, Vol. VI, 1897, p. 305, or Astrophysical Journal, Vol. VIII, 1898, p. 25.

[2] Subsequent experiments by Maxwell himself on the viscosity of air (Phil. Trans. 1866, p. 258) assign a length of 10.6 eighth-metrets to the average free path. The mean of all the determinations is 7.6 eighth-metrets.

[3] Pogg. Ann. 1858, Vol. III, p. 251; or Phil. Mag. 1859, Vol. XVII, p. 89.

of time. By combining Clausius's estimate with Maxwell's determination, the present writer was able, in 1860, to infer that the average spacing of the molecules of a gas at the temperatures and pressures which prevail in our houses is about a ninth-metret, and that accordingly there are about a uno-eighteen of molecules (1 followed by eighteen ciphers) in each cubic millimeter of the gas. This estimate was communicated to the Royal Society in May, 1867, and will be found in the Phil. Mag. for August, 1868, p. 141. Further, it is known to chemists that there are two chemical atoms in each molecule of many gases. From this, and from the known degree in which vapors contract when they are condensed into the liquid or solid state, we may infer that the average spacing of chemical atoms in solids and liquids lies somewhere in the neighborhood of the tenth-metret (0.0000000001 of a meter), and that accordingly there are something like a uno-twenty-one of chemical atoms in each cubic millimeter of solids and liquids—not exactly that number, but somewhere near it. He thus arrived at an estimate—an estimate, not a determination—as to the number of molecules in a gas, and as to the number of chemical atoms in solids and liquids. Such knowledge is imperfect, but is much better than knowing nothing about the scale on which nature is working in this branch of her operations.

The general results of the information acquired in 1860 were:

1. That the mean length of the free paths of the molecules of air at a barometric pressure of 760 millimeters and at a temperature of 17° C. is about six eighth-metrets. This was a determination.

2. That the mean spacing of the molecules in a gas at the same temperature and pressure is of the same order as[1] a ninth-metret. This was an estimate.

3. That the mean spacing of the chemical atoms of which solids and liquids consist lies somewhere in the neighborhood of a tenth-metret. This, like the last, was an estimate.

[1] In molecular physics, where our estimates, and even our determinations, inevitably fall far short of attaining exactness, it is very convenient to be able to describe the result as being "of the same order as" some specified magnitude.

To give definiteness to this expression, imagine units where there are ciphers in fig. 1. They are a geometrical series, each unit having a value ten times that of the unit to its right. Next form the corresponding series with $\sqrt{10}$ as its factor. This will interpolate a new term between every two consecutive terms of the former series. Thus, on either side of the unit so situated in our table as to represent a ninth-metret, will be terms one of which will have the value $\sqrt{10}$ ninth-metrets, and the other $1/\sqrt{10}$ of a ninth-metret. Now, any quantity between these two limits may be spoken of as "of the same order as a ninth-metret." In accordance with this convention, 3 ninth-metrets, 2 ninth-metrets, 1 ninth-metret, ½ ninth-metret, and ⅓ ninth-metret are all quantities "of the same order as" a ninth-metret. Any of these lengths is better represented by a ninth-metret than it would be by either a tenth-metret or an eighth-metret.

When we deduce the number of molecules in a gas from the spacing of the molecules we have to deal with the cube of an already estimated number, and accordingly

4. That the number of molecules in a cubic centimeter of gas at standard temperature and pressure is somewhere in the neighborhood of a uno-twentyone. This follows as a corollary from (2).

5. That the number of chemical atoms in a cubic centimeter of a solid or liquid is a number of the same order as a uno-twentyfour. This follows from (3).

6. That the masses of the chemical atoms probably lie between the twentysecondet and the twentyfifthet of a gram. This follows from (4) and from the known densities of solids and liquids.

The tenth-metret, the smallest of the above measures, is the ten-thousand-millionth part of a meter. It is about the two-thousandth part of the smallest interval which the best microscope can detect when most carefully handled.

Another branch of physical inquiry has introduced us into the same region of magnitudes, and has even carried us farther. The wavelengths of visible light range from 38 to 76 eighth-metrets, and can, by methods which will be described farther on, be measured with such marvelous precision that it is possible to detect differences of wavelength which amount to a very small fraction of a tenth-metret.

NATURE'S OPERATIONS ON A LARGE-SCALE.

When we turn our attention to nature's operations on the large scale, we find that the greatest lengths we can as yet succeed in measuring are the distances of those few stars which have perceptible parallax.[1] The distances of these stars from the solar system range from four to fifteen metro-sixteens, and it is not likely that any star could send us light enough to be visible in any of our telescopes if a thousand times more remote. At a distance, then, of about 10,000 metro-sixteens—that is, at a distance of about a metro-twenty—our

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the range implied by the phrase "of the same order as" becomes widened. It now ranges from $\sqrt{1000}$ times the assigned value (in this case a uno-eighteen per cubic millim.) to $1/\sqrt{1000}$ times this value; so that it includes 30, 20, 10 times, and 1/10, 1/20, and 1/30 of a uno-eighteen. Any of these numbers is much better represented by a uno-eighteen than it would be by a uno-fifteen, the number which is a thousand times smaller, or by a uno-twenty-one, the number which is a thousand times larger. The knowledge thus reached as to the number of molecules that are present may seem very indefinite; but it is far from being valueless.

[1] Attempts have been made to infer the parallax of binary systems from a spectroscopic determination of the difference of velocity in the line of sight of the constituent stars, combined with the known periodic time and the apparent angular size and form of the system. This method has been applied to γ Virginis and to γ Leonis with results which are not yet free from doubt on account of the extreme delicacy of the observations, but which seem to place these stars at distances, in the case of γ Virginis, of about 60 metro-sixteens, and in the case of γ Leonis of 150. These are distances which are one step of our scale farther—i. e., about ten times farther—from us than those of which the parallax can be directly measured. (See Astr. Nach. No. 3510, or Nature for August 25, 1898.)

knowledge of the starry universe comes to an end. It is perhaps possible that the great nebula in Andromeda and a few other nongaseous nebulæ are stellar systems distinct from that of which the Milky Way is the outlying portion, and which is commonly spoken of as the stellar universe. If so, such of these other "universes" as can be visible to us probably lie within a sphere which extends into the space beyond our stellar system, perhaps some hundred times farther than the boundary of the Milky Way, and may accordingly need, to represent the distances of some of them, numbers inserted in the next column of our table (fig. 1). Accordingly, the column of metro-twentyones is in the table indicated as one of those included within the range of what man possibly already knows something about.

From this preliminary survey it appears that man is only acquainted with a strictly limited portion of the scale upon which the real operations of nature are being carried on. All her operations upon an ultra-stellar scale, all her activities at infra-molecular degrees of proximity, are kept from our view by that heavy veil of Isis which man's limited senses and his restricted intellectual powers can not lift. It raises us in the scale of thinking beings to see clearly where our knowledge must end, and to have ascertained definitely which part of the boundless range of nature's actual operations is that which human powers are able to gauge and which human minds can adequately grasp. The survey may be rendered definite with the help of the table comprised in fig. 1, in which numerical digits are to take the place of some of the ciphers. According to the place where we insert these numbers we can make them express by how many meters, or by what fraction of a meter, we are to measure any of the magnitudes with which man has become acquainted throughout the whole range of his study of nature.

In this table metros mean decimal multiples of the meter; metrets mean its decimal submultiples, and kīlem (to be pronounced with the *ī* long,[1] as in mile) is used as convenient English for the French "kilometer." The first few places in the table, and the last four or five, lie beyond the range of our present knowledge. Nevertheless, they are included, in order that the table may not be unduly shortened by temporary ignorance on our part, but may provide a large margin for possible future discoveries.

The significance of the survey is best appreciated by examining separately the four groups into which the table is divided, and it is convenient to begin with Group C, as it includes the measures most familiar to us.

GROUP C (LABORATORY MEASURES).

Group C extends from kīlems (kilometers) on the left, down to tenths of a micron on the right. The central subsection *v* includes the measures most in use in our laboratories, from meters down to tenths

[1] In χιλιάς, "a thousand," and in all Greek words derived from χιλιάς the ι is long.

ULTRA-

STELLAR

REMOTE-

NESS.

Uno-twenty of Kilems.

u

000 000 0 00

UNKNOWN.

Some nebulæ

LIGHT

TAKES.

10,560,000 years.

0 00

of a millim or millimeter. Subsection *u* includes those larger measures which men have also in everyday use—from tenths of a meter up to kilems or kilometers. The third subsection *w*, from millims (millimeters) down to tenths of a micron, covers the entire range of the microscope, and indeed travels somewhat beyond the grasp of that instrument, since the smallest interval at which two objects can be seen as two by the best immersion objectives supplemented by the best immersion condensers, and most carefully handled, is but little less than two-tenths of a micron, which is the one hundred and twenty-seven thousandth of an inch; whereas subsection C*w* extends twice as far, i. e., down to one-tenth of a micron. This brings us within the border of the next group—the group of molecular intervals—almost all of which lie farther beyond the reach of the microscope than microscopic objects lie beyond the grasp of the naked eye.

GROUP D (MOLECULAR QUANTITIES).

On the borderland between groups C and D we find the lengths of waves of light, all of which can be represented by numbers inserted in the column which is the extreme right-hand column of Group C and the extreme left-hand column of Group D. The wave-lengths of visible light extend from a little less than 4 seventh-metrets to a little less than 8 seventh-metrets. The ultra-violet light which reaches the Earth from the Sun carries us down to about 3 seventh-metrets; the light which has been explored by Professor Hartley extends the range nearly down to 1½ seventh-metrets, and Professor Schumann has got down to light whose wave-length is about 1 seventh-metret. Thus the wave-lengths of light come all of them upon the column which, in our table, is on the border between microscopical magnitudes and molecular. Almost the only true molecular length long enough to be measured in this column is the average free path in attenuated air or in some other gases. On the other hand, when air is as dense as it is at the surface of the Earth, the average lengths of these free paths has to be recorded in the next column (the column of eighth-metrets), and may be considered as about the longest of legitimate molecular intervals. According to Maxwell's determinations, it seems to be about 7½ eighth-metrets. The wave-lengths of Röntgen rays perhaps extend into this column.

One or two units in the next column, the column of ninth-metrets, may be taken as about the average interval at which the molecules of ordinary air are spaced; and a unit or two in the following column, that of tenth-metrets, is about the average spacing of the chemical atoms of which solids and liquids consist. It will be seen that none of these intervals extend beyond D*u*, the subsection of large molecular magnitudes.

When we attempt to penetrate farther we find that we can only obtain a glimpse of those more fundamental events in Nature, the size

of which or the range of which has to be measured in the next three columns, i. e., in tenthets of the decimeter, of the centimeter, of the millimeter. These all come into subsection *v*, the subsection of medium molecular magnitudes. That there are events of this kind going on unremittingly within every chemical atom is indicated to us by the lines in the spectra of the chemical elements, for these are caused by such events. Here, at present, human knowledge stops. The whole of the work which Nature is carrying on at still closer quarters, although we are well aware that it must lie at the basis of all the rest, is totally hidden from our view, except so far as the speculations of mathematicians may doubtfully attempt to probe it; and in all such conjectures the speculator has to substitute something very much simpler for what is really going on. However, Group D is represented in our diagram as including another subsection, *w*, going 10,000 times farther still, in order by this extension to provide for the possibility of future discoveries which we hope may some day be realized.

Very little is known about the events going on within chemical atoms, of which we have found that the range is to be measured in tenthet-decimeters, tenthet-centimeters, or tenthet-millimeters, and even the fact that there are such events lies near the limit of our knowledge; and yet these excessively minute quantities can be dealt with accurately when they present themselves as differences of wave-length. This is truly astonishing when we remember that we are here measuring lengths that are from 100,000 to 1,000,000 times smaller than the most minute interval that can be detected by the microscope—as much smaller than a micron as a tenth or hundredth of an inch is less than three-quarters of a mile. Nevertheless these lengths can be determined with precision because the position of a line in the spectrum depends on its wave-length, and the difference of the wave-lengths of the closest lines which can be photographed as double is excessively small; and again, because two rays with a still smaller difference of wave-length may give rise to interference effects which can be detected by the interferometer. By the spectrometer measures can be carried at all events as far as the fiftieth of a tenthet-meter, i. e., as far as to one or two tenthet-centimeters, while with the interferometer determinations can probably be carried one step of our scale farther, i. e., to one or two tenthet-millimeters. Here, for the present, our powers end; and we can not fail to be impressed by the extraordinary accuracy which has been attained in measuring wave-lengths by the methods spoken of above. It is a degree of accuracy which ascertains the length of a wave of light within a millionth of its entire length, thus equaling and even surpassing the best results obtained when comparing with excessive care international standard yards or meters; in which a determination within one fifthet (the 100,000th) of the whole length is probably the most that can be fully relied on.

GROUP B (PLANETARY INTERVALS).

We have next to direct our attention to Nature's operations on a great scale, and first to Group B, which deals with events within the solar system. This group, like the others of our survey, may conveniently be divided into subsections—*u*, *v*, and *w*.

B*u*, the subsection of large planetary measures, indicates the place in our table in which to record the distances of the planets from the Sun, or from one another, as is seen from fig. 2. These distances are most conveniently read out as so many metro-tens.

The next subsection, *v*, makes similar provision for representing the distances of the satellites from their primaries, and for recording the size of the Sun, which belongs to the same order of magnitude. This appears from fig. 3, in which the distances may conveniently be expressed as so many earth-quadrants, meaning by the "quadrant" 1,000 stages, or 10,000 kilems, which is approximately the distance on the earth's surface from the equator to the pole.

FIG. 2.

Distances of the Planets from the Sun, in metro-tens.

[The subsection Bu provides for all of these.]

GROUP B. PLANETARY INTERVALS.			
Bu			
0 000	000	000	0
1	= One metro-ten.		
5 · 8	Mercury.		
10 · 8	Venus.		
15	Earth.		
22 · 8	Mars.		
Here come the minor planets.			
78	Jupiter.		
143	Saturn.		
287	Uranus.		
450	Neptune.		

FIG. 3.

Distances of Satellites from their Primaries, expressed in earth-quadrants.

[The subsection Bv provides for all of these.]

GROUP B. PLANETARY INTERVALS.			
		Bv	
0	000	000	000 0
Quadrant, or Metro-VII		1	
Radius of Sun		69 ·	75
Distances of—			
Moon		38 ·	4
Satellites of Mars		0 ·	9
		2 ·	3
(Nova)		18	
Satellites of Jupiter ..		43	
		68	
		109	
		192	
Satellites of Saturn ...		19 ·	5
		25	
		31	
		39 ·	5
		55	
		128	
		162	
		380	
(Nova)	1	200	
Satellites of Uranus ..		19 ·	8
		27 ·	5
		45	
		60	
Satellite of Neptune ..		35 ·	4

There remains the *w* subsection, the subsection of smallest planetary measures. These stand related to the other planetary distances in somewhat the same way as microscopical intervals are related to other laboratory measures. They may be called geographical intervals, since in this subsection we measure the radii of the planets and distances on their surfaces—quantities which can conveniently be expressed as so many stages, each stage being 10 kilems (or 6¼ miles[1]), as shown in fig. 4.

FIG. 4.

Radii of planets, expressed in stages.

[The subsection Bw provides for all of these.]

GROUP B. PLANETARY INTERVALS.		Bw
0 000	000	000 0
Earth-quadrant,	1	000
Radius of sun,	69	750
Radii of—		
Mercury,		240
Venus,		610
Earth,		637
Mars,		340
Jupiter,	7	000
Saturn,	6	100
Uranus,	2	500
Neptune,	2	600

FIG. 5.

Examples of measured stellar distances, expressed in metro-sixteens.

[The subsection Aw provides for all of these.]

GROUP A. STELLAR DISTANCES.		Aw
0 000	000	000 0
One metro-sixteen,		1
Distance at which parallax would be 1″,		3·1
Distances of—		
α Centauri		4
61 Cygni		6
Sirius		8
α Lyræ	1	5
Limit of distance that can be ascertained by parallax	3	0

GROUP A (STELLAR DISTANCES).

The last group is that of stellar distances. These are most conveniently measured in metro-sixteens.

The four units we have found it most convenient to use in dealing with large magnitudes are very simply related to one another, as appears from the following list of them:

The unit we have found it convenient to use for geographical distances is the stage, the stage being a million of centimeters, or 10 kilems, or 6¼ miles.

[1] That is, 6¼ metric miles. In science the mile of 1,600 meters, the furlong of 200 meters, the chain of 20 meters, and the perch or pole of 5 meters should always be used instead of the so-called "imperial" measures of the same names. Here the old or imperial measures are to the new or metric measures in the ratio of 100.582 to 100, which is the same as the ratio of 172.8 to 171.8, between which last numbers the difference is 1.

The unit for the distances of satellites from their primaries is the earth quadrant, the quadrant being 1,000 stages.

The unit for the distances of planets from the sun is the metro-ten, the metro-ten being 1,000 quadrants, which is the same as a million stages.

The unit for stellar distances is the metro-sixteen, the metro-sixteen being a million metro-tens, or one billion stages.

The position which the metro-sixteen, or billion stages, occupies is indicated on the table. Light in the open ether takes 1.056 year (nearly a year and three weeks) to travel a metro-sixteen, so that the metro-sixteen is a little more than what in astronomy has sometimes been called the "light year."

The distances of the nearest stars, those few of which the parallax can be directly measured,[1] fall within Aw, the subsection of smallest stellar distances, as appears from the examples shown in fig. 5.

Thus Aw includes the distances of the nearest stars along with substellar distances; that is, distances from the sun to stations between the solar system and the nearest star. Such substellar intervals probably exist between the stars of a cluster.

The farthest stars visible to us are probably less than 10,000 times farther than the few whose parallax can be directly measured, since a star sending us one hundred-millionth part of the light of Sirius would probably not be visible.

If this view is correct Av, which is the middle subsection of Group A, provides places to represent the distances of the stars visible to the naked eye, along with all those which our telescopes can reach. Accordingly, a sphere of which the radius is a metro-twenty, or some two or three metro-twenties, would include our whole stellar universe. Now, our table extends 1,000 times beyond the column of metro-twenties, so that the greater part of subsection Au makes provision for measuring distances as much farther out than the most distant star known to us, as a sphere with a mile for its radius ranges beyond a concentric sphere, with less than a yard for its radius.

It is just possible that the inner portion of this extension is necessary to represent man's present knowledge; that, in fact, some of the non-gaseous nebulæ—e. g., the great Nebula in Andromeda—may be stellar "universes" distinct from ours and located somewhere within the larger sphere. If so, when we looked upon the speck of light which brightened up in the Nebula of Andromeda a few years ago we may have been then actual spectators of an event which really happened some hundreds of thousands of years ago, the waves of wireless telegraphy which communicated the information to us having occupied the whole of that immense time upon their swift journey.

[1] See footnote on page 211.

OF THE RELATION BETWEEN LIGHT AND OUR SCALE.

This leads us to consider the relation in which light stands to our survey. It is useful to do so, since it gives unity to our survey to con-

FIG. 6.

STELLAR DISTANCES.				PLANETARY.	
0	000	000	000	0	000 000
9	467		94	6	7

9 467 — is the distance which light will travel in one hundred millions of years.

94 6 7 — is the distance which light will travel in one year.

sider how our table is related to light, which in one direction reaches, by the minuteness of its waves, the border land of molecular magnitudes and in the other direction, by reason of its great speed, can traverse immense distances in periods of time which we can grasp. The relationship is exhibited in the lower section of fig. 1, which gives the times which light must have to enable it to reach us from the distances represented by a unit in each of the indicated parts of the table. The information there recorded may be supplemented by that added in fig. 6.

ON THE MEASUREMENT OF TIME.

The same table may be employed for measuring time. Intervals of time for the purposes of physical inquiry are best measured by the distances over which light in the open ether would travel in those periods. In this way measures of distance become measures of duration upon that scale upon which a metro-eight (which is the same as the centimo-ten) represents one-third of a second—a scale which in practice is found to be very convenient, especially for the study of molecular physics. To represent a second of time on the diagram, insert the digit 3 instead of the cipher which occupies the middle place in the planetary group of positions. In this way of measuring time 300 meters of time (1,000 feet[1]) is the same as the millionth of one second.

OF MOLECULAR EVENTS.

In molecular physics the periods of time which have to be dealt with are almost inconceivably shorter than any to which we are accustomed. The unit of time which the present writer has found the most generally convenient is the micron of time—the time which light takes to advance

[1] That is, 1,000 metric feet. In science the yard of 9 decims, the foot of 3 decims, and the inch of 25 millims should always be used instead of the so-called "imperial" measures of the same names. Here the old or imperial measures are to the new or metric measures in the ratio of 101.6 to 100, or in the ratio of 63½ to 62½, or in the ratio of 127 to 125. It may be useful to point out that lathes and dividing engines provided with Whitworth screws the pitch of which is known in imperial inches may be made to produce screws or graduate scales in the metric measures by simply introducing two change-wheels, one with 127 and the other with 125 teeth.

one micron forward in the open ether. It is the hundredth part of the jot (or fourth-metret of time), which unit he found it convenient to use in his memoir on the production of double and multiple lines in spectra by perturbating forces acting on the electrons. (See Sc. Trans. R. D. S., Vol. IV, p. 565.)

One of the conveniences of the proposed way of representing time is its perfect flexibility. In each investigation we may select as our unit of time that of the whole decimal series which happens to be the most convenient to use in the investigation. In the above-mentioned inquiry it happened that a relatively large unit was the most convenient. In other inquiries the micron, which is 100 times briefer, is a more convenient unit, and in some few, in which very much smaller periods of time were under consideration, the tenth-metret of time was employed.

The micron of time is the XIV[t] (fourteenthet) of the third of a second; that is, the three hundredth part of the billionth[1] of a second. To magnify it till it becomes one second of time is the same process as to magnify the fifth part of the thousandth of a second until it becomes 1,900 years, i. e., the whole duration of the Christian era. It is instructive to bear this in mind when dealing with molecular events.

In dealing with molecular events it is well to conceive a magnified model of what is really going on, in which all lengths are so enlarged and all times so much prolonged as to bring both within the range of what we can conveniently perceive. In order to do this, the magnification with respect to time will need to be greater than that with respect to space. A good magnification for many purposes is a magnification of all lengths by a uno-ten and a magnification of the durations by either 3 or 6 uno-fourteens.[2] (See Scientific Proceedings R. D. S., Vol. VIII, p. 372, or Philosophical Magazine for October, 1895, p. 381.)

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[1] A billion in Great Britain is a million of millions.

[2] The magnification of molecular intervals by a uno-ten may be called standard magnification of molecular events; because it means the representing of molecular events which require to be recorded in Group D by a model of them so large that it records them in the corresponding parts of Group C, the group of magnitudes with which we are most familiar.

A magnification of molecular magnitudes which is a thousandth or a ten-thousandth part of this standard will often be found useful. On the former of these scales chemical atoms may be represented by beads, on the other by very fine sand used in hourglasses, while in the standard model the chemical atoms are somewhat like quadrupeds of various sizes crowded together.

The magnification of the durations by 3 XIV (three uno-fourteens) means that each micron of time becomes a second, so that an event in the molecular world which occupies a fraction of a micron of time is represented by an event of the same kind in our model which occupies the same fraction of a second. This, in the case of a great number of molecular events, brings the events occurring in the model within the range of human perceptions. If the time magnification is by 6 XIV (six uno-fourteens), a molecular event that occupies some fraction of a micron of time is represented by an event in the model which occupies the same fraction of two seconds; and this is sometimes convenient where we wish to compare molecular motions with the motions of pendulums or of the limbs of animals, since a pendulum which beats seconds is one whose periodic time is two seconds.

When by this or other means we have attained the power of viewing events from the molecular standpoint, we begin to perceive that chemical reactions, even those that occur with explosive violence, are far from being the sudden events they seem to ordinary human apprehension. What is really occurring in nature is a protracted and eventful struggle between the members of two opposing armies, each individual of which has his own personal history during the struggle, and is fully occupied with his own acts, which are, perhaps, as many, as various, and as different from those of his neighbors as are the thoughts and acts of the individual soldiers during the progress of a battle.

What comes under the observation of a chemist is the state of things which preceded this eventful period and that other state of things which followed it. As to what Nature has been really doing his record is a blank. It is not unlike the inscription one often sees upon tombstones, "Born in such a year; died in such another," while the real event, the intervening life, is passed over in silence.

How, then, ought the student of molecular physics to record the incidents of the eventful period of a chemical reaction? The incidents of the operations that are then going on are vastly more numerous, are probably as various, and are done with as little hurry, when we view them from the molecular standpoint, as are the acts of human artisans or of other animals while accomplishing some piece of work; and they are, relatively speaking, persisted in for an almost immeasurably longer time, inasmuch as the fifth of a thousandth of a second in the molecular world corresponds to something like one thousand nine hundred years in ours.

An estimate of this kind is of service, because it leads us to see that biological and chemical processes, even where they seem to us to take place with suddenness, are from the molecular standpoint protracted events consisting of individual transactions, each of which can only occur when the opportunity presents itself. They are not the outcome of the ordinary current of molecular events, but, on the contrary, each step of progress in them may have to wait long for some very exceptional combination of circumstances to arise. The present writer once saw doublets thrown thirteen times in succession with unloaded dice at the close of one game of backgammon and at the beginning of the next game. It must be an unusual experience for a human being to be witness to so rare an event. The probability of it is only 1 in 13,060,700,000. Yet so great is the number of molecules in a gas and so frequent their encounters that some millions of cases occur every second in every cubic micron of the air about us in which an encounter between molecules has taken place under conditions as exceptional as the above; and equally unusual events probably occur some thousands of times more frequently in the encounters between the molecules of two liquids or of a liquid and a solid. It is thus that chemical reac-

tions and events in biology can extend over a duration which is appreciable by us, even in the case of explosions, the fact being that in all such events it is their excessive slowness from the molecular standpoint that has to be accounted for. On the other hand, the frequency, when estimated from the human standpoint, of events which are excessively rare when viewed from the molecular standpoint, has enabled all the constituents of an atmosphere to escape from the moon in the time which has elapsed since the moon became separated from the earth, and occasions such a leaking away from the upper regions of the earth's atmosphere of hydrogen and helium, the atmosphere's lightest constituents, as would become appreciable within a few millions of years were it not that these gases are being continuously filtered into the atmosphere from beneath.

CONCLUSION.

No physicist can consult the diagram presented in figure 1 without being struck by its resemblance to an absorption band in a spectrum. Nature is occupied in working everywhere over the entire spectrum; man's knowledge of her works is confined to what occurs within this one absorption band. How much changed would be the aspect under which the human mind would have had to view nature if the position of the absorption band had occupied a different place—if, for example, the range of our knowledge had been Groups B, C, D, and E, instead of A, B, C, and D, with such a full knowledge of molecular objects and events as we now enjoy of objects that range from kīlems down to microns, and with such a lessened knowledge of Group C as we now have of planetary events! An equally startling change would be made if the range had been shifted the other way—if we had no knowledge of microscopic or molecular events, just as we now possess none of those which go on within and beyond subsection *w* of Group D; if, at the same time, we had only a smattering of knowledge about Group C, such as the fragments we are now able with difficulty to obtain about Group D, accompanied, however, by some real acquaintance with the immense universe that lies beyond Group A.

Along with these considerations we should ever bear in mind that behind and above the great universe of natural objects, and the true cause of all the rest, there stands the Autic Universe, the mighty Autos, to which the present writer endeavored to draw attention in an earlier paper, and of which the THOUGHTS that are our real selves are part. (See Scientific Proceedings of the Royal Dublin Society, Vol. VI (1890), page 475.)

APPENDIX.

A standard model of molecular phenomena is described above, in the footnote on page 219.

The writer can strongly recommend the corresponding standard model of celestial phenomena. This is made by taking tenthets of all

on the bank of time. Geology owes immeasurable obligation to this eminent physicist for the deep interest he has taken in its problems and for the profound impulse which his masterly computations and his trenchant criticisms have given to broader and sounder modes of inquiry.

At the same time it must be recognized that any one line of reasoning, however logically and rigorously followed, is quite sure to lead astray if it starts from limited and uncertain premises. It is an easy error to press the implications of any single phase of the complex phenomena of geology until they shall become scarcely less misleading than the looser speculations which they seek to replace. A physical deduction which postulates an excessively short geological history may as easily lead to false views as did the reckless license of earlier times. Interpretations of geological and biological phenomena made under the duress of physical deductions, unless the duress be certainly known to be imperative, may delay the final attainment of the real truth scarcely less effectually than interpretations made on independent grounds in complete negligence of the testimony of physics. It is in the last degree important that physical deductions and speculations should be regarded as positive limitations only so far as they are strictly demonstrative. Falling short of demonstration, they are worthy to be regarded as moral limitations only so far as they approach moral certainty. In so far as they are drawn from doubtful assumptions, they are as obviously to be placed in the common category of speculations as are those tentative conceptions which are confessedly but the possible foreshadowings of truth. The fascinating impressiveness of rigorous mathematical analyses, with its atmosphere of precision and elegance, should not blind us to the defects of the premises that condition the whole process. There is perhaps no beguilement more insidious and dangerous than an elaborate and elegant mathematical process built upon unfortified premises.

Lord Kelvin's address is permeated with an air of retrospective triumph and a tone of prophetic assurance. The former is fairly warranted to the extent that his attack was directed against the ultra wing of the uniformitarian school of the earlier decades. It might be wholesome, however, to remember that there were other camps in Israel even then. There were ultra-conservatives in chronology as well as ultra-radicals. There were ultra-catastrophists as well as ultra-uniformitarians. Lord Kelvin's contributions have as signally failed to sustain the former as they have signally succeeded in overthrowing the latter. The great body of serious geologists have moved forward neither by the right flank nor by the left, but on median lines. These lines have lain, I think, rather in the field of a qualified uniformitarianism than in the field of catastrophism. Even the doctrine of special acceleration in early times, or at other times, has made only

qualified progress toward universal acceptance. The body of competent geologists to-day are probably more nearly disciples of Hutton, Playfair, and Lyell than of their opponents. But such is the freedom and the diversity of belief, of attitude and of method, among geologists that as a class they can not be placed either here or there in the schools, nor could they thirty-five years ago.

But we are not primarily concerned with these matters of the schools and of the past. The address presses upon our attention matters of present interest and of profound importance. Referring to his former wide-ranged estimate of the time of the consolidation of the earth, Lord Kelvin says that "we now have good reason for judging that it was more than twenty and less than forty million years ago, and probably much nearer twenty than forty" (Science, May 12, p. 671), and he gives qualified approval to Clarence King's estimate of twenty-four million years. In the course of the address he speaks of "strict limitations," of "sure assumption," of "certain truth," and of "no other possible alternative;" he speaks of "one year after freezing," and even of "half an hour after the solidification;" he speaks of "a crust of primeval granite," of a depth of "several centimeters," and of other details of dimension and of time and of certitude so specifically and so confidently that it must encourage, in the average reader, the impression that the history of the earth is already passing into a precise science through the good offices of physical deduction. Is this really true? Can the uninstructed layman or the young geologist safely repose confidence in these or any other chronological conclusions as determinate? Can these definite statements, bearing so much the air of irrefutable truth, be allowed to pass without challenge? What is their real nature and their true degree of certitude when tested respecting their fundamental postulates and their basal assumptions?

With admirable frankness Lord Kelvin says (Science, May 12, p. 672):

"All these reckonings of the history of underground heat, the details of which I am sure you do not wish me to put before you at present, are founded on the very sure assumption that the material of our present solid earth all round its surface was at one time a white-hot liquid."

It is here candidly revealed that the most essential factor in his reasonings rests ultimately upon an assumption, an assumption which, to be sure, he regards as "very sure," but still an assumption. The alternatives to this assumption are not considered. The method of multiple working hypotheses, which is peculiarly imperative when assumptions are involved, is quite ignored. I beg leave to challenge the certitude of this assumption of a white-hot liquid earth, current as it is among geologists, alike with astronomers and physicists. Though but an understudent of physics, I venture to challenge it on the basis of physical laws and physical antecedents.

By way of preface it may be remarked that the postulate of a white-hot liquid earth does not rest on any conclusive geological evidence, however generally it may be entertained as a probable hypothesis. Students of the oldest known rocks are not yet agreed that these are all igneous even. But granting that they may be all either igneous or pyroclastic, there is a wide logical gap between this admission and the postulate that they were all liquid at one time and enveloped the whole earth. Looking quite in the opposite direction is the testimony of the complex structure and intricate combination of rocks, diverse at once in chemical, mineralogical, and structural characters, which the basement complex presents. The relations of the great batholite-like masses to the enveloping foliated rocks, and of analogous combinations of intrusive aspect, imply the presence of a portion of the basement complex in the already solid state when the remainder entered it in the liquid state. It would be a bold petrologist who would insist that it has been demonstrated that the basement complex is simply the molten envelope of the primitive earth solidified in situ, however much he might be disposed to entertain this view among his working hypotheses. It would be petrological hardihood to maintain that it was even a "sure assumption." Without denying that the basement complex may be the direct or the indirect offspring of a supposed molten state, no dogma of certitude is now admissible on geological grounds.

The hypothesis of a primitive molten earth is chiefly a deduction from the high internal temperature and from the nebular hypothesis. But it remains to be shown that the high internal temperature may not also be a sequence of an earth which grew up by meteoric accretion with sufficient slowness to remain essentially solid at all stages. An attempt has recently been made to show that a highly heated state of the interior of the earth would have resulted from the self-compression of the mass during its accretion.[1] The methods of reasoning employed in this attempt were identical with those of Helmholtz relative to the heat of the sun, save that they were applied to a solid body. The computations of Mr. Moulton seem to indicate that gravitative concentration may have been an adequate cause of internal heat. In addition to this the thermal effect of molecular change and tidal kneading require recognition. Until these agencies are rigorously tested and found wanting, inferences based on the alternative hypothesis can scarcely be the ground of sure assumption. The irregular distribution of internal heat is more notably in harmony with the hypothesis of internal compressive generation than with that which makes it a residuum of a molten state whose temperature should be approximately uniform. If this irregularity be assigned to volcanic action, it must be remembered that vulcanism is itself a part of the irregularity and

[1] "A group of hypotheses bearing on climatic changes."—Jour. Geol., Vol. V, No. 7, October-November, 1897, p. 670.

adds to the burden of explication. Both hypotheses ultimately appeal to the same source, the gravitative descent of the earth's substance. Their differences lie in the modes of action assumed, respectively, and these modes are determined by the antecedent conditions of aggregation. Has it been demonstrated that these antecedent conditions were of the one kind and not of the other?

Lord Kelvin obviously assumes a nebulous state of the earth as the controlling antecedent condition. It is not quite clear whether he adopts the complete gaseous theory of Laplace, including the earth-moon gaseous ring, or not. Apparently, however, he has not adopted the gaseous earth-moon ring, but has substituted therefor a meteoroidal ancestry for the earth, for he says (p. 706):

"Considering the almost certain truth that the earth was built up of meteorites falling together, we may follow in imagination the whole process of shrinking from gaseous nebula to liquid lava and metals, and solidification of liquid from central regions outward."

A little further on he speaks of "the gaseous nebula which at one time constituted the matter of our present earth."

Without feeling quite certain that I am not in error, I interpret these sentences to mean that the matter of the earth was in a meteoroidal condition just previous to its falling together, and that it passed into the gaseous condition as a result of the heat of impact, and that from thence it shrank into the liquid and later into the solid state. If this be correct, it would be interesting to learn on what grounds the older hypothesis of a nebulous ring, once regarded as a quite sure assumption, has been abandoned, and whether the reasons for that abandonment do not bear adversely also on this modified phase of the gaseous hypothesis. The strongest objection recently urged against the Laplacean gaseous ring is the apparent inability of the feeble gravity of such a ring to overcome the high molecular velocities of its lighter constituents at the high temperatures necessary to maintain the refractory material of the earth in a gaseous condition.[1] In addition to this radical objection to the gaseous earth-moon ring, there is the extreme probability that, if formed, it would cool below the temperature of volatilization of rock substance before it would concentrate into a globe.

The studies to which reference has just been made seemed to show that even in the globular form it is doubtful if the earth could be volatilized without the dissociation of its water and the loss of its hydrogen by molecular projection away from the earth. The inquiry seemed even to raise a doubt whether the vapor of water, as such, or the atmospheric gases could be retained at the temperature of rock volatilization; indeed, it seemed that the oceanic and atmospheric constituents might even be in jeopardy at the temperature of white-hot

[1] "A group of hypotheses bearing on climatic changes," Jour. Geol., Vol. V, No. 7, October-November, 1897, pp. 658–668.

lava. Without insisting that these molecular inquiries are demonstrative—for they only profess to be preliminary—they seem, at least, to justify the radical inquiry whether the hypothesis that the earth was once a gaseous nebula can be entertained with any confidence, in the light of modern molecular physics. As an abstract proposition in physics addressed to physicists, would Lord Kelvin feel free to assert that the water now on the surface of the earth would be retained within its gravitative control if the earth were heated so that its rock substance was volatilized? May I be pardoned for inquiring whether Lord Kelvin has not joined the company of geologists and neglected some of the physical considerations that bear pertinently on the problem in hand?

But passing this point and striking hands with Lord Kelvin in assuming "the almost certain truth that the earth was built up of meteorites falling together," what imperative reason is there for inferring a gaseous or even a white-hot liquid condition as a result? It goes without saying that the energy of impact of the falling meteorites would be sufficient, under assumable conditions, to give rise to the liquid condition, and much more, but the actual condition that would be assumed by the earth would be dependent wholly on the rate at which the meteorites fell in. If they fell in simultaneously from assumable distances an intensely hot condition may be predicated with all the confidence of logical certitude. If they fell at as great intervals as they do to-day, a low surface temperature may be predicated with equal certainty. If they fell in at some intermediate rate, an intermediate thermal state of the surface must be postulated. No physical deduction can be more firm than that the temperature of the surface of the earth would be rigorously dependent on the rate of infall, so far as the influence of infall alone is concerned. Before a white-hot condition can be regarded as a safe assumption, it must be shown that the meteoroids would necessarily fall together at a highly rapid rate; otherwise the heat of individual impacts would be lost concurrently, as is now the case, and would not lead to general high temperature.

Now, has Lord Kelvin, or any other of our great teachers in physics or in astronomy, followed out to a final conclusion, by the rigorous processes of mathematics, the method and rate of aggregation of a multitude of meteorites into a planet, so as to be able to authoritatively instruct us as to the rapidity at which the ingathering would take place? Can the problem be solved at present with any such close approximation to precision as to determine whether a liquid or a gaseous state would or would not ensue? I assume that the most probable hypothesis relative to the distribution and movements of the meteorites is one that assumes that they consisted of a swarm or belt revolving about the sun in the general neighborhood of the present orbit of the earth; in other words, some form of meteoroidal substitute for the

gaseous ring of the Laplacean hypothesis. The hypothesis may, doubtless, diverge much in detail, and, indeed, in some very important factors, but I assume that no radical departure from this can be entertained without endangering the peculiar relations of the earth to the rest of the solar system and the harmonious relations of the whole; without, in other words, jeopardizing the consanguinity of the planets. If a distribution of meteorites bearing any close resemblance to the Saturnian rings, the foster parents of the nebular hypothesis, be assumed, a definite problem is presented for determination. If the rings of Saturn, which are quite certainly formed of discrete solid matter, were to be enlarged so that they should lie outside Roche's limit, and so escape the sphere of specially intense tidal strain which will permit no aggregation, what reason is there to think that they would gather together precipitately? Does the tidal influence, which, within Roche's limit, is able to tear a satellite to pieces, cease instantly outside the limit and give place to a precipitate tendency to come clashing together? On the contrary, is it not difficult to demonstrate, by rigorous processes, even the method by which the meteorites will aggregate, much less their rate, or even to demonstrate that, apart from extraneous causes, they will fall together at all? Is not the presumption in such a case favorable to a slow rather than to a rapid aggregation? If a distribution like the meteoroidal swarms that are associated with the comets of the solar system be assumed, a definite problem is set concerning which some appeal to observation is possible. Here the observed tendency is toward dispersion rather than aggregation. In either of these assumptions, or in any other assumption, the problem involves the balance between gravitative forces, revolutionary forces, and tidal forces, and the gravitative forces are not simply those between the meteorites mutually, but those between the meteorites and the central solar body and the exterior planetary bodies, a complex of no mean intricacy. Is it certain that these forces would be so related to each other as to produce a swift ingathering of the whole swarm or belt, or, on the other hand, an ingathering prolonged through a considerable period? If the latter be the case (and, in the absence of demonstration, is it unreasonable to think it quite as probable as the opposite?), are there any imperative grounds for assuming that a liquid state of the earth would result? Until the rate of aggregation is worked out fully and rigorously, are there any moral prohibitions, strict or otherwise, to a free interpretation of geologic and biologic evidence on its own grounds? Is not the assumption of a white-hot liquid earth still quite as much on trial as any chronological inferences of the biologist or geologist?

It of course remains to be seen whether the alternative hypothesis of an earth grown up slowly in a cold state, or in some state less hot than that assumed in the address, would afford any relief from the

limitations of time urged upon us. At first thought it would perhaps seem that this alternative would but intensify the limitations. Since the argument for a short history is based on the degree to which the earth is cooled, an original cold state should but hasten the present status. But this neglects an essential factor. The question really hinges on the proportion of potential energy convertible into heat which remained within the earth when full grown. There is no great difference between the alternative hypotheses so far as the amount of sensible heat at the beginning of the habitable stage is concerned; for, on the one hand, the white-hot earth must have become relatively cool on the exterior before life could begin, and, on the other, it is necessary to assume a sufficiency of internal heat coming from impact and internal compression, or other changes, to produce the igneous and crystalline phenomena which the lowest rocks present. The superficial and subsuperficial temperatures in the two cases could not, therefore, have been widely different.

So far as the temperatures of the deep interior are concerned there is only recourse to hypothesis. It is probable that there would be a notable rise of temperature toward the center of the earth in either case. In a persistently liquid earth this high central temperature would be lost through convection, but if central crystallization took place at an early stage through pressure much of the high central heat might be retained. In a meteor-built earth, solid from the beginning, very much less convectional loss would be suffered, and the central temperature would probably correspond somewhat closely to the density. The probabilities, therefore, seem somewhat to favor a higher thermal gradient toward the center in the case of the solid meteor-built earth.

But if we turn to the consideration of potential energy there is a notable difference between the two hypothetical earths. In the liquid earth the material must be presumed to have arranged itself according to its specific gravity and, therefore, to have adopted a nearly complete adjustment to gravitative demands; in other words, to have exhausted, as nearly as possible, its potential energy—i. e., its "energy of position." On the other hand, in an earth built up by the accretion of meteorites without free readjustment there must have been initially a heterogeneous arrangement of the heavier and lighter material throughout the whole body of the earth, except only so far as the partial liquefaction and the very slow, plastic, viscous, and diffusive rearrangement of the material permitted an incipient adjustment to gravitative demands. A large amount of potential energy was therefore restrained, for the time being, from passing into sensible thermal energy. This potential energy thus restrained is supposed to have gradually become converted into heat as local liquefaction and viscous, molecular, and massive movements permitted the sinking of the heavier

material and the rise of the lighter material. This slow conversion of potential energy into sensible heat is thought to give to the slow-accretion earth a very distinct superiority over the hot-liquid earth when the combined sum of sensible and potential heat is considered. The theoretical difference is capable of approximate computation, and Mr. F. R. Moulton has kindly undertaken to make the computation in a simplified hypothetical case which may give some impression of the possible order of magnitude of this factor. For the purposes of the computation the earth was assumed to have been composed of 40 per cent of metal with a normal surface specific gravity of 7 and 60 per cent of rock with a normal surface specific gravity of 3. These combined would give an earth whose average specific gravity would be only 4.6. The real specific gravity (5.6) is supposed to have been obtained by compression which would amount to about 18 per cent. Very likely the proportion of metal is put too high and the effect of compression too low, but the purpose of the computation is only to show the theoretical possibilities of the case. The metal is supposed to have been originally scattered uniformly through the rock material in meteoric fashion, and to have gathered thence to the center, forcing the rock material outward so far as necessary. The heat produced Mr. Moulton found to be sufficient to raise the temperature of the whole earth (specific heat taken at 0.2) more than 3,000° C. The magnitude of this result is sufficient to require the careful consideration of the potential element unless the whole hypothesis can be set aside. It is large enough to cast the gravest doubt on any conclusion based on the rate of a supposed decline of internal temperature. Complete readjustment of the interior matter, however, is not postulated under the slow-accretion hypothesis. It is only assumed that a slow readjustment has been in progress throughout the geological ages and still is in progress, and that this has changed a certain amount of potential energy into sensible heat and that this heat has contributed to the maintenance of the internal temperature of the earth.

But there are in addition incidental factors which enter effectively into the case. The gravitative readjustment of the heterogeneous interior material is presumed to have taken place by the descent of the metallic and other heavier materials toward the center and the reciprocal ascent of lighter materials from the central region toward the surface, this being accomplished in various ways, the most declared of which has its superficial manifestation in volcanic action. Now, this process of vertical transfer, besides developing heat in proportion to the work done, as above indicated, also incidentally brings the hotter material of the interior toward the surface and thus increases the subsurficial temperature. It is a species of slow convection. This convection is in no radial sense different from that which is supposed to have taken place in the liquid earth, save that it was delayed so that

the heat is available within the life era of the earth, instead of being brought to the surface and dissipated in the prezoic hot stage, when it was a barrier to the existence of life instead of an aid.

Again, in the liquid earth there were the best imaginable conditions for the intermixture of the earth constituents and for the formation of such chemical and mineral combinations as best accorded with the high pressures of the interior. In the heterogeneous solid earth, on the other hand, such combinations were restrained and delayed and have been able to take place only slowly throughout the secular intermingling of the internal material. It, therefore, hypothetically follows that throughout geological ages, as the internal material was able slowly to readjust itself, new chemical and mineral combinations become possible. These combinations would be controlled by the high pressure in the interest of maximum density, and of hypothetically possible mineral combinations, only those would form which gave the higher density.[1] Thus a slow process of recrystallization in the interest of greater density would be in progress throughout the ages. This denser crystallization would set free heat. It would furthermore permit the shrinkage of the whole mass and consequent intensification of its self-gravitation, and this would in turn result in further development of heat. This large possible shrinkage meets the demands of geological phenomena at a point where the liquid earth has been felt to conspicuously fail. The losses of heat from the earth, as computed by Lord Kelvin and other authorities, and the skrinkage resulting therefrom have long been held to be quite incompetent to produce the observed inequalities. Their incompetence is now very generally admitted by careful students. Lord Kelvin also admits this by implication when he says (sec. 31, p. 706):

"If the shoaling of the lava ocean up to the surface had taken place everywhere at the same time, the whole surface of the consistent solid would be the dead level of the liquid lava all around, just before its depth became zero. On this supposition there seems no possibility that our present-day continents could have risen to their present heights, and that the surface of the solid in its other parts could have sunk down to their present ocean depths, during the twenty or twenty-five million years which may have passed since the consistentior status began, or during any time however long."

In addition to this recognized quantitative deficiency, the present writer has been led to question its qualitative adaptability. The phenomena of mountain wrinkling and of plateau formation, as well as the still greater phenomena of continental platforms and abysmal basins, seem to demand a more deep-seated agency than that which is supplied by superficial loss of heat. This proposition demands a more explicit statement than is appropriate to this place, but it must

[1] Prof. C. R. Van Hise has worked this out elaborately in manuscript not yet published.

be passed by with this mere allusion. It would seem obvious, however, that an earth of heterogeneous constitution, progressively reorganizing itself, would give larger possibilities of internal shrinkage, and that this shrinkage must be deep seated as well as superficial. In these two particulars it holds out the hope of furnishing an adequate explanation for the deformation of the earth where the hypothesis of a liquid earth seems thus far to have failed.

But the essential question here is the possibility of sustained internal temperature. It is urged that the heterogeneous, solid-built earth is superior to the liquid earth in the following particulars: (1) It retains a notable percentage of the original potential energy of the dispersed matter, while in the liquid earth this was converted into sensible heat and lost in prezoic times; (2) it retains the conditions for a slow convection of the interior material, bringing interior heat to the surface, a function which was exhausted by the liquid earth in the freer convection of its primitive molten state; (3) it retains larger possibilities of molecular rearrangement of the matter and of the formation of new minerals of superior density, whereas the liquid earth permitted this adjustment in the prezoic stages. In short, in at least these three important particulars, the slow-built meteoric earth delayed the exercise of thermal agencies until the life era, and gradually brought them into play when they were serviceable in the prolongation of the life history, whereas the liquid earth exhausted these possibilities at a time of excessive conversion of energy into heat and thus squandered its energies when they were not only of no service to the life history of the earth, but delayed its inauguration until their excesses were spent.

Let it not be supposed for a moment that I claim that the alternative hypothesis of a slow-grown earth is substantiated. It must yet pass the fiery ordeal of radical criticism at all points, but it is the logical sequence of the proposition that a swarm of meteorites revolving about the sun in independent individual orbits and having any probable form of dispersion would aggregate slowly rather than percipitately. If the astronomers and mathematicians can demonstrate that the aggregation must necessarily have been so rapid as to crowd the transformed energy of the impacts into a period much too limited to permit the radiation away of the larger part of the heat concurrently, the hypothesis will have to be set aside, and we shall be compelled to follow the deductions from the white-hot liquid earth, or find other alternatives.

But I think I do not err in assuming that mathematical computations, so far as they can approach a solution of the exceedingly complex problem, are at least quite as favorable to a slow as to a rapid aggregation. If this be so, the problem of internal temperature must be attacked on the lines of this hypothesis as well as those of the common hypothesis before any safe conclusion can be drawn from it respecting the age of the earth.

Another basis upon which the address urges the limitation of the earth's history is found in tidal friction. The limitations assigned on this basis are not, however, very restrictive. The argument is closed as follows:

"Taking into account all uncertainties, whether in respect to Adams's estimate of the ratio of frictional retardation of the earth's rotary speed, or to the conditions as to the rigidity of the earth once consolidated, we may safely conclude that the earth was certainly not solid five thousand million years ago, and was probably not solid one thousand million years ago." (P. 670).

And in a footnote it is added:

"It is probable that the date of consolidation is considerably more recent than one thousand million years ago."

The foundations of any argument involving the relations of the moon to the earth are very infirm. In the first place, no hypothesis respecting the moon's mode of origin, or of the time in the history of the earth when it became aggregated and came into effective possession of its tidal function, can claim even a remote approach to substantiation. There is not only no substantiated theory of the origin of the moon, but there can scarcely be said to be even a good working hypothesis, for the radical reason that the hypotheses offered will not work. George Darwin, who has probably studied the subject more assiduously and more profoundly than any other investigator, ancient or recent, strongly expresses the situation when he says, in his recent work on The Tides (p. 360):

"The origin and earliest history of the moon must always remain highly speculative, and it seems fruitless to formulate exact theories on the subject."

The annular theory of Laplace encounters in their maximum intensity the objections which arise from the application of the modern doctrine of molecular velocities. The gravitative control of an attenuated ring having the mass of the moon over its constituent material must have been exceedingly low, while the high temperature necessary to sustain the refractory material of the moon in a gaseous condition must have rendered the molecular velocities very high, so that no material except that of very high atomic weight and consequent low molecular velocity could be presumed to have been retained. But the specific gravity of the moon (3.4) seems a fatal objection to the assumption that it is composed wholly of material of very high atomic weight. Besides, it is difficult to understand how the high temperature of a ring of such attenuation could have been maintained during the time necessary for its concentration. This was less difficult when it was assumed, as formerly, that the temperature of the sun at that time was excessively high, as was also that of the earth. But modern inquiry seems decid-

edly opposed to the assumption of excessively high temperatures at that stage. On the contrary, it has recently been urged from different quarters that the early temperature of the sun's surface must have been much lower than at present, and this is also implied in certain statements of the address (p. 711, sec. 43). There are also grounds for grave question as to the high temperature of the earth, as has already been indicated. Under the revised forms of the nebular hypothesis there seems no substantial reason for supposing that if the matter of the moon was once distributed in a ring about the earth, it could maintain the gaseous condition throughout the stages of its condensation. The hypothesis therefore rests upon exceedingly doubtful premises, and upon exceedingly questionable deductions from these doubtful premises.

The fission hypothesis of George Darwin has recently replaced it in favor, but the above quotation implies that even its founder does not now rest much confidence in it. The objections to the theory are several and grave. In the first place, the theory of the fission of a celestial body by high rotation, as worked out independently by Darwin and Poincaré, requires that the separated bodies should not be very greatly different in mass, i. e., the smaller body should not be less than one-third the mass of the larger. But the mass of the moon is but one-eightieth of that of the earth, and hence it lies far outside the computed limits of applicability of the fission process.

Another difficulty lies in the effect of tidal strain itself. George Darwin, in his recent work on The Tides (p. 259), assigns 11,000 miles from the center of the earth as Roche's limit. This leaves a tract of 7,000 miles above the terrestrial surface within which the earth's tidal force would be so great as to tear the moon to fragments, and, perhaps, scatter these into the form of a ring. The rings of Saturn are supposed to illustrate this form of intense tidal action. The escape of the moon, even presuming it to have been separated from the earth, would, therefore, have been jeopardized by its transformation into a meteoroidal ring or swarm. If the fragments, after having been torn apart, were still sufficiently affected by a minute tide to be carried away from the earth in a slow spiral, the time occupied in passing outward beyond Roche's limit must have been protracted; and, after their escape from it into a zone where conditions not hostile to aggregation might, perhaps, have been afforded, there must probably have been another protracted period before the aggregation of the moon would have been sufficiently advanced to give it appreciable tidal effect upon the earth. It remains, therefore, to be determined, if this hypothesis is followed, at what stage in the evolution of the moon it was sufficiently concentrated to assume effective tidal functions. This is a question also applicable to the aggregation of the moon under the Laplacean hypothesis, if it be modified so as to conform to the demands of mod-

ern scientific probability. It also applies to any hypothesis which postulates aggregation from a dispersed condition. In any case, it seems necessary to determine when the moon became full grown before it is possible to assign a positive date for the commencement of effective tidal action. It would appear that such action might be developed gradually, as the material of the moon became aggregated. During such gradual assumption of the tidal function the reaction between the moon and the earth must have been of a feeble sort, and a recomputation of its amount, based on a series of hypotheses which shall cover the whole ground of legitimate speculation, would seem necessary before any satisfactory conclusions can be reached.

It may be urged that the computations of George Darwin, following, in backward steps, by the masterly application of mathematical analysis, the stages of the earth-moon relationship, give a firmer ground for conclusions. In a qualified degree this must be conceded. But it is to be remarked, in the first place, that the mathematics becomes indecisive before the origin of the moon is reached, which may signify that this is not the true line of approach to the origin of the moon, or that there is some error or defect in the assumptions. It would seem to be obvious, however, that if the tidal function was the result of a slow aggregation which began at an indeterminate stage in the earth's existence the numerical results of a computation based on a full-grown moon may need radical revision.

Furthermore, the agencies which are assumed to have accelerated the rotation of the earth in its earlier history must not be neglected. If they may safely be assumed to have been competent to give the earth a rotary speed sufficient to detach from itself the matter of the moon, as is postulated in the Laplacean and the fission hypotheses in common, the same agencies, if more evenly distributed in time, might prolong the period of acceleration so that it should be coincident with that of tidal retardation and offset it in any degree that falls within the legitimate limits of assumption. We encounter here again, in another form, a deduction from the assumption of a very rapid concentration of the matter ingathered to form the earth and moon, and the consequent exhaustion of its energy in an early stage. If, however, the concentration were less rapid and less complete in the early history of the earth, as is postulated by the accretion theory, as herein entertained, acceleration might be far less advanced in the earliest stages and be greater in the later stages. Hence, the retarding effects of tidal friction may have been more effectually antagonized by the shrinkage of the earth during the progress of geological history. Mr. Moulton has computed the effects of the internal change of metal and rock material, assumed in a hypothetical case on a previous page, on the speed of rotation of the earth, and found that it would accelerate the then current rate, whatever it was, about one-fifth. If, therefore,

the delayed central concentration left some notable part of the acceleration to be gained during the period of geological history, and if, at the same time, a slow aggregation of the moon delayed its effectual tidal influence upon the earth and the reciprocal influence of the earth upon it, the whole history may be notably affected in the direction at once of less maximum speed and of less retardation; i. e., of more near approach to uniformity.

If we turn to the geological data that bear on the question of former high rotation and subsequent retardation, we find ample support for profound skepticism regarding the applicability of the tidal argument. As pointed out by Lord Kelvin, if the rotation of the earth were once notably greater than at present it should have resulted in an oblateness of the spheroid such that the equatorial regions would now be all dry land, unless the body of the earth were deformed to correspond to the slackening rotation in an almost perfect manner. But there is not the slightest evidence in the configuration of the earth of such an equatorial land tract. The equatorial belt is notably oceanic rather than otherwise. Reciprocally, there should have been, with the gradual slackening of the earth's rotation, an accumulation of the oceanic waters about the poles, but there is no geological evidence of such an accumulation in any appreciable degree. In the arctic regions, as exemplified in Greenland, Spitzbergen, and the arctic islands of America, there are ancient shallow-water deposits, which lie both above and below the present oceanic level. These deposits range throughout the Paleozoic and represent in some less degree both the Mesozoic and Cenozoic eras. The nature of these shallow-water deposits is such that they can not have been formed at great depths below the oceanic surface, so that, with the allowance of a few hundred feet, it is possible to locate the ancient horizons relative to the crust of the earth at most or all of these periods. From these it may be inferred with great confidence that the ancient ocean surface in the arctic regions was in numerous stages of Paleozoic, Mesozoic, and Cenozoic eras not notably different from that of to-day. The facts even justify the seemingly extravagant statement that at several stages in geological history, early and late, the surface of the ancient ocean did not vary a foot from that of the present, since it must have passed both above and below the present horizon repeatedly during the earth's history. Geological evidence, therefore, interpreted on its own legitimate basis, seems to lend no appreciable support to any theory that postulates a high speed of rotation for the early earth or a low speed of rotation for the present earth, unless that hypothesis is correlated with the assumption of an almost perfect adjustability of the form of the earth to the changing rotation, in which case the argument of Lord Kelvin set forth on page 670 (Science, May 12) stands confessedly for naught.

If we postulate a slow accretion of the earth and of the moon alike,

the whole subject of the former speed of rotation of the earth and the relations of the earth to the moon take on a new aspect and invite investigation along the lines of new working hypotheses. Can it be shown that it is absolutely necessary that the aggregating meteoroids gave to the earth an exceedingly high rotation at the outset? Is not this assumption of high rotation merely an offspring of the nebular hypothesis? If the moon were aggregated slowly and came into tidal functions at a late stage, and at a distance from the earth's center quite unknown, may not all its relations to the earth have developed on much more conservative lines than those worked out by Darwin and at the same time preserve those apparently significant relations to the movements of the two bodies to which Darwin has so strongly appealed in support of his hypothesis of the history of the two bodies? In other words, without challenging the validity of Darwin's most beautiful investigation in the essentials of its method, may not a change in the premises deducible from an equally legitimate hypothesis of the original condition of the two bodies lead to results in equally satisfactory accord with the existing relations of the two bodies?

At any rate, as remarked at the outset, the time limits assigned on tidal grounds are not very restrictive, even on the assumptions made, and when they shall be worked out on revised data in accord with the newer hypotheses they may, perhaps, even be found to favor the longevity of the earth and become one of the arguments in support of it.

II.

A third line of argument relative to the habitable era of the earth is drawn from the theoretical age of the sun. After stating the probability that, if sunlight was ready, the earth was ready both for vegetable and animal life within a century, or at least a few centuries, after the consolidation of the earth's surface, Lord Kelvin inquires whether the sun was ready, and replies:[1]

"The well-founded dynamical theory of the sun's heat, carefully worked out and discussed by Helmholtz, Newcomb, and myself, says no if the consolidation of the earth took place as long [ago] as fifty million years; the solid earth must in that case have waited twenty or fifty [thirty?] million years for the sun to be anything nearly as warm as he is at present. If the consolidation of the earth was finished twenty or twenty-five million years ago, the sun was probably ready, though probably not then quite so warm as at present, yet warm enough to support some kind of vegetable and animal life on the earth."

Here is an unqualified assumption of the completeness of the Helmholtzian theory of the sun's heat and of the correctness of deductions drawn from it in relation to the past life of the sun. There is the

[1] Science, May 19, 1899, p. 711.

further assumption, by implication, that no other essential factors entered into the problem. Are these assumptions beyond legitimate question? In the first place, without questioning its correctness, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a complete theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly, no careful chemist would affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position, and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. The Helmholtzian theory takes no cognizance of latent and occluded energies, of an atomic or ultra-atomic nature. A ton of ice and a ton of water at a like distance from the center of the system are accounted equivalents, though they differ notably in the total sum of their energies. The familiar latent and chemical energies are, to be sure, negligible quantities compared with the enormous resources that reside in gravitation. But is it quite safe to assume that this is true of the unknown energies wrapped up in the internal constitution of the atoms? Are we quite sure we have yet probed the bottom of the sources of energy and are able to measure even roughly its sum total?

There are some things hereabouts in the instruction we receive that puzzle us with our geological limitations:

1. We are taught that there is a certain critical temperature for every substance, above which it takes the gaseous form and no amount of pressure can reduce it to the liquid or solid state.

2. We are taught that gases are compressible to an indefinite extent provided their temperatures be above the critical point.

3. We are told the temperature of the interior of the sun is probably above the critical temperature of any known substance, and hence that all the material of the interior of the sun is probably gaseous.

4. We are taught that so long as the substances of the sun remain in the gaseous condition the temperature of the sun must rise from increased self-compression. It can not, therefore, fall to the critical temperature of the component substances, and must, therefore, continue in the gaseous state and grow hotter and hotter.

5. We are taught that gravity varies inversely as the square of the

distance. As the distance between any two particles is halved, their mutual attraction is raised fourfold. Perpetual halving would cause the attraction to mount up toward infinity.

In the sun, then, there seems to be this interesting combination: (1) A gaseous mass already above the critical temperature growing hotter and hotter by self-compression and bound to grow hotter and hotter so long as it remains a gas; and it is bound to remain a gas until it falls below the critical temperature, which it can not do while it continues to grow hotter; (2) a gravity that increases fourfold with every halving of distance and that is bound to increase so long as concentration continues, and concentration must continue while the substance is a gas and the gravitative pressure increases.

What is the logical outcome of this kind of logic and this sort of a combination? A geologist begins to grow dizzy contemplating such thermal possibilities. Why should not atoms, atomecules, and whatever else lies below, one after another, have their energies squeezed out of them, and the outer regions be heated and lighted for an unknowable period at their expense?

There was a time when the chemical theory of the sun's heat was fairly satisfactory to the scientists of the day, but its inadequacy appeared in time. There followed a period in which the meteoroidal theory of the sun's origin was deemed adequate, but its defects soon became apparent. There has followed the contractional theory, the validity of which is perhaps not less questioned now than was the validity of the chemical and meteoroidal hypotheses in their day of acceptance, but, judging from the past, it may easily appear in the future that the Helmholtzian theory is inadequate in some measure not unlike its predecessors.

But assuming, as we are wont to do, that the limits of our present knowledge are a definition of the facts, has the evolution of the sun been worked out with such definiteness and precision as to give a determinate and specific history of its thermal stages from beginning to end? It is one thing to tell us, on the basis of the contractional theory, that the total amount of thermal energy originally potential in the system is only equal to so many million times the present annual output, but it is quite a different thing to give a specific statement of the actual time occupied by the sun in the evolution and discharge of this amount of heat and to define its successive stages. It is with this actual history that we are specially concerned. The distribution of the computed heat in time may have been such hypothetically as to shorten the period of its expenditure not simply to twenty or twenty-five millions of years, as indicated by Lord Kelvin, but to four or six millions of years, as deduced by Ritter.[1] On the other hand, the dealing out of this amount

[1] Astrophysical Journal, December, 1898; Journal of Geology, p. 93, No. 1, Vol. VII, 1899.

of heat may hypothetically have occupied a period many times the twenty or twenty-five million years postulated. It seems altogether necessary to determine specifically the distribution of the heat in time before any approach to a satisfactory application to geological history can be made. The period of twenty or twenty-five million years named can have little moral guiding force until this problem is solved. But the literature of the subject shows an almost complete neglect of this consideration. While certain of the physicists and astronomers have been instructing us "e superiore loco," they seem, with very rare exceptions, to have overlooked this vital factor in the case. Even in computing the sum total of heat they have, for the most part, heretofore neglected the central condensation of the sun and in their computations have substituted a convenient homogeneity. This is recognized in a more recent number of Science (May 26), in the article by Dr. See, in which he offers a correction which involves an extension of the previously assigned output (eighteen million times the present annual radiation) to about thirty-two million times the annual radiation. But even in making this correction he neglects to consider the distribution of this heat in time, and leaves upon the reader the impression that the life history of the earth was limited to thirty-two million years. Assuming the correctness of his computations, the past thermal discharge of the sun is merely limited to thirty-two million times the present annual expenditure. For aught that appears to the contrary, the actual output of this heat may have been spread over any assignable number of years. It is obvious, upon consideration, that a certain distribution of this past heat would favor longevity of life upon the earth, provided it could exist with a more limited heat supply than the sun is now yielding. On the other hand, it is equally evident that if the supply be distributed in certain other ways, either in the nature of excessive prolongation or of excessive concentration, the life era will be shortened. Doubtless the admonitory physicists have assumed that it was sufficient for the gross purposes of restraining geologists within due limits to determine the total amount of heat without assiduously considering the actual facts relative to its distribution, but some of us are unwilling to accept this loose method of dealing with the problem, since there are resources of application of which our physical friends have perhaps not taken cognizance. For example:

1. If at a certain stage in the evolution of the sun it occupied essentially all the space within the earth's orbit, and was giving forth one-half as much heat per year as now, it would possibly have sufficed for the needs of life upon the earth essentially as well as at present, without the assumption of any change in the constitution of the earth or of its atmosphere. For, on this supposition, approximately one-half of the space into which the earth radiated its heat would be blanketed by the sun, and the heat thrown forth from the earth would be measurably

caught and returned, and hence the loss of heat by radiation from the surface of the earth would have been reduced.

2. If at the same time we suppose that the material now concentrated in the outer planets was dispersed in a broad nebulous or meteoric belt mantling the heavens on the opposite side, another means would be provided by which some portion of the heat radiated away would be caught and returned to the earth, and a further small reduction in the original receipt of heat from the sun may be made consistently with the existence of life. This outer belt would be very tenuous, and its effects correspondingly meager, but it is a factor to be considered in a complete set of assumptions.

3. If, in addition to this, we make the consistent assumption that many other bodies of the heavens which are now concentrated into suns or into dark bodies were then in a more dispersed nebulous or meteoroidal condition, the general space of the stellar universe would be partially mantled, and there would be less free scope for the escape of the heat, solar and terrestrial alike, which is now freely lost through the open regions of space. It may be conceived that there was a common blanketing of the heavens by the dispersal of its now concentrated matter. This conception is the logical companion of the supposed dispersal of the solar matter. If the volume of matter in the stellar universe could be supposed to be sufficient, it might be so distributed hypothetically as to mantle the whole heavens and largely prevent the escape of central heat outward, just as the central heat of the more concentrated bodies is conserved at the present time. Under this conception the history of the stellar universe may be characterized as a progressive clearing up of nebulosities and meteoroidal dispersions and the concentration of its matter about certain points, leaving between vast open spaces through which heat is now radiated away with a facility unrealized in the earlier stages. The quantitative value of such a suggestion must be left to the determination of astronomers, who have the best data for forming a conjecture as to the ratio of matter to space in the stellar universe and as to the possibilities of its dispersion at a period coincident with the earlier stages of the earth's history.

4. A modification of the conditions assumed in the foregoing paragraphs may be postulated in which the earth is regarded as having made its early growth within the primordial meteoric aggregate, perhaps a great flattened meteoric spheroid, which initially extended beyond Neptune in nebular fashion and whose present attenuated representative may perhaps be found in the zodiacal light. In this case the thermal environment of the early earth was that furnished by the interior of the spheroid, though far out from the center. The conditions only became external gradually as the growth of the planets exhausted the peripheral portion of the meteoric spheroid.

5. The foregoing hypotheses, which do not seem to be so completely out of accord with the possibilities of the case as to be inadmissible tentatively in the absence of a positive solution of the early terrestrial environment, are concerned with the external relations of the earth. If we turn to the earth itself it may be remarked that the nature of its atmosphere very radically conditions the amount of heat requisite for the support of life. Dr. Arrhenius has recently made an elaborate computation relative to the thermal influence of certain factors of the atmosphere, and has arrived at the conclusion that an increase of the atmospheric carbon dioxide to the amount of three or four times the present content would induce such a mild climate in the polar regions that magnolias might again flourish there as they did in Tertiary times. On the other hand, he concluded that a reduction of less than 50 per cent would induce conditions analogous to those of the glacial period of Pleistocene times. The vast quantities of carbon dioxide represented in the carbonates and carbonaceous deposits of the earth's crust imply great possibilities of change in the constitution of the atmosphere of the earth in respect to this most critical element.

6. But there are more radical considerations that relate to the early thermal history of the earth. To be sure, if we are forced to adopt the hypothesis of a white-hot liquid earth, with all its extravagant expenditures of energy in the early youth of the earth, we can take no advantage of these possible resources, but under the supposition that the meteorites gathered in with measurable deliberation it is theoretically possible to find conditions for a long maintenance of life on the earth, with little or no regard to the amount of heat which the early sun sent to it. In the earliest stages of the aggregation of the earth under this hypothesis, while it was yet small, it can scarcely be supposed to have been habitable, because its mass was not sufficient to control the requisite atmospheric gases; but when it had grown to the size of Mars—that is, to a size representing about one-tenth of its present aggregation, or, to be safe, when it had grown to twice the size of Mars, or about one-fifth of its present mass—it would have been able to control the atmospheric gases and water, and, so far as these essential items are concerned, it would have presented conditions fitted for the presence of life. At this stage the larger portion, four-fifths by assumption, of the matter of the earth would yet be in the meteoroidal form and doubtless more or less closely associated with the growing nucleus. If the infalling of this four-fifths of the material of the earth were duly timed, so as to be neither too fast nor too slow, it would give by its impact upon the atmosphere of the earth a sufficiency both of heat and of light to maintain life upon the surface of the earth. The plunging down of these meteorites upon the surface might be more or less destructive to the life, but only proportionately more so than the fall of meteorites to-day. It would not be necessarily fatal to life,

especially oceanic life; indeed, the strokes of the meteorites might not be more inimical to the perpetuity of any given form of life then than are the attacks of its numerous enemies to-day. It was only another form of jeopardy. The latitude as to variation of rate of infall would be rather large. The infall must not have been so rapid as to have given a universal surface heat above 100° C. The life of hot springs crowds close upon this upper limit, as Lord Kelvin has indicated. The infall must not have been so slow as to have permitted the surface heat to fall universally below 0° C., making allowance for other sources. These other sources might have permitted the meteoric supply to fall considerably below the quantity represented by a surface temperature of 0° C. Between this indeterminable low point and a supply equivalent to 100° C., similarly qualified, there is a quite wide range. Those who have insisted upon the precipitate infalling of meteorites at such a rate as to reduce the earth to a nebulous condition will probably not feel entitled to doubt the adequacy of this source of light and heat. They can only question the possibility of the meteorites falling in slowly enough to permit the coincident presence of life on the earth.

This hypothesis starts life at a period when the earth was one-fifth grown and prolongs it throughout the rather slow gathering in of the last four-fifths of the earth's mass, and hence gives to the earth a long era of autogenic life conditions.

Now, if a hypothesis relative to the early constitution and the growth of the rest of the solar system concordant with this be entertained—that is, a constitution of a predominantly meteoroidal rather than a gaseous condition, and of a slow rather than a precipitate aggregation—it will, perhaps, appear that the output of heat by the sun in the stages concurrent with this autogenic life period of the earth may have been small. The autogenic thermal era of the earth may thus have corresponded to a period of slight thermal loss by the sun.

As time went on the ingathering of the terrestrial meteorites gradually became more and more distant from one another (since the scattered material was progressively exhausted by previous infalls), while the central or solar aggregation was yet only in its early stages and was gradually increasing in heat. If this increase was in a ratio somewhat proportionate to the decline of the autogenic heat of the earth, an equalizing compensation might result, and the earth gradually pass from the relatively independent autogenic thermal stage to the dependent solar stage which has continued to the present. Thus, by the prolonged coincidence of increase on the one side with decrease on the other, the life history of the earth may have been transferred from meteoroidal to solar dependence without such a radical disruption of continuity as to have been generally destructive.

This speculation may seem at first thought to be far-fetched, and to

be poised on a ticklish combination of conditions, and it may, indeed, prove, when critically studied, to be really so, but yet it is submitted that it follows along coherent lines connected ultimately with the fundamental proposition that dispersed meteoroidal matter might gather in slowly rather than precipitately. On this point hangs all the law and the prophets.

If astronomers, physicists, and mathematicians will jointly attack the formational history of the solar system stage by stage, following each stage out into details of time and rate, and taking full cognizance of all the alternatives that arise at each stage, it will then be possible, perhaps, to decide whether the conditions of the early earth were such as to require a large or a small amount of heat from the sun for the sustenance of life, and whether the sun was wasting heat prodigally in those days or conserving it for later expenditure. The present measure of the earth's needs may be no measure of its early needs. The sun's present expenditure may be no measure of its early expenditure.

In view of all these considerations, I again beg to inquire whether there is at present a solid basis for any "sure assumption" with reference to the earth's early thermal conditions, either internal or external, of such a determinate nature as to place any strict limitations upon the duration of life.

The latter part of the address is concerned with novel suggestions regarding the behavior of the supposed liquid surface of the earth in the stages just preceding its final solidification, involving a theory of the formation of the primitive surface rocks and of the original continents and ocean basins. The discussion of this I must leave to the petrologists, merely venturing the hint that they may find some occasion to reconstruct current petrological doctrines if they are to be brought into consonance with the new views offered.

The point of greatest general interest in this part of the address is the sharp statement of opinion that, if the original lava ocean had solidified equably in all its parts and produced a dead-level surface all around the globe, there seems no possibility that our present continents could have arisen to their present heights, or the ocean basins have sunk to their present depths, during twenty or twenty-five million years, or during any time, however long. (Exact words previously quoted.) Lord Kelvin adds:

> Rejecting the extremely improbable hypothesis that the continents were built up of meteoric matter tossed from without upon the already solidified earth, we have no other possible alternative than that they are due to heterogeneousness in different parts of the liquid whic constituted the earth before its solidification.

This is as strong an assertion of the necessity of assuming crusta' and subcrustal heterogenity as any advocate of a slow-accretion earth could wish. If the word "liquid" and what follows be stricken out,

and the words "meteoroidal aggregate" be substituted in the sentence quoted, it will be a rather too strong statement of the alternative explanation which springs obviously from the meteorological hypothesis herein urged. It is not easy to see how such heterogenity as is required to account for the continents and ocean basins could arise from a white-hot liquid-surfaced earth descended from a gaseous earth. To those who do not follow the petrological conceptions of the address, but who conceive the hypothetical lava ocean to have been one great solution, stirred by convectional and other currents and depositing crystals as supersaturation arose from change of temperature or from change in the solution itself, there seems not much more reason to suppose that its deposits would have been localized persistently on the sites of the present continents than to suppose that the present enveloping solution—the ocean—if duly concentrated, would localize in a similar way the crystals which it would throw down. But this must be left to the petrologists. I can not, however, express too strongly my appreciation of the value of Lord Kelvin's stalwart opinion respecting the incompetency of the thermal theory of crustal deformation, since this carries with itself, more remotely and occultly (*pace* Kelvin), an implication of like weakness in the theory of the white-hot earth itself.

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AN ESTIMATE OF THE GEOLOGICAL AGE OF THE EARTH.[1]

By J. Joly, M. A., D. Sc., F. R. S.

INTRODUCTION.

The extremes to which, in the time of Lyell, the principles of Uniformitarianism were carried did much to injure a doctrine which, properly restricted, defines the only scientific attitude open to the geologist in dealing with the past and the future.

Rightly defined, this doctrine is no other than that held and lived up to by every scientific man. It asserts that we may justly prolong into the past and future the activities of to-day till sufficient reason be shown to interrupt them by catastrophe or change. The onus of examining into the "sufficient reason" rests with the disciple of uniformitarianism. It is, in fact, his business to seek and define the limitations in time of the actions he is familiar with.

He differs from the catastrophist or convulsionist in the stringency with which he defines and examines the reasons for postulating such changes and catastrophies, and, it may be said, the reluctance with which he resorts to such modes of explanation. If existing operations, when extended into the past, are not in discord with probabilities, he prefers the existing operations to alternative ones, even if the latter in themselves involve nothing improbable.

The assumption of uniformity of present activities enters into many attempts to estimate the age of the earth, dated from the beginning of those changes which may be referred to the action of water upon the face of an igneous lithosphere. Such attempts, broadly speaking, deal with the fact that a lithosphere, cooling from fusion and then subjected to aqueous solution, is molecularly unstable in presence of the latter agency; nor can final stability be attained till all molecular ties are remade in the common solvent and retained under the conditions of their formation—in other words, till complete solution has been effected and all is immersed in the common solvent. It is possible that so long

[1] Read May 17, 1899, by J. Joly, honorary secretary of the Royal Dublin Society, professor of geology and mineralogy in the University of Dublin. Reprinted from Scientific Transactions of the Royal Dublin Society. New Series, Vol. VII, Part III, 1899.

as subsidence and elevation are possible, tides exist, and evaporation progresses, this state can not be attained. To-day we find ourselves in the midst of these cycles. We perceive soils formed under subaerial actions, partly from former igneous rocks, partly from former sediments, decaying year by year under the solvent actions to which they are exposed, and then carried away under new molecular arrangements to the common reservoir of the ocean. There further changes of molecular bonding arise, and part become diffused in solution—increasing the density of the ocean—while part form precipitates under the actions of the living or dead molecular forces existing in the new conditions in which they are placed. Thus the land would be melted down into the sea if the disturbed gravitational balance—as well as other causes—did not constantly upraise it from the water, maintaining the cycle of operations.

That these actions have progressed, broadly speaking, at a uniform rate since the earliest recognizable sediments were laid down is a tenet which has not seriously been impugned. It has been claimed that the rate of removal of the subaerial land surface—by solution and transportation—has been, on the whole, uniform. Of course probabilities only can be advanced in support of such views, which must probably forever remain in the domain of speculative geology.

In the method of approaching the question of the age of the earth advanced in this paper, the foregoing tenet requires only acceptation in part—that part of it which refers to the removal of the land surface by solution. It has to be accepted as a preliminary step that this, on the whole, has been constant. Herein are involved a constancy, within certain fairly wide limits, of rainfall over the land areas; a constancy, within fairly wide limits (which can roughly be defined), of the exposed land area, and a constancy in the nature and rate of solvent actions going on over the land surfaces. The grounds on which this amount of uniformity is accepted are given in this paper. One other tenet must be accepted, that the primeval ocean—that formed on first condensation of the water upon the land—did not contain the amount of dissolved sodium now entering so largely into its constitution. The grounds upon which this is claimed are also dealt with further on.

How can these data be used to determine what may be termed the epigene age of the earth? In the sea or in its deposits those elements are recognizable which enter also into the constituents of the solid part of the earth's crust. In the rivers these elements are also recognizable as being continually poured into the ocean. Very accurate estimates of the quantities of these elements in the ocean exist. The dissolvent contents of many of the great rivers of the earth and the mean composition and mean volume of the entire river discharge have been estimated.

Now, if any of the elements entering the ocean is not again withdrawn, but is, in a word, "trapped" therein, reappears as no exten-

sive marine deposit, and is not laid down sensibly upon its floor, and if the amount of uniformity already defined is accepted, evidently in the rate of annual accretion by the ocean, from the rivers, of this substance and the amount of it now in the ocean, the whole period since the beginning of its supply can be estimated.

Such an element is sodium. We take for this calculation the element alone, thus avoiding the obscure question of its ionisation, which does not concern the issue. The quantity of sodium now in the sea, and the annual rate of its supply by the rivers, lead, it will be seen, to the deduction that the age of the earth is 99×10^6 years. Certain deductions from this are, it will be shown, warranted, so that the final result of this paper will be to show that the probable age is about 89×10^6 years. Also, that this is probably a major limit, and that considerable departure from uniformity of activities could hardly amend it to less than 80×10^6 years.[1]

In the claims to uniformity here involved, much is avoided that is most uncertain in those methods of calculation which repose upon a knowledge of the volume of sediments removed from the land and deduced from the geological record. Not only in the latter methods is the rate of denudation of the land surface difficult or impossible to determine with any degree of accuracy, owing to the difficulties attending determinations of the amount of sediment discharged by rivers, but the bulk of the material which has been acted upon must, to a considerable extent, be matter of speculation; for even when the best is done to determine the true thickness of the sedimentary deposits, what is missing at the unconformabilities is, in many cases, unknown.[2] The method used in this present estimate, on the other hand, involves two quantities, the amounts of which are ascertained with an accuracy depending only on the number of our observations—the dissolved matter in the sea (which is almost homogeneous in composition) and the average dissolved matter in the rivers of the world. That the information regarding the latter quantity available for the present calculation is not final is very probable. A complete knowledge of the dissolved solids of river discharges must involve analyses of all the principal river waters, and these chemical investigations must be combined with volumetric measurements of the discharge. In some cases such observations should be seasonal. Failing such complete knowledge, the data used in this paper may be said to afford an approximation to the nature and amount of river discharge. That no more than an approxi-

[1] In Mr. T. Mellard Reade's calculation of a minor limit of the earth's age from the amount of calcium sulphate in the sea (Chemical Denudation in Relation to Geological Time, Daniel Dogue: London, 1879), the substance chosen does not possess the requisite qualifications to enter into such a calculation as is advanced here. It may be observed, as not altogether immaterial, that the principal calculations of this paper were made independently of Mr. T. M. Reade's interesting views.

[2] A good account of the difficulties involved appears in Wallace's Island Life, Chap. X.

mate uniformity can be claimed for these factors over the past is doubtless true; but this does not eliminate the necessity of accurate knowledge where such is available in the application of the method, for the possibility of errors occurring of the same sign must always be borne in mind.

Although the uncertainty attending the estimates of the volume of sedimentary rocks involved in the recognized geological method of prosecuting inquiry into the age of the earth must be admitted, it will be seen further on that the sodium contents of the sea considered in connection with the knowledge we possess of the chemical composition of the sedimentary and igneous rocks lends support to estimates that have been made of the total bulk of the sedimentaries. Here the two methods of inquiry into the geological age of the earth appear to mutually support one another.

This is involved in the following consideration, which, for the sake of clearness, may be outlined here. The mean composition of the siliceous sedimentary rocks can be arrived at approximately by taking the average of the many reliable analyses available of various classes of such rocks as are most abundant. For present purposes it is only necessary to consider the alkali content of these rocks. Furthermore, the mean alkali content of the principal igneous rocks can, in a similar manner, be investigated. The mean composition of these rocks has been estimated with more especial care by F. W. Clarke, of the United States Geological Survey, and the composition of the older crust of the earth in this way approximately determined. It is, in the first place, found that the alkali content of the latter is considerably in excess of that of the former. Accepting, now, an approximate estimate of the bulk of siliceous sedimentary rocks, and restoring to this the sodium now contained in the ocean, the sodium content of the original crust, or of the average of the eruptives, is obtained with a fair degree of approximation.

Here we observe in the sodium deficit of the detrital siliceous sediments the results of its gradual abstraction by the influences of denudation. There can surely be but this one legitimate explanation of the fact that the great bulk of the detrital sedimentaries is deficient in sodium by just that amount of this body as is contained in the ocean plus a relatively small allowance for the deposits of rock salt. It is to be observed that we can effect such a restoration in the case of no other elemental body dissolved in the sea. The amount present of the chemically related substance potassium will not fit the detrital sediments. It exhibits a deficiency. For obvious reasons the calcium and magne sium salts will also be deficient.[1]

[1]The reasons referred to are principally the continual abstraction and precipitation of these bodies from the sea, giving rise to limestones and dolomites, and the presence of calcium and magnesium in the ocean sediments.

An interesting fact, however, is in the case of the potassium revealed as the result of very simple calculation. The present potassium discharge of the rivers, if prolonged into the past, as the duration of this is determined by the sodium constituent, would have fed into the ocean just about the missing quantity of potassium. The rocks, in short, negative the view commonly urged that the discrepancy between the alkali ratios (sodium : potassium) of rivers and ocean indicates chemical differences in the river waters of the past. It is quite the other way. Any alteration of the alkali ratio arising from a change in the potash constituent in the river water of former ages will leave the record of the rocks, while correct for the soda, incorrect for the potash.

The legitimate deduction appears to be that the potash discharge of the rivers of the past is to be sought for in oceanic deposits and the sediments. This question is briefly considered in this paper.

One other important factor in the legitimacy of the conclusions arrived at in this paper may be referred to here. The assumption that the early waters of the ocean did not contain the sodium at present forming so large a part of its total solid contents is supported not only as a deduction of the facts just quoted, but by a consideration of the silicated state of the elements on a lithosphere cooling from fusion and the subsequent effects on such a magma or crust of any probable abundance of acids derived from the chlorine of the ocean, were this free to form hydrochloric acid. It is submitted that such a body of acid vapor and liquid would be neutralized by the various silicated bases, and divided in such proportion among these as would result in what is relatively a small quantity of sodium chloride brought into solution. Our knowledge of the relative abundance of the elements in the earth's lithosphere enables a very definite allowance for this primeval action to be effected.

The consideration of the question of the uniformity in the rate of denudation involves inferences based on the known deficiencies of rainfall in many parts of the earth's surface—the "rainless" regions. Where such exist, there will be elasticity as regards subsidence or upheaval and rate of denudation into the ocean. The first causes the inward retreat upon the land of the watershed defining the oceanic supply, the second its outward advance. But there is no reason to suppose the amount of supply will, on the whole, vary. There is such elasticity to-day to the extent of one-fifth the total land surface of the globe.

In the next place, the nature of the soils derived from rocks of very various origin enters into consideration. Our existing knowledge shows that there is remarkable uniformity in these, whether derived from igneous or sedimentary rocks. It is in the soils that solvent denudation is chiefly effected. The greater alkali content of the eruptives—leading to their more rapid yield of those substances on first breaking

down- -is probably compensated by their physical character, in many cases conferring greater durability upon them. These and other considerations lead to the view that there is no sufficient evidence to ascribe greater alkali content to the rivers of the past.

The origin of the interstratified beds of rock salt, the solvent denudation of the sea, and the order of magnitude to be ascribed to an allowance for the latter are briefly considered in the paper, as well as other questions which arise.

When all corrections are made and the requisite latitude of error taken into account, it would appear that the consideration of solvent denudation points to an age for the earth, dating from the settlement of water upon its surface, of between eighty and ninety millions of years.

I.—THE ESTIMATE OF GEOLOGICAL TIME.

On the basis that the ocean possesses an average depth of 2,000 fathoms and occupies eight-elevenths of the area of the globe, its total mass is calculated to be 1.322×10^{18} tons.[1] Its ingredients in solution are:

Chloride of sodium	77.758
Chloride of magnesium	10.878
Sulphate of magnesium	4.737
Sulphate of lime	3.600
Sulphate of potassium	2.465
Bromide of magnesium	0.217
Carbonate of calcium	0.345
	100.000

and the total salts are approximately 3.5 per cent of the mass of the whole.

On these data the absolute masses of the ingredients of the ocean are calculable:

NaCl	$35,990 \times 10^{12}$ tons.
$MgCl_2$	$5,034 \times 10^{12}$ tons.
$MgSO_4$	$2,192 \times 10^{12}$ tons.
$CaSO_4$	$1,666 \times 10^{12}$ tons.
K_2SO_4	$1,141 \times 10^{12}$ tons.
MgBr	100×10^{12} tons.
$CaCO_3$	160×10^{12} tons.
	$46,283 \times 10^{12}$ tons.

Of the sodium chloride 39.32 per cent is sodium. In the sea there is therefore a mass of sodium in solution amounting to $14,151 \times 10^{12}$ tons.

[1] Encyclopedia Britannica—Article, "Sea." The analyses are Dittmar's, from the reports of the *Challenger* expedition.

Sir John Murray,[1] as the result of the analyses of nineteen rivers—many of which are principal rivers of the world—arrives at the following estimate of the dissolved materials in tons per cubic mile of river water:

	Tons.
$CaCO_3$	326,710
$MgCO_3$	112,870
$CaSO_4$	34,361
$Ca_3P_2O_8$	2,913
Na_2SO_4	31,805
K_2SO_4	20,358
$NaNO_3$	26,800
NaCl	16,657
LiCl	2,462
NH_4Cl	1,030
SiO_2	74,577
Fe_2O_3	13,006
Al_2O_3	14,315
Mn_2O_3	5,703
Organic matter	79,020
	762,587

He further estimates that the total volume discharged by the rivers into the ocean is 6,524 cubic miles per annum.

Taking 32.40 per cent of the sodium sulphate, 27.06 per cent of the sodium nitrate, and 39.32 per cent of the sodium chloride as sodium, we obtain a total mass of sodium of 24,106 tons per cubic mile; and multiplying this number by the number of cubic miles of river water annually discharged into the ocean, we find that this amounts to 157,267,544 tons.

The quotient of $14{,}151 \times 10^{12}$ divided by $15{,}727 \times 10^{4}$ is very nearly 90×10^{6}.

From these data, then, the period of time required to supply its present amount of sodium to the ocean by rivers possessing the average approximate compositions of the existing rivers would be ninety millions of years.

The foregoing figures admit of amendment as the result of a more recent estimate of the volume of the ocean by Sir John Murray.[2] He estimates the volume as 323,800,000 cubic miles, very closely. Taking the weight of a cubic mile of sea water as 43×10^{8} tons, this affords a mass in tons of 1.392×10^{18}, or a mass 5.3 per cent, nearly, in excess of that previously assumed, which of course raises the figures obtained for geological time in years by a corresponding amount. Thus on this more carefully estimated basis the period of geological denudation becomes 94.8×10^{6} years, nearly.

But this number admits of still further amendment on another and perhaps more complete estimate of the oceanic area. Prof. Hermann

[1] Scottish Geographical Magazine, 1887, p. 76. [2] Ibid., 1888, p. 1.

Wagner[1] reconsiders Sir John Murray's estimate just quoted and arrives at the result that the whole land surface of the globe is 55,814,000 square miles and that the oceanic area bears to this the ratio of 2.54 to 1. On this estimate, and accepting Sir John Murray's estimate of the mean depth of the ocean (2,076 fathoms = 2.393 miles), the bulk of the ocean in cubic miles is 339,248,000. This gives a mass of 1.460×10^{18}, of which the sodium constitutes $15,627 \times 10^{12}$ tons, and the period of denudation arrived at is 99.4 millions of years.

This is probably the most accurate basis on which to obtain this quotient and will be accepted in what follows. The estimate will be modified to some extent on further considerations.

II.—THE ORIGINAL CONDITION OF THE OCEAN.

The existence of primitive high temperature conditions affecting the materials of which the earth is composed is inferred as the result of many observations and analogies. These need not be referred to here.

The globe, as we find it, possesses as its lithosphere siliceous and aluminous compounds, which are volatile only at temperatures probably much exceeding 2,000° C., and carbonates of the alkaline earths, which at a much lower temperature dissociate into a gaseous oxide and stable solid oxides. In the hydrosphere now enveloping a large part of the lithosphere we find a vast bulk of water, gaseous at all pressures above the temperature 370° C., and dissolved in it a quantity of a halogen salt sufficient in amount, as may be easily shown, to cover the entire globe to a depth of 112 feet if crystallized out into solid sodium chloride.

The effect of a temperature so elevated as 1,500° C. upon the materials of the earth's surface will, then, result as follows, according to our laboratory experiments:

The carbonic anhydride will, if previously formed, exist in a stable state. Free oxygen and hydrogen will represent the present ocean, water gas ceasing to be stable at normal pressures at temperatures somewhat over 1,200° C. The alkaline earths, the iron, and the alkalies will be silicated and exist as lime, magnesium, iron, sodium, and potash aluminum silicates in a state of fusion. Quartz melts below 1,500° C., and the largely preponderating number of silicates possess melting points ranging between 900° and 1,500° C.[2]

The chlorine, now combined, as assumed, with the sodium in the sea, will have entered most probably into combination with the hydrogen and exist as hydrochloric-acid gas. This compound is stable up to 1,700° C., nearly.[3]

[1] Scottish Geological Magazine, 1895, p. 185.

[2] The Melting Points of Minerals, by J. Joly, Proc. R. I. A., 1891, II, p. 44.

[3] See the investigation of the stability of the compounds referred to by Carl Langer and Victor Meyer, Pyrochemische Untersuchungen, Braunschweig, 1885.

That the sodium chloride could not exist as such is shown in the everyday operations of the pottery kiln, whereby common salt is decomposed in presence of water vapor, the sodium uniting with the oxygen of the water vapor and the heated earthenware to form sodium aluminum silicate, and the chlorine with the hydrogen of the water vapor to form hydrochloric acid. The glaze produced in this way on the earthenware is highly insoluble.

Under this condition of temperature a gaseous pressure of not less than 300 atmospheres—probably between 300 and 400—must have obtained, due to the oxygen, hydrogen, carbonic anhydride, and hydro. chloric acid. This pressure can not, however, be supposed to have influenced the chemical combinations occurring in the liquid silicated magma of the earth's surface.

If we transfer our attention to a later epoch, when a temperature of say 1,000° C. was attained, we observe that water vapor would be stable, and a crust would be forming upon the surface of the earth. We find now events progressing in this early solid crust which have already been indicated by Lord Kelvin.[1] The break up and submergence of the denser solid constituting the crust would certainly lead to a considerable intermingling of layers probably previously differentiated by specific gravity acting on a mass which was hardly likely to be molecularly homogeneous throughout. We must note, however, that this action can only have extended to comparatively shallow depths, as such descending fragments would soon find themselves buoyed up and re-fused by the denser magma beneath.

Observations on the behavior of silicates at high temperature show that these bodies are stable for the most part, certainly up to 1,500° C., but upon complete fusion readily yield up included or combined water. Still, under the conditions of pressure and temperature obtaining at the surface of the earth at the period we refer to, it is probable that much volatile matter was held in solution in the melted magma and ultimately trapped in the solid crust. How far this was a glass, or how far crystalline differentiation had progressed, does not much concern the present issues, and is, in any case, difficult or impossible to determine.

We now transfer our attention to yet another period of the earth's early history—an eventful period, when the temperature near or at the surface had fallen to the critical temperature of water, 370° C. At this temperature a pressure of 196 atmospheres would suffice to liquefy it. The pressure was very probably much above this, even at points high up in the atmosphere.

When this critical temperature was attained at such a point in the

[1] "On the secular cooling of the earth," and "On the rigidity of the earth." Mathematical and Physical Papers, Vol. III. See also Green's Physical Geology, 1882, p. 655.

atmosphere as to be attended by pressure conditions exceeding the critical pressure an instant change of state occurred. The water resulting—almost still a vapor, but possessing a surface, although a highly energetic one—probably floated in the equally dense vapor, or sinking into hotter layers beneath immediately resumed its vaporous state. Its condition was, in fact, highly unstable as regards upward or downward motion;[1] finally the temperature sank till water established itself upon the surface, here and there over hotter areas doubtless flashing into vapor, but gradually gaining a resting place upon the surface.[2] For a long period the fall in pressure attending its own condensation must have maintained it in a state of ebullition.

Effects were produced at this stage which may well claim here a moment's consideration.

Sensible shrinkage due to secular cooling, and the great earth-folding which has since wrinkled the earth's surface, had not yet taken place. Let us suppose a depression anywhere upon the comparatively uniform surface receiving the precipitated water. Over this area the pressure is increased, elsewhere it is reduced. The effect of this is to cause, on the one hand, a further depression of the early sea bottom and to establish a drainage into it, and on the other to facilitate the expansion and extrusion of any heated volatile matter held in solution in the lavas beneath the dry land; a diminution of density of the land masses and corresponding upheaval. Further precipitation of water would widen and deepen the early oceans. Finally the uniform pressure of about 300 atmospheres becomes concentrated as a pressure of some 400 atmospheres over perhaps eight-elevenths of the earth's area, if we assume some such concentration of water as at present exists. The several conditions attending the gradual precipitation of the gaseous envelope upon the surface render it improbable that a uniform ocean covering the entire globe ever existed, even if it could have remained in equilibrium on a thinly crusted earth possessing an energetic substratum.

The effects of this new distribution of pressure must have been to flood the land areas with lavas extruded from beneath. A change of pressure of from 300 atmospheres to one comparatively nil might be represented by an unloading of our present continental areas to the extent of 3,600 feet of rock of a specific gravity of 2.5.[3] And this

[1] A Theory of Sunspots. By J. Joly. Roy. Dub. Soc. Proc., n. s., Vol. VIII., 1898, pp. 697–700.

[2] Professor Sollas, F. R. S., in his lectures in dwelling on the facts of the inception of ocean basins, has frequently pointed out that these must have dated from the rainfall attendant on the fall of temperature to the critical temperature of water.

[3] One effect of this would be that over the land surfaces the melting point of a rock such as Diabase would be raised about 8° C. This would tend to confer some greater rigidity on the exposed crust of the earth.

unloading must have been effected in a comparatively short period—"instantaneously," if contrasted with the slow unloading effected by denudation.[1] Such a redistribution of pressures must have inaugurated remarkable lithological differences in the subaqueous and subaerial portions of the lithosphere. It is to be anticipated that beneath the ocean the effects of the primordial conditions of fusion in presence of volatile matter at high pressure would be more perfectly preserved than over the early land areas, where the reduction of pressure and still shallow crust would tend to the expansion and extrusion of the original magma. A diminished mean density of the suboceanic crust does not, however, necessarily follow. On the contrary, the conditions of greater pressure under which it was formed must be supposed to have conferred greater density upon it, and to have favored the differentiation and crystallization of the denser silicates. If sufficient time elapsed for these differences to become deeply established in the crust of the earth, a subsequent reversal of the distribution of pressure must be improbable. It is difficult to conceive that the limited range of transportation attending denudation can have led to any extensive subsequent redistribution of equilibrium. Tidal convulsions would appear to be the only refuge of those who object to the permanence of the continents.[2]

The upper part of what is now the earth's solid crust must, as we have urged, have contained, as silicates in the form of slag, lava, or rock, the alkaline earths now appearing chiefly as carbonates, the alkalies now distributed between the salts of the sea and the alkali silicates of the rocks, along with iron and alumina. The early hydrosphere must, for want of other known alternative, be supposed to have contained a quantity of hydrochloric acid roughly represented by the chlorine now in the ocean. Carbonic anhydride also entered into its composition, and the atmosphere, enveloping all, must have still been largely in excess of our present atmosphere, principally owing to the presence of carbonic anhydride and hydrochloric acid. The waters of the early ocean, and the rain which then fell upon the lavas and rocks of the land, possessed solvent powers greatly in excess of what we at present observe. Those who have maintained that the sea was "salt" from the first, if they paused here, would doubtless find considerable support for their views; and, of course, the right or wrong of the matter turns upon what one means by "salt." We are

[1] It is not to be supposed that tidal disturbances permitted this allocation of the surface to take place quietly, and without swaying at each vibration of our satellite, then possibly much closer to the terrestrial surface.

[2] See Physics of the Earth's Crust (by the Rev. Osmond Fisher: Macmillan & Co., 1889, pp. 297, 298), where increased density of the lithosphere beneath the ocean is for other reasons inferred.

only concerned now with one element of the ocean, the sodium, and it will be easy to show that complete neutralization of the acid hydrosphere would have been attended by only a relatively small introduction of sodium into the ocean.

We can make a rough estimate of the results of this primeval chemical denudation—and hence of the correction on the estimate of geological time involved in the primitive saltness of the ocean—by allocating the action of the acid among the constituents of the early crust; but we have first to inquire into the percentage relations of these constituents.

Mr. F. W. Clarke has estimated the percentage amounts of the elements contained in the earth's surface crust. In Mr. Clarke's first report[1] the mean of 880 selected analyses of American and European igneous, volcanic, and crystalline rocks is tabulated along with the means of the component analyses divided into local groups, as the rocks of the Western States, of northern California, of European volcanic and crystalline rock, etc.; and it is remarked as the result of comparing these groups that "the thesis that the crust of the earth is fairly homogeneous in composition is thus sustained by positive evidence." In a later publication[2] 960 analyses are consulted, and these of a still more carefully selected and reliable character, giving an average "which may fairly represent the composition of the older crust of the earth." The result, which closely agrees with the earlier estimate, is contained in the column below.

SiO_2	59.77
Al_2O_3	15.38
Fe_2O_3	2.65
FeO	3.44
CaO	4.81
MgO	4.40
K_2O	2.83
Na_2O	3.61
H_2O	1.51
TiO_2	.53
P_2O_5	.21
	99.14

This approximates to a diorite, and would fall among Rosenbusch's series of "granito-dioritischen" and "gabbro-peridotitischen" magmas.[3]

Such a rock or lava attacked by a heated solution of hydrochloric acid must ultimately yield its iron, calcium, magnesium, potash, and soda as chlorides.

[1] Bulletin of the U. S. Geological Survey, No. 78, 1891, p. 34.

[2] Bulletin of the U. S. Geological Survey, No. 148, 1897, p. 12.

[3] Elemente der Gesteinslehre. Stuttgart, 1898, p. 187. See No. 15 of this group for a rough approximation to Clarke's average.

The atomic percentages of Clarke's average are given by him as follows:

Iron	4.71
Calcium	3.53
Magnesium	2.64
Potassium	2.35
Sodium	2.68

The chlorine taken up may be assumed to be distributed as follows: In the first instance—Fe_2Cl_6, $CaCl_2$, $MgCl_2$, KCl, and NaCl.

The chlorine will be allocated as follows among these elements:

Four and seventy-one one-hundredths units of weight of iron take up 9 units of chlorine nearly; 3.53 units of calcium join with 6.3 units; 2.64 units of magnesium take up 7.6 units; 2.35 units of potassium take up 2.14 units, and 2.68 units of sodium unite with 4.1 units of chlorine. From this it follows that the chlorine taken up by the sodium bears to the total amount of acid neutralized the ratio of 1 to 7.5. If, then, there had not been any supply of chlorine subsequently from the rocks, as there has been, this would represent the fraction of the present sodium chloride which was, with comparative rapidity, thrown into the primeval ocean in the first stages of denudation. In other words, of the entire quantity of HCl at that early period neutralized by reaction with the constituents of the rocks only 14 per cent can have been expended in bringing the sodium into solution as sodium chloride.

If, therefore, we estimate the chlorine in the original ocean, we may, on the foregoing basis, take 14 per cent of this as having existed in it as sodium chloride.[1]

In estimating the chlorine in the primeval ocean we have to consider that what is now in it is in excess, to some extent, of what originally existed in it by the amount that has been discharged by the rivers during the subsequent history of the earth. Clarke shows that careful analysis of rocks reveals this element in many rocks wherein it had previously not been looked for. He estimates that it exists to the extent of 0.01 per cent of the original crust.[2] In river discharges it will be seen (*ante*) to amount to no inconsiderable amount, entering chiefly as chloride of sodium, but also as lithium and ammonium chloride. The chloride of sodium is undoubtedly partly derived from

[1] Note in reference to the calculation respecting the neutralization of free hydrochloric acid:

The effect of the aluminum should also be taken into account. This would reduce the estimated percentage of acid neutralized in the formation of sodium chloride, and so raise somewhat the estimate of geological time.

The margin of error assumed in the final estimate of geological time must, however, cover the oversight, but leaving the balance of probabilities in favor of a duration more nearly ninety than eighty millions of years.

[2] Bulletin U. S. Geological Survey, No. 148, p. 13. See also Bischof's Chemical and Physical Geology.

the sea itself. It enters into the composition of rain water in districts bordering or near the sea. It would appear that farther inland it is an inappreciable constituent of rain water. At Rothamstead the average of 71 analyses afforded 0.33 of chlorides in 100,000. At Lands End this rose to 21.8 in 100,000. On the west and east coasts of Scotland it is 1.19 and 1.26, respectively, per 100,000. In London it is 0.12, and in Ootacamund, India, it is only 0.04 per 100,000 parts,[1] the latter town being some 300 miles from the coast, in south India. The amount in British rivers free from pollution is 1 in 100,000; and evidently, as these represent a concentration to one-third of the rainfall, this amount would be accounted for by the chlorine carried from the sea.

This is not the case with the great rivers of the world. Many of these must derive their chlorides from the rocks by solvent denudation.[2] Some deduction should, however, be made for the supply referable to the sea. A deduction of 10 per cent is probably sufficient; it is a correction on a correction. This need be applied to the sodium chloride only, reducing it from 16,657 tons per cubic mile to 15,000 tons, in round numbers; and, multiplying by the number of cubic miles of river discharge, the total annual supply to the ocean is 97.8×10^6 tons of sodium chloride. There are also 16×10^6 tons of lithium chloride, and 6.5×10^6 tons of ammonium chloride. These quantities include a total of nearly 76×10^6 tons of chlorine. If we assume that the final result as to the duration of denudation will not be far from 86×10^6 years, we arrive at a total deduction of $6{,}536 \times 10^{12}$ tons as a correction on the amount of chlorine contained in the sodium chloride of the ocean.

On the other hand, however, an estimate of the total quantity of chlorine in the ocean must take into account the quantity of magnesium chloride present in it. This, calculated on the most recent estimate of the mass of the ocean (referred to *ante*) amounts to $5{,}568 \times 10^{12}$ tons, containing $4{,}161 \times 10^{12}$ tons of chlorine. The total chlorine in the original ocean is arrived at by adding to $24{,}155 \times 10^{12}$ tons, contained in the chloride of sodium, $4{,}161 \times 10^{12}$ in the magnesium chloride, and subtracting $6{,}536 \times 10^{12}$, as subsequently introduced. The result is $21{,}780 \times 10^{12}$ tons.

We can now apply these figures to correct the original estimate of geological time, which assumed that all the sodium in the ocean had been delivered by the rivers.

According to the results obtained by considering the effects of solution of a primitive crust approximating to the present eruptive and

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[1] Thorp's Dictionary of Applied Chemistry; article, "Water."

[2] See Bischof's Chemical Geology, Chap. VII, Vol. I, English edition, 1854. Bischof thinks the rivers can carry back to the sea only very little from the beds of rock salt (p. 111).

igneous constituents of the earth's crust, 14 per cent of the chlorine fixed in the salts brought into solution would be united with sodium. This we now find will amount to $3{,}049 \times 10^{12}$ tons combining with $1{,}972 \times 10^{12}$ tons of sodium. But we have already seen that to-day there are $15{,}627 \times 10^{12}$ tons of sodium in the ocean. The correction is 12.6 per cent. This on the period of 99.4×10^6 years is 12.5×10^6 years nearly, which is evidently a subtractive correction and reduces the estimate of geological time to 86.9×10^6 years.

This correction is based on the view that the chlorine now in the ocean, or nearly this amount, must originally have been free in the atmosphere and hydrosphere. This is assumed as the only alternative open to us in disposing of this substance. Previous writers have accepted this view. If free, it can hardly have been otherwise combined than with hydrogen. The dissociating temperature of HCl is some 500° above that of water; hence the chlorine would have taken its hydrogen before the formation of water was possible.

Sterry Hunt has further assumed that sulphur, in the form of acid gas, entered into the composition of the primeval atmosphere. The early high temperature condition would result in "the conversion of all carbonates, chlorides, and sulphates into silicates, and the separation of the carbon, chlorine, and sulphur in the form of acid gases, which, with nitrogen, watery vapor, and a probable excess of oxygen, would form the dense primeval atmosphere."[1]

That sulphuric acid existed in the early atmosphere and hydrosphere in at least relatively small quantities, is more than probable. In the sea-salts of to-day it forms a relatively small part, and is being supplied by the rivers at a rate which, acting over geological time, is far more than sufficient to account for all that is in it.[2] It is being constantly thrown down upon the ocean floor in the form of calcium sulphate, constituting about 0.7 per cent of the red clay, and 0.4 per cent of the radiolarian ooze, 0.3 per cent of the diatom ooze, and about 0.8 per cent of the globigerina ooze, as well as entering into other extensive floor deposits of the ocean. Sulphur exists, according to Clarke, as a constituent of the fundamental crust, amounting to 0.06 per cent. We find, then, not only a source of supply in the rocks, but an annual river supply more than adequate to account for what is in the ocean. We can not, therefore, fairly make allowance for its solvent action in early times. If free, it probably existed in relatively minute quantities.

In the case of carbonic acid we can effect a fairly satisfactory estimate of its amount from the volume of limestone rocks now on the earth's surface; but in the case of this acid we find a much less energetic body

[1] Chemical and Geological Essays, 1897, p. 40.

[2] The annual river supply of the element sulphur is about 124×10^6 tons, and the mass of S in the ocean is about 12×10^{12} tons. A part of the sulphates of the rivers is derived from rain, and hence from the sea.

than hydrochloric acid. Its activity has probably extended all through geological time, and its gradual absorption been marked by the limestones of all ages. We must bear in mind, however, what has frequently been pointed out, that the volcanic extrusion of this substance throughout geological time has probably been considerable.

The only warranted correction of large amount appears, then, to be that due to the attack upon the early rocks by the hydrochloric acid. We make a deduction of 12.5 millions of years for the primeval salinity of the ocean brought about by this means. It is, however, evident that the solution of this amount of material was not effected instantaneously, but by an accelerated denudation, extending over some period of time, the duration of which, we can satisfy ourselves, however, is nearly negligible. A rough estimate of the amount of denudation, as a layer of rock removed from the whole terrestrial land surface, is easily arrived at.

We have already found, in fact, that $1,972 \times 10^{12}$ tons of sodium must, roughly, have been dissolved out of the primeval rocks. In Clarke's table of the atomic ratios of the constituents of these rocks, sodium appears as 2.68 per cent. The entire mass of rock reduced to detrital sediment and brought into solution to supply this amount can, of course, be estimated from this. It amounts to 73×10^{15} tons. On Professor Wagner's determination already referred to, the area of the globe is about $1,965 \times 10^{5}$ square miles; and, taking the specific gravity of the original rock to be that of diorite (2.95), we find the amount denuded would cover the earth to a depth of 157 feet.

The most careful estimate of the present mean rate of subaerial denudation of eruptive and crystalline rocks amounts to about 1 foot in 3,000 years, removed from the surface, partly in solution, partly by transportation, to the rivers and sea. The primeval ocean was, according to our view, a dilute solution of HCl.

Bearing in mind the fact that the solution of hydrochloric acid would become impoverished as time progressed and insoluble residues cover up a fraction of the earth's surface, it would seem to be sufficient to assume that its mean rate of activity was five times that of present subaerial agents. This affords 1 foot in 600 years, or to denude 157 feet of rock, 94,200 years. In fact, even at the present rate of denudation, the large surface we have assumed as exposed to denudation reduces the period of time required to remove such a rock layer to a negligible duration. The only correction that is admissible would be a unit in the decimal place of our correction of 12.5×10^{6} years, reducing the correction to 12.4×10^{6} years, and leaving geological time, dated from the condensation of water upon the globe, to be 87×10^{6} years.

The foregoing corrections involve not only the assumption that the hydrochloric acid was free, but also that we may assume from the mean analyses of igneous, eruptive, and crystalline rocks a knowledge

of the primeval crust exposed to denudation. The latter assumption appears justified, not only for the reason that any other is gratuitous and prima facie unwarranted, but also from the fact that the sodium contents of the sedimentaries, as now existing, if increased by what is now the ocean, reverts very nearly to that of Clarke's primary crust.

The assumption of this acidic denudation of the primeval rocks leaves the ocean charged principally with chlorides at the dawn of geological history. Carbonates must also have entered into the composition of the primeval ocean, probably as minor constituents. Sulphates possibly also existed in relatively small quantities.

Sterry Hunt believed that the waters imprisoned in the pores of the older stratified rocks, and which are "vastly richer in salts of lime and magnesia than those of the present sea," might be regarded as the fossil sea-waters of the ancient ocean. He gives a theory of the subsequent changes in ocean chemistry, suggesting that the carbonates of the alkalies and the alkaline earths in subsequent geological history carried to the sea by the rivers, would first precipitate the dissolved alumina and the heavy metals, "after which would result a decomposition of the chloride of calcium of the sea water, resulting in the production of carbonate of lime or limestone and chloride of sodium or common salt."[1]

III.—THE SUPPLY OF SODIUM BY THE RIVERS.

Before turning to other considerations we must attend to a correction which we have already touched upon and which is not negligible. In deducting from the river supply of sodium a quantity equal to 10 per cent of the sodium chloride as being derived directly from the sea, we evidently reduce our divisor and so increase our estimate of geological time. The deduction of 10 per cent can, of course, be accepted as no more than a rough allowance—possibly a little excessive.

The quantity of sodium chloride thus assumed as derived from the sea is 1,657 tons per cubic mile of river water, or 108×10^5 tons for the entire annual river discharge. Calculating the sodium only, this becomes 42×10^5 tons per annum. We have already calculated the quantity of sodium in the ocean of to-day, and found it to amount to $15{,}627 \times 10^{12}$ tons. But of this we have reason to believe $1{,}972 \times 10^{12}$ tons are to be ascribed to the rapid denudation of the original rocks, leaving $13{,}655 \times 10^{12}$ tons to be accounted for by subsequent supply from the rivers. This river supply amounts to a total of $15{,}727 \times 10^4$ tons per annum, to which must be applied the correction for the observed supply to rivers of sodium abstracted from the sea and precipitated upon coastal countries by rain water. This, as we have just seen, is

[1] Chemical and Geological Essays, 1897, p. 41. See also Bischof's Chemical and Physical Geology, London, 1855, Vol. I, p. 7, and Sir A. Geikie's Text-Book of Geology, third edition, p. 412, Deposits in Salt and Bitter Lakes.

estimated at 42×10^5 tons per annum. Hence the river supply is now reduced to $15{,}307 \times 10^4$ tons. The quotient of $13{,}655 \times 10^{12}$ by $15{,}307 \times 10^4$ is 89.2×10^6. To this number of years may be added the decimal 0.1×10^6 years as the period approximately required to effect the denudation of the primitive rocks to the extent of fixing the free hydrochloric acid, giving, finally, as the estimate of the duration of denudation, 89.3×10^6 years.

It must not be understood from the foregoing that we claim a degree of accuracy for our estimate approximating to so small a time interval as 100,000 years. The period is only taken into account as arising from our figures. It will be seen later on that a far larger margin of error is of necessity assumed.

It will conduce to clearness to summarize here a statement of the corrections:

Basis of calculation.	Duration, in millions of years, of geological time since condensation of water on the globe.
1. If no free acid existed in the primeval atmosphere and the total river supply of sodium be assumed as derived, at a uniform rate, from the rocks..................	99.4
2. As 1; but assuming that free acid in the original atmosphere, to the extent calculated from the chlorine now in the sea, less that subsequently supplied by rivers, attacked the original rocks and became neutralized in negligible time............	86.9
3. As 2; but allowing for a period of acid denudation at a rate five times the average rate of present subaerial denudation	87
4. As 3; and assuming 10 per cent of the sodium chloride in the river discharge to be derived from the ocean..	89.3

Of these estimates, No. 4 is based on the most complete estimate of probabilities.

We have still to consider known or possible sources of disturbance which, with our present knowledge, hardly admit of numerical approximation. We hope to show, however, that the resultant of their often opposed effects was probably subtractive, and must be included in an allowance of about 10 per cent.

IV.—THE SALINE DEPOSITS.

Very considerable deposits of rock salt, etc., occur among bedded rocks of various ages—even those of early Paleozoic times—as the salt range of the Punjab, which dates back to Cambrian age.[1] That these in the aggregate represent a very considerable mass of sodium chloride can not be doubted, although their local character and

[1] Sir A. Geikie's Text-Book of Geology, third edition, p. 737.

limited extent reduce this amount probably to but a small fraction of that contained in the sea.

It is believed by some geologists that such beds were derived from the sea by inclosure of bays, etc., and evaporation to dryness of the landlocked water. There are, however, many arguments for believing that such occurrences must have been rare, and for the support of the opposed view that they represent the deposits of areas deficient in rainfall. In the hypothetical bays a bar must occur by crust elevation in such a position as to cut this off and imprison the water. This must be effected sufficiently rapidly to overtake the tidal scour which proves the more effective in preserving the channel of communication with the sea the more narrow the channel becomes. But this is not all. The landlocked bay is very unlikely to contain the salts adequate to account for the thickness of the beds and periodic variations formed in the deposit. Fresh influx of sea water must be therefore obtained, or the advocates of this view must now join hands with the advocates of the rival theory and claim "rainless" conditions to finish the deposition of salts in the inclosed area.

In the best example known of a salt lake of marine origin (the Caspian Sea) the waters as a whole are not so saline as those of the Mediterranean. Ultimately evaporation must, however, lead to extensive salt deposits in this sea. But these will only to a fractional extent be derived from the sea. "Salt lakes of oceanic origin are comparatively few in number;"[1] and we see by this example of one that it by no means follows that the salt deposits so derived ever formed part of the original ocean save to a small extent.

The ordinary history of the rock-salt deposit is undoubtedly that of the majority of the present salt lakes of the world. The formation of such deposits is, indeed, inevitable wherever a depression and rainfall below the amount required to flood the depression to repletion exist. The inflow of the rain to such an island basin, indeed, diminishes as the basin fills up, and the evaporation correspondingly increases. When the latter balances the rain supply, the waters continue to grow in salinity, till a salt lake—derived from denudation within the watershed—is formed. Such have been formed in all ages at periods even older than the Silurian. Thus "some of the more important beds of America belong to Upper Silurian, Carboniferous, Triassic, or Tertiary ages, and vary in thickness from a mere film to upward of 1,200 feet," and are ascribed by Mr. G. P. Merrill to the evaporation of water in inclosed lakes and seas.[2]

So far as our present theory is concerned, there is no need to take these into consideration; for, in point of fact, they are already consid-

[1] Sir A. Geikie's Text-Book of Geology, third edition, p. 410.

[2] G. P. Merrill's Treatise on Rocks and Rock Weathering and Soils, p. 120, 1897.

ered in the estimate of river supply to the ocean furnished by Sir J. Murray, which takes into account only that falling directly into the ocean. The drainage of the "rainless" regions of the earth—regions where the rainfall is less usually than 11 inches per annum, and which do not drain into the sea—is excluded. As in the present, so in the past, we conclude that such regions existed scattered over the land surface at various periods of the earth's history; and we find no better confirmation of the preservation of the present climatic conditions than exists in these telltale beds of saline deposits. They furnish a striking support to the uniformitarian views here advocated.

At the best the stratified salt deposits of the earth must form only a very small fraction of what has accumulated in the waters of the ocean. The rock salt of the latter would cover the entire dry land of the earth to a depth of 400 feet. The other deposits are entirely local, and but rarely attain this thickness. We see from what has been said that the fractional part of some of these deposits, which actually go to throw error into our calculations, makes so small a part of a small correction that we are not concerned with its estimation.

V.—THE ALKALIES OF THE ROCKS.

It is a fact of great interest in connection with our present consideration that the igneous and eruptive rocks, as a whole, possess an amount of soda alkali which preponderates over potash alkali, while in the case of the sedimentary rocks this is in the very large number of cases reversed, the potash alkali exceeding the soda alkali.

This becomes clear in the light of what we have already considered as regards the gradual derivation of the salts of the ocean in the process of formation of the sedimentaries, coupled with the fact that, under or during conditions of weathering, potash aluminium silicates are more resistant than soda aluminium silicates. This and another cause for the retention of the latter salts will be reverted to again. The fact we wish to dwell on here is the ultimate one that this chemical distinction, broadly speaking, exists between the igneous and sedimentary rocks. We shall also find that the restoration of the known amount of sodium in the ocean to the sedimentary rocks will bring them up to the sodium percentage of the igneous rocks. A like restoration can not be effected for the potash alkali, owing to reasons we have briefly to point out further on.[1]

The average igneous and eruptive primitive crust rock arrived at by Mr. Clarke (*ante*) possesses the alkali percentage:

K_2O	2.83
Na_2O	3.61

[1] In dealing with this question in this and the ensuing section the sodium and potassium of the sea will be calculated as the oxides, for the convenience of frequent references to rock analyses.

This we may compare with the results of averaging the rock analyses selected by H. Rosenbusch in his "Elemente der Gesteinslehre."[1]

These may be tabulated as follows (the references are to the pages of the work referred to):

	K_2O.	Na_2O.
Eruptive rocks:		
Mean of 19 Liparites, Trachytes, Basalts, etc. (p. 35)	3.08	4.37
Plutonic rocks:		
Mean of—		
18 Granites (p. 78)	4.26	3.80
46 Syenites (pp. 106–126)	5.60	6.80
25 Diorites (pp. 140, 141)	1.86	3.82
6 Essexites (p. 172)	2.49	4.68
8 Theralites and Shonkinites (p. 176)	3.86	4.96
14 Gabbros (p. 151)	0.77	2.62
Mean of eruptive and plutonic rocks	3.11	4.44
Also the following dynamically altered eruptive rocks:		
Mean of—		
5 Porphyroids and Serecite gneisses (p. 440)	6.44	1.68
12 Mica gneisses (p. 468)	4.24	2.99
4 Amphibole gneisses (p. 484)	3.14	5.13
6 Pyroxene and Augite gneisses (p. 486)	2.63	2.66
13 Hälleflinte gneisses (p. 493)	2.18	2.68
10 Amphibolites (p. 515)	2.02	3.96
6 Eklogites (p. 520)	0.35	1.85
Mean of metamorphic eruptive rocks	3.00	2.99
Mean of all	3.06	3.72

With reference to the last division of rocks—the metamorphic eruptives—Rosenbusch admits the old standing difficulty of distinguishing between the altered sediments and the altered eruptives. Thus gneisses derivable from sediments give just such chemical proportions as appear in those referred to eruptive origin. There exists no sure criterion for classing a gneiss according to its origin (p. 472, loc. cit.).

This is a well-known difficulty, and has been the subject of much research and speculation. We dare not do more than suggest here that in doubtful cases the general law of the alkali ratios of eruptives and sedimentaries, where this admits of application, should carry weight with petrologists.

The uncertainty referred to may, of course, affect the estimated composition of the crust rock and of the siliceous sedimentaries. Examination will, however, show that the uncertainty being confined to a couple of groups only, can probably affect the final averages but little in either case.

[1] Stuttgart, 1898.

We may now refer to a similar table of the sedimentaries, still deriving our figures as averages calculated from Rosenbusch's work:

Mean of—	K_2O.	Na_2O.
16 Sandstones, Quartzites, and Graywackes (p. 391)	1.56	0.92
12 Clays and Shales (p. 420)	1.71	0.48
17 Clay Slates (p. 425)	2.68	1.19
6 Calcareous Clay Slates and Whetstones (p. 428)	2.69	0.98
16 Phyllite schists, or Clay-mica schists (p. 437)	3.52	1.53
10 Schists (Serecite, Ottrelite, Chlorite, etc.) (p. 436)	2.36	1.04
13 "Pelit gneisse" (Phyllite gneisses) (p. 470)	3.18	1.69
8 "Psammit gneisse" (Sandstone gneisses) (p. 471)	1.95	2.13
3 Amphibole gneisses (p. 484)	1.44	2.65
4 Mica schists (p. 497)	3.85	2.07
Mean of sedimentaries	2.49	1.47

We may observe further that the averages afforded by the valuable collection of analysis of American rocks, compiled by Messrs. Clarke and Hillebrand, will be found to confirm these results.[1]

For the original crust, Clarke's alkali ratio works out—

$$\frac{Na_2O}{K_2O}=\frac{1.29}{1},$$

and Rosenbusch's,

$$\frac{Na_2O}{K_2O}=\frac{1.22}{1}.$$

On the other hand Rosenbusch's sedimentary rocks show that—

$$\frac{Na_2O}{K_2O}=\frac{0.59}{1}.$$

When it is remembered that, age by age, those sediments were being deposited, some directly from the parent igneous rocks and others by denudation of former sediments, the great importance to the present hypothesis of this broad difference in the alkali ratios, and in the absolute amounts of sodium and potash in the original and derived rocks, must be evident.

If now the inference is right that the missing alkalies were supplied to the ocean, we should expect to find on a rough approximation of the bulk of sedimentaries, and hence of the original rocks giving rise to them, that such a mass of parent rock would be adequate to supply the sodium in the ocean. And this is actually the case; we find, in fact, that the estimated amount of sedimentary strata would, in its formation, be adequate to yield to the ocean the sodium that is in it, assuming these sedimentaries to be derived from rocks having the mean composition of the important eruptive masses now known.

[1] U. S. Geol. Survey, Bulletin No. 148, 1897.

Even more, the result of the calculation indicates that what is in the ocean is not quite a full measure of the sodium washed from these rocks. Recollecting that the stratified rock salt—the former inland-sea deposits—should enter the estimate on the side of the amount credited to the ocean, the result must be regarded as satisfactorily favoring the hypothesis. The restoration of the potash is attended with difficulties to be referred to later, which render such a satisfactory result impossible.

For an estimate of the amount of sedimentary rocks on the earth's surface we are indebted to Mr. T. Mellard Reade.[1] For the average thickness of sedimentary rocks down to the base of the Cambrian Mr. Reade takes a volume equal to the land area covered to the depth of one mile, this being based on the results of borings, sections, etc. This commends itself as a good approximation. He further, however, assumes that a similar volume of sediment exists under the sea. The latter assumption is probably excessive, even if it includes the relatively small additional amount of dissolved matter in the ocean. Pre-Cambrian sedimentary rocks are so comparatively limited in amount that the inclusion even of these, as defined by our present knowledge, can hardly justify the total of the estimate. However, we will provisionally accept it and carry out our calculation applied to the mass so defined.

Mr. Reade[2] estimates 10 per cent of the land sediments to consist of calcareous rocks, and also that the total mass of calcareous rocks of the earth would suffice to cover its surface to a depth of one-tenth of a mile. To arrive at the amount of siliceous detrital sediments from these estimates we must deduct from his estimate of the total sedimentaries such a mass of calcareous rocks as would cover the earth to a depth of one-tenth of a mile, and further make an allowance for precipitated materials other than calcareous. Neglecting the last deduction as being a comparatively small one, we find that the deduction of the calcareous rocks leaves his estimate of rocks other than calcareous to amount to a layer 1.6 of a mile in thickness over the land-area of the earth. Hence the mass in tons is equal to $558\times10^5\times1.6\times2.5\times42\times10^8$, or 94×10^{16} tons nearly. The value 558×10^5 is the area of the layer in square miles, 1.6 its thickness in miles, 2.5 the assumed specific gravity of the rock, and 42×10^8 the mass in tons of a cubic mile of water. The mean soda of the more abundant sedimentaries amounts, as we have seen, to 1.47 per cent. Hence 13.8×10^{15} tons of soda exist in this mass of detrital sedimentary rocks. To this must be added the known amount of soda in the sea, which is obtained by converting $15,627\times10^{12}$ tons of the chloride to the oxide, giving 21×10^{15} tons. The restoration of this to the rocks therefore raises their amount of Na_2O to 34.8×10^{15} tons.

[1] Geol. Mag., Vol. X, 1893, p. 97.

[2] Geol. Mag., Vol. VI, 1879, and Proc. Roy. Soc., Vol. XXVIII, 1879, p. 281.

The total bulk of sedimentary rock on Mr. Reade's estimate is, however, equal to a layer 2 miles deep over the dry land.[1] This amounts to 116×10^{16} tons; hence, with the sodium of the ocean restored to them, we find the soda percentage from the fraction $\frac{3.48}{116}$, which is 3 per cent. This is about the soda percentage of many granites, gneisses, and diorites, etc., but falls somewhat short of the average of the eruptive and igneous rocks. The stratified salt deposits would somewhat raise the figure to over 3 per cent. Clarke's average original crust has 3.61 per cent.

It appears very probable that we may in part trace the deficiency to the estimate of sedimentary rock beneath the ocean. This must be mainly precipitated material. The detrital deposits can only be a fraction of that upon the land. We can easily see how an estimate on somewhat different and, it is submitted, more satisfactory bases may be effected, bringing almost exact agreement between the restored sediments and the primal rock.

We can use the broad fact—to be presently shown—that the comparison of disintegrated and decomposed rock material of the present day, constituting soils of various rock-formation, reveals a loss of constituents of parent rock amounting on the average to 38 per cent. When it is remembered that such soils represent in many cases extreme stages of weathering never attained to by many sediments, but that these latter are often the result of little more than disintegration and transportation, it appears probable that 30 per cent may be assumed as the loss by solution of the entire detrital sediments. We accept 1 mile deep of these on the land, and, confining ourselves to purely detrital siliceous sediments, assume that as much as 10 per cent of what is on the land is in the sea, or, say, a total of 1.1 mile deep over the land area. We include in this the pre-Cambrian detrital sediments.

To recover from this the original mass of parent rock, we assume that a loss of 30 per cent by solution occurred in the process of denudation, or, in other words, the 1.1 mile of detrital sediments is 70 per cent of the original mass of parent rock. The mass of 1.1 mile deep of sedimentary rock of specific gravity of 2.5 will be 64×10^{16} tons. This being assumed as 70 per cent of the original mass, the latter is 91×10^{16} tons.

The mass of 64×10^{16} tons contains 940×10^{13} tons of Na_2O. Adding the amount in the ocean (21×10^{15} tons), we obtain 30.4×10^{15} tons.

[1] In stating that there is as much sedimentary rock under the sea as upon the land Mr. Reade possibly implies that the submarine sediments are to be estimated as possessing a *thickness* of 1 mile. Mr. Reade's calculation of the geological age of the earth on the rate of denudation of the sediments appears, however, to involve that the *bulk* of sedimentary material beneath the ocean is, in his opinion, to be taken as about equal to what is upon the land, or the total bulk is equal to the land-area covered to a thickness of 2 miles. (Geol. Mag., Dec. 3, Vol. X, pp. 97-100.)

This restored to the original mass of 91×10^{16} tons gives a percentage of 3.34.

It may be independently shown that the soda ratio of the original rock to that of the sedimentaries supports the view that 30 per cent must have been about the loss, by solution, of the original rock. We assume that the sedimentaries are derived from an original rock, such as Clarke arrived at, but we assume no more.

To see this, we have to refer again to Mr. Merrill's valuable book,[1] which gives a useful collection of analyses of rocks and their derived soils.

Omitting a few cases, i. e., a phonolite containing a soda-zeolite, giving exceptional results on weathering, an incompletely recorded basalt, and a soapstone, his examples give the following results:

	Percentage loss of entire rock revealed in the residual soil.	Percentage of each constituent lost.	
		Na_2O.	K_2O.
Granite (p. 209)	13.47	28.62	31.98
Gneiss (p. 215)	44.67	95.03	85.52
Syenite (p. 216)	56.28	97.11	81.85
Diabase (p. 221)	14.93	12.83	29.15
Diabase (p. 222)	39.51	95.37	45.88
Basalt (p. 223)	60.12	74.41	83.34
Diorite (p. 225)	37.51	84.87	38.75
Mean	38.0	69.7	56.3

This indicates that if at this stage of weathering these soils were removed, redeposited, and reconsolidated, the mass of the parent rock would have been correctly estimated, on the basis that the mass removed in solution formed but 38 per cent of the original rock. At this stage of weathering we see that 69.7 per cent of the original soda was removed.

If we assume that the loss of the soda bears to the loss of the entire rock a constant ratio—and with the exception of the first-quoted diabase this appears supported by the individual examples—we can apply to the mean analysis of the sedimentaries on the one hand and to that of the mean original crust on the other to arrive at a rough estimate of the loss of entire rock by solution in the process of formation of the former.

We find that (ante) 3.61 per cent of Na_2O in the crust is represented by 1.47 per cent in the sediment. From these figures we can calculate the amounts of this constituent lost and saved. To effect this accurately we must suppose some one constituent to pass over without loss from the one rock to the other and use its percentage as a stand-

[1] Treatise on Rocks, Rock Weathering, and Soils. Macmillan, 1897.

lated on the amount of the chloride, the total is 968×10^{13} tons. Compare this now with the sodium of the ocean calculated as soda, and amounting to $2,100 \times 10^{13}$, and we have a ratio of 1 to 2.2. Had we assumed 0.8 as the missing percentage of potash, allowing such a deficiency as exists in the case of the soda to be accounted for by glauconite and other marine deposits of the land, and estimating that the deficient 0.8 per cent existed now in the suboceanic deposits, we find in the sea and its deposits 796×10^{13} tons. This bears to the soda the ratio of 1 : 2.7, which fairly well agrees with the ratio obtaining in the alkalies of the rivers.

From these figures we see that the deficiency indicated by the rocks is quite adequate to justify the supposition that the present alkali ratio of the rivers existed in the past. To suppose the river supply still less in the past is to make the record of the sedimentary rocks still more astray; or, from another point of view, the record of the sedimentary rocks—if we accept the same data as agreed with the facts with regard to soda alkali—suggest that the rivers of the past must have discharged an equal, or even greater, amount of potash than at present.

We may put the matter again in another way, which brings out more clearly the true nature of the evidence: The ratio of the potash to the soda in the rivers, if preserved throughout the history of denudation, would account for the alkali relations of the primitive and the derived rocks. This is independent of our estimate of geological time. The argument is, in fact, mainly directed against any assertion that the relative amounts of the alkalies supplied by the rivers of to-day is at variance with their probable past supplies.

If this ratio has varied seriously in the long past, then a difficulty not easily surmounted has to be faced. The difficulty may be put thus: The mean potash percentages of the parent and of the derived rocks are determinable, and the difference represents a certain amount of potash which may be considered within limits known. This must have been removed from the parent rocks in some manner. If not by denudation, then in what manner? The fact that we can not estimate it in the sediments or in the suboceanic deposits appears legitimately referable to our ignorance. The assumption that the rivers supplied less potash in the past leaves the revelation of the rocks inexplicable. The assumption is made in order to explain what is really a hypothetical deficiency (that of the potassium in the oceanic reservoir), and renders inexplicable an actual known deficiency (that of the potassium in the rocks).

The argument thus supports our uniformitarian views by overbearing an objection often urged against the uniform supply of the constituents of the rivers.

This brings us face to face with the question as to where and in what form this missing potash is to be sought.

The glauconite deposits of the deep-sea boundaries and the stratified marine sediments undoubtedly must be chiefly made responsible. The composition of this substance is given in the report on the deep-sea deposits of the *Challenger* expedition, where it is shown that it contains from 2.52 to 4.21 per cent of potash derived from the sea water. It may amount to 50 per cent or 60 per cent of the shallower deposits, or even more. The percentage of soda is from one-third to one-seventeenth of the potash, and therefore will hardly enter into consideration in this paper.

The formation of this substance appears dependent on the condition that the organic matter in the chambers of foraminifera should reach the bottom, which, if so, will perhaps account for the absence of this body from the deeper deposits. The organic matter "transforms the iron in the mud into sulphide, which may be oxidized into hydrate, sulphur being at the same time liberated. This sulphur would become oxidized into sulphuric acid, which would decompose the fine clay" (terrigenous débris), "setting free colloid silica, alumina being removed in solution. Thus we have colloid silica and hydrated oxide of iron in a condition most suitable for their combination." "There is always a tendency for potash to accumulate in the hydrated silicate formed in this way, and, as we have stated before, this potash must have been derived from the sea water."[1]

The following extract (p. 384) will serve to show the opinions of the authors (Sir J. Murray and Professor Renard) on the widespread nature of this deposit:

"It has already been stated that glauconite is one of the minerals most widely distributed in sedimentary rocks. It is found in the primary formations of Russia and Sweden among sands and gravels, in the Cambrian sandstone of North America, in the Quebec Group of Canada, and in the coarse Silurian sands of Bohemia. In the secondary formations its presence is more pronounced, for example, in the Lias, and especially in the middle and upper layers of the Jurassic system in Russia, in Franconia, in Swabia, and in England. It has a still greater development in the sands, marls, and chalks of the Cretaceous formation. It will suffice to recall the glauconitic rocks of the Neocomian, of the Gault, and of the Cenomanian in various regions, such as the glauconitic marls of France, Germany, England, and several parts of North America. The abundance of glauconite is continued into the Tertiary formations, from the lowest up to the highest horizons of the series.

"From this rapid enumeration it is seen that glauconite traverses the whole of the geological periods and its formation is continued in modern deposits along many continental shores explored by the *Challenger* and other expeditions."

The analyses show that the mineral may vary in composition.

"All that can be said is that the glauconite now forming at the bottom of the sea is, like the glauconite of geological formations, a

[1] Report, p. 389.

hydrous silicate of potash and of ferric oxide, containing always variable quantities of alumina, ferrous oxide, magnesia, and often lime." (P. 386.)

Merrill gives analyses showing that the glauconitic marls of New Jersey contain up to 7 per cent of potash, and remarks on the extent of such beds in the Cretaceous formation of New Jersey.[1]

Potash is also taken up by organisms in the sea, more especially by the seaweeds. A very considerable amount must exist in the immense masses of vegetation in the shallower waters of the sea.

There is further a very interesting manner in which potash is abstracted from the sea and returned to the land, which must, in its extension over geological time, have served to return immense quantities to the soils of coastal regions. This is by means of rain water.

In Dr. Angus Smith's work on Air and Rain it is recorded that near Caen, in France, it has been estimated (by M. J. Pierre) that a hectare of land annually receives from the atmosphere, by means of rain, 8.2 kilograms of KCl and 8 kilograms of K_2SO_4, amounting to a total of 7.9 kilograms of potassium. This is 1.23 tons of potassium per square mile per annum, or 1.48 tons of K_2O.

Now, it is a well-known fact that, whereas sodium salts so brought to the land are again freely yielded up by the soils, potash salts are retained. Vegetation also requires these salts as an essential constituent. Sodium salts are not essential to vegetation.[2]

In connection with this the relative losses of the alkalis, as shown in the table (ante) compiled from Mr. Merrill's work on Rock Weathering, should be considered. It appears from that table that the average loss of potash in the soils taken as examples was 56.3 per cent, the soda loss being 69.7. According to this the rivers are not carrying sufficient potash into the sea relatively to soda to account for what is going on under the decomposing effects of subaerial agencies.

We can see, too, that the revelations of the soil analyses are at variance with the broad facts of rock chemistry, to which we have been frequently referring. Thus, if we effect for potash a similar calculation to that carried out for soda and estimate from the average potash percentages in the sedimentary detrital rocks and of the primary crust rock the amount of potash lost and saved (assuming, as before, the alumina as the constant factor) we find the K_2O lost to be 15 per cent and the K_2O saved to be 85 per cent, which is evidently at variance with the soil analysis.

The discordance appears to be set at rest in the light of what we have already stated regarding the retention of potash in soils, recollecting that the surface soil will be the poorest in potash, whether by loss to

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[1] Report, p. 134.

[2] See Roscoe's and Schorlemmer's Chemistry, II., Part I., p. 57; also Mendeleeff's Chemistry, 1897, I., p. 546.

vegetation or by leaching out of soluble salts retained in the deeper lying parts. The matter is stated as follows by Mendeleeff:[1]

"The primary rocks contain an almost equal proportion of potassium and sodium. But in sea-water the compounds of the latter metal predominate. It may be asked, What became of the compounds of potassium in the disintegration of the primary rocks if so small a quantity went to the sea-water?

"They remained with the other products of the decomposition of the primary rocks. When granite or any other similar rock-formation is disintegrated there are formed, besides the soluble substances, also insoluble substances—sand and finely divided clay, containing water, alumina, and silica. This clay is carried away by the water, and is then deposited in strata. It, and especially its admixture with vegetable remains, retains compounds of potassium in a greater quantity than those of sodium. This has been proved with absolute certainty to be the case, and is due to the absorptive power of the soil. If a dilute solution of a potassium compound be filtered through common mold used for growing plants, containing clay and the remains of vegetable decomposition, this mold will be found to have retained a somewhat considerable percentage of the potassium compounds. If a salt of potassium be taken, then, during the filtration, an equivalent quantity of a salt of calcium—which is also found, as a rule, in soils—is set free. Such a process of filtration through finely divided earthy substances proceeds in nature, and the compounds of potassium are everywhere retained by the friable earth in considerable quantities. This explains the presence of so small an amount of potassium salts in the waters of rivers, lakes, streams, and oceans, where the lime and soda have accumulated."

This "absorptive power of the soil," according to Professor Hilgard,[2] is more displayed in arid than in humid regions.

The conclusion of the whole matter appears to be that, whereas the sodium compounds tend to accumulate in the waters of the ocean, the potassium compounds tend to be stored in the solid form or retained upon the land, and that to the causes which bring about this separation, and not to any differences in part processes of denudation, the remarkable scarcity in the ocean of potassium relatively to sodium is to be ascribed.[3]

VII.—UNIFORMITY OF DENUDATION BY SOLUTION.

Land area and rainfall.—The most prominent considerations involved in the question of how far the present rate of denudation by solution may be accepted as an average of that extending over past times are

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[1] See Roscoe's and Schorlemmer's Chemistry, II., Part I., p. 57; also Mendeleeff's Chemistry, 1897, I, pp. 546, 547.

[2] Quoted by Merrill. Treatise on Rocks, Rock Weathering, and Soils, pp. 369–370.

[3] The Palagonite coating on basic volcanic glass—apparently derived by a hydration and alteration of the glass and the taking up of a small additional amount of potash and soda (apparently from the sea)—is hardly sufficiently abundant, according to present knowledge, to justify consideration here. (See the report on the deposits, p. 304.) The Phillipsite appears to be a purely alteration product of the basic débris. (See Merrill, loc. cit., p. 375.)

that of the varying ratios of land and sea areas of the past and the amount of rainfall received upon the latter. The fact that paleontologically similar deposits in the various parts of the world are not necessarily contemporaneous, but homotaxial, debars the geologist from mapping the sediments of any horizon (even were these fully known) as forming part simultaneously of the oceanic area. Could he even claim full assurance here, the land areas supplying the sediments must still remain unknown.

In this difficulty indirect inferences only can be resorted to.

Those who accept the stability of the continents and oceans as a whole can not well admit that the balance of land and water was ever very seriously interfered with. Sir J. Murray[1] has calculated that if the present land of the globe were reduced to the sea level by being removed to and piled up in the shallow waters of the ocean its extent would be altered from the present 55×10^6 to 80×10^6 square miles, the ocean simultaneously changing from 137.2×10^6 to 113×10^6 square miles. The mean height of land, which is at present 2,250 feet, would become 0, while the mean depth of the ocean, at present 2,080 fathoms, would increase to 3 miles, 23.45×10^6 cubic miles of material being transported into the sea.

If the earth's crust were rigid and neither subsidence or elevation ever took place, such a calculation would mark the extreme distribution of the existing subaerial material which would be possible under the action of denuding agencies. It could only be brought about by an infinitely prolonged denudation and quiescence of the crust.

As a matter of fact, however, we know that over the continental areas there have been frequent depressions and elevations, and these acting alternately again and again over the same area. The Uniformitarian, we assume, regards this shifting balance of land and water as confined mainly to the area indicated above, the 80,000,000 square miles marking out the elevated plateaux of the globe. The dry land of to-day occupies some 68 per cent of this area. It can not be supposed to have ever occupied 100 per cent of it, for then sediments must have been laid down in the present ocean troughs. That such sedimentation, again, as we see in the great formations could have been effected without large areas of exposed land is impossible. These rocks infallibly assert the existence of dry land proportional to their own magnitude and complementary to their own submergence. The sedimentary deposits themselves suggest, then, from the necessities of their supply, a limit on the other side; that is, to the reduction of land area in past times.[2]

[1] Scottish Geological Magazine, 1888, pp. 1 et seq.

[2] See Wallace's Island Life, Chap. VI. See also Green's Physical Geology, 1892, pp. 687 et seq., and Three Cruises of the Blake, by A. Agassiz, 1888, pp. 126 and 166. The question of the permanence of continents and oceans has been so much discussed that further reference here is unnecessary.

The conditions of subaerial denudation of the present suggest considerable latitude within which the ratio of land to water may vary without affecting the denudation to the ocean. This is shown in the fact that the present amount of rainfall on the land is not sufficient to denude more than four-fifths of its area into the sea. The rainless regions of the earth are estimated by Sir J. Murray to amount to 12.2×10^6 square miles.[1] Over these regions the rainfall is less than 10 inches, and is reevaporated without reaching the sea. If the land area were diminished by this number of square miles, the effect on the supply to the ocean would probably be but small. If, on the other hand, it increased beyond its present extent, the rainless area would also most probably increase; but the denudation to the ocean would probably again be only affected in a comparatively small degree. In the extreme case of the entire land plateau being occupied by dry land, the disturbance of balance might so far affect the amount evaporated from the oceans as to diminish the land denudation.

Many causes act to influence the rainfall on the earth. The larger ones, as we have seen, will hardly act to produce great variations. The smaller we can not suppose, reasonably, will always conspire to act one way. We have already referred to the fact that, if the non-oceanic origin of the rock-salt beds be accepted, these deposits point to just such rainless regions in the past as now exist. The most cautious conclusion, we submit, must be that the facts of earth history over geological time, as we know them, do not point to any great or long-continued changes in the conditions of subaerial denudation.[2]

Chemical denudation.—Quite another factor in the uniformity of solvent denudation is the chemical and physical nature of the rock surfaces and soils exposed during the successive ages of the earth's history. With reference to the view that in earlier times larger areas of igneous rocks were exposed to denudation than in more modern periods, some remarks on soil and weathering are necessary.

We see in soil formation of the present day a process of ever deepening disintegration of the parent rock, and simultaneously progressing decomposition of the upper layers. This results in a surface layer, possessing a reduced percentage of the more soluble materials, which protects the richer material beneath. If the rock itself is for physical or chemical reasons highly resisting, the leaching out of soluble

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[1] Assuming that over areas with less than 20 inches rainfall there is complete reevaporation, only 36,697,400 square miles actually drain into the sea. Loc. cit.

[2] [*Note added in the press.*] The possibilities of sun history, however, enter the question. Professor Perry (Nature, July 13, 1899, p. 247) states it as his belief (in reference to Professor Newcomb's view that sun heat can have varied but little during Paleozoic time) that there may have been millions of years during which the sun may have been radiating at only one-third or one-tenth of its present rate. This would, of course, lead to diminished meteorological activity generally, although the denudative effects due to ice might increase. Those who hold that in the past there was much increased denudative activity should bear the possibility referred to by Professor Perry in mind.

materials from the surface layer must ultimately progress further for a given advance of disintegration than if the rock rapidly yields to the actions tending to disintegrate it.

In the surface layer the rain charged with carbonic and humic acids principally exerts its effects, the more soluble constituents yielding, of course, before the less soluble, and so growing finer in grain as time progresses. The more soluble substances thus become concentrated in the finer constituents of the soil.[1]

Ultimately, if mechanically transported to the rivers, a sorting, according to mass and dimensions, occurs. The finer-grained particles are carried on a current which drops the coarser particles. Thus the finer silts are richest in the soluble constituents of the former soils. They constitute material on which vegetation flourishes, and if deposited in the ocean build up rock masses rich in alkalies, chiefly, as we have seen, in potash. Nearer the shore the coarse grits and sandstones, poor in alkalies, accumulate.

Subsequent upheaval brings to the surface rocks, of which the finer-grained and softer varieties are those possessing the larger share of alkalies. These generally, owing to secondary or in some cases primary mica and their fineness of grain, are most distinctly cleavable. Such slates contain from 3 to 5 per cent of alkalies.

The dissolved materials pass through a different history, but in the limestones, etc., to which they give rise most generally there exists an amount of detrital feldspathic matter sufficient, when again uplifted and weathered, to yield soils scarcely less rich in alkalies than those derived directly from the parent rock.

This last fact is one of great interest. Merrill shows that soils derived as residual material from the most diverse rocks are very similar in composition.

The full tables should be consulted.[2]

	K_2O.	Na_2O.
Residual soil from limestone, Wisconsin, 4.5 feet from surface	1.61	2.19
Same, 8½ feet from surface	.93	.80
Residual soil from limestone, Wisconsin, 3 feet from surface	.83	1.45
Same, 4½ feet from surface	1.60	1.37
From dolomite, Alabama	2.32	.17
From diabase dike, North Carolina	Trace.	Trace.
A gabbro soil, Maryland	.86	.40
Subsoil from Trenton limestone, Maryland	4.41	.29
Soil from Triassic sandstone, Maryland	4.03	.79
Trenton limestone, unaltered	Not det.	Not det.
Residual soil from same	2.50	1.20
Gneiss, Virginia	4.25	2.42
Soil from same	1.10	.22
Diorite, Virginia	.55	2.56
Soil from same	.45	.56

[1] See Rocks, Rock Weathering, and Soils, pp. 365, 366, where this is proved by mechanical and chemical analysis.

[2] Loc. cit., pp. 305, 306; also, pp. 358, 359.

In the above it appears that the soils derived from the igneous rocks—more especially the more basic ones—show a greater poverty in alkalies than those derived from limestones and sandstones. This probably arises, in part, from more soluble alkali constituents being present, but in many cases doubtless from a more resistant parent rock leading to the more complete weathering of the soil. On the other hand, the more soluble limestones rapidly concentrate their siliceous materials to a soil rich in very fine feldspathic and other particles.[1]

In short, the daily and yearly action of the weather upon such soils would not show a yield of alkali greater in the case of those residual from igneous rocks than from those residual from sedimentary rocks. The attack on the rock beneath must furnish a very minute supply of alkalies contrasted with what is proceeding from the soil. Merrill refers to a calculation in reference to one of the Trenton limestone soils that in every cubic foot of soil "158,000 square feet of surface are exposed to the action of water and air as well as to the roots of growing plant."[2]

It is, too, a fact of common observation and comment, that igneous and eruptive rock masses are more slowly denuded than the majority of sedimentaries. Whether in regions of limestone or slate the higher and more abrupt surface features are generally the granitic or igneous masses, and this obtains although the weathering, as dependent on chemical decomposition, is most active on the eruptives.[3] The effect is greatly physical in origin.

> "In stratified rocks there is as a rule a lack of homogeneity, certain layers being more porous than others or containing mineral constituents more susceptible to the attacking forces."[4]

A full account of the conditions at work, so far as our present knowledge extends, appears in Merrill's work, already so frequently referred to.

The entire consideration shows that the greater richness in alkalies of the original igneous rocks is conjoined to such resistant physical properties as in the general case involves the more rapid turn over of the less rich sediments. The frequently greater richness of the residual soils of the latter is a consequence of this.

But, apart from such considerations, have we any valid reason to expect in the past a more rapid solution of the rocks than progresses at the present day? Factors enter the question on each side. The denser atmosphere of carbonic anhydride which may have obtained in the Paleozoic epoch, and which would have contributed not only more

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[1] See Rocks, Rock Weathering, and Soils, p. 307.

[2] Loc. cit., p. 308.

[3] Merrill, loc. cit., p. 271. The familiar appearance of igneous dikes standing out like walls above surrounding sedimentary rocks is an example.

[4] Loc. cit., p. 248.

carbonic acid to the rain, but by its great pressure have enabled this to take up a greater quantity, finds a set-off in the subsequent much greater development of vegetation. The humic and allied acids exert, as is now known, a powerful influence in promoting decomposition.

"There is reason to believe that, in the decomposition effected by meteoric waters, and usually attributed mainly to carbonic acid, the initial stages of the attack are due to the powerful solvent capacities of the humus acids."[1]

The mechanical action of the roots is also a very important factor. Now, these effects of vegetation were probably absent during the Pre-Cambrian and early Paleozoic epochs. Indeed, if the dense atmosphere of carbon dioxide existed, its mere mechanical effects when urged to the speed of a gale would have sufficed to destroy any but lowly plants in sheltered positions.[2]

The carbonic anhydride of the atmosphere of to-day by no means corresponds in amount with that which effects the operations going on in the soils. The percentage of CO_2 in soils is far greater than in the air. The decay of vegetation is probably ultimately responsible for this increase. While the CO_2 in 10,000 parts by weight of the atmosphere may be about 6, that in soils rich in humus may rise to 543 parts.[3] This is the atmosphere actually concerned with the destruction of feldspars, etc.

The existing soils of a considerable part of the Northern Hemisphere are due to the glacial effects of older Quaternary times. However, in the loess of China, Europe, etc., the adobes of America, and similar clays, surface deposits are found which may well have been represented in the remote past. In these we find alkali percentages comparable with the sedimentary soils, the potash ranging from 1.03 to 2.13, the soda from 0.57 to 1.63. The state of comminution is also remarkable.[4]

The interesting evidence of pre-Paleozoic granitic decay described by Dr. R. Bell, of the Canadian Geological Survey, and referred to by Merrill,[5] should be referred to by those interested in the question, although, as not being of a quantitative nature, the evidence does not, save for its general teaching, concern us here. Other cases of evidence for pre-Cambrian denudation are mentioned in the same treatise. Mr. Merrill concludes:

"These and other illustrations that might be given point unmistakably to the identity of geological processes and correspondence in results since the earliest times, even did not analogy and the thousands of feet of secondary rocks furnish us safe criteria upon which to base our inferences."

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[1] Geike, Text Book of Geology, third edition, p. 472.

[2] Possibly these mechanical effects may be accountable for the earlier forest vegetation possessing the morphological characters of that now clothing exposed and mountainous regions rather than those of the leafy trees of our valleys and plains. Its habitat, moreover, appears to have been the marsh and the sunken place.

[3] Merrill, loc. cit., p. 178.

[4] Loc. cit., p. 330.

[5] Loc. cit., pp. 275, 276.

Approaching finally the question as to whether a correction on the geological age of the earth previously arrived at is fairly due, according to our lights, on the score of the greater mass of detrital sediments now reposing on the land areas compared with those of the earliest times, we have, as we have seen in these very sediments, rocks of a physical character which forbids us to pronounce, in many cases, on the relative effectiveness of igneous and sedimentary rocks, as contributing to solvent denudation. We have also factors of both earlier and later times acting to accelerate solvent denudation. Of these the least speculative is the influence of vegetation, which is a post-Early-Paleozoic factor mainly. Again, the land uplifted from the primeval ocean, after the free acids were for the most part neutralized, was, we must infer, overlain with insoluble siliceous residues. To make any deduction or addition is not warranted. There appears no good reason to suspect that our broad uniformitarian principles are leading us into considerable error where, more especially, such disturbing causes as we are compelled to recognize are both of positive and negative signs. But the whole consideration should undoubtedly lead us to widen the margin we allow for error in our estimate of geological time.

VIII.—THE ALKALIES OF SEDIMENTS AND THE GEOLOGICAL AGE OF THE LATTER.

A very interesting but difficult line of inquiry is suggested in the probable facts of geological denudation which we have reviewed.

If the detrital sedimentaries of more recent geological age are derived, or in part derived, from preexisting sediments, we would anticipate that the detritals of successive periods should generally show diminishing alkali percentages. The inquiry is complicated by the necessity of observing that rocks of similar origin are in each case compared. The finer-grained sediments will be, as we have seen, the richest in alkalies, for the reason that the more soluble constituents of soils are just those which are reduced to the finest dimensions. Hence, when in the course of time the mechanical sorting of the river exerts such effects as the sieve of the investigator, the finer sediments laid down in sea or lake come to differ in their chemical nature from the coarser. Again, the percentage of soluble material in the soil may, as we have also seen, depend to some extent on the nature of the parent rock, and hence one soil may differ from another in the percentages of alkalies contained in the derived silts. However, by careful attention to the petrological and, above all, the physical character of the slates or clay slates we compare, some record of progressive change might be expected to be revealed.

Although our investigation labors under the difficulty that the existing records were not sought with a view to its prosecution, there are some broad indications of the evidence we seek which we are justified in referring to.

Let us look at the analyses of the "roofing slates." In these a certain fineness of grain and attendant similarity of history are probably in most cases involved. These are types mainly of the finest sediments. It does not appear that we have any reason to suppose that their deposition, consolidation, and prolonged existence in the rocks added to or subtracted from their original chemical constituents. With these we may probably compare clay slates of more modern periods and the finest muds now being laid down in estuaries and lakes.

Referring to Clarke and Hillebrand's collection of rock analyses,[1] we find sixteen analyses of roofing slates of Cambrian age from Vermont and New York. The mean percentage of added potash and soda alkalies is 5.05. In Rosenbusch (loc. cit., p. 425) the alkalies in a Welsh roofing slate are recorded as 5.38 per cent; a Cambrian clay slate of the Fichtelgebirge, 5.53 per cent; a Lower Silurian clay slate of the same region, 4.10 per cent, and a Silurian clay slate from Christiania, 5.60 per cent. These are otherwise mutually fairly concordant in chemical composition, and also concordant with those from the United States. The mean of all these affords 5.08 per cent of alkalies, the potash in each case exceeding the soda.

In this same table of Rosenbusch's we find a Devonian roofing slate, Erbstollen, with 3.04 per cent of alkalies. Three other Devonian slates, not named as roofing slates (Nos. 2, 3, and 4), show a mean of 3.54 per cent.

In the culm we find a roofing slate having 3.22 per cent; another culm roofing slate, 5 per cent, and an upper culm gray clay slate of the Fichtelgebirge, 2.99 per cent.

If we compare with these ancient sediments those now being deposited, we obtain the following figures: Bischof records 1.47 per cent of alkalies in the suspended matter carried down by the Rhine near Bonn.[2] Although this is fast-moving water, the general analyses otherwise closely resemble roofing slates and clay slates, as Bischof points out. The mud of the Nile near Cairo affords 1.96 per cent.[3] Merrill gives two analyses of fine muds washed by the sea into harbors and bays on the coast of North Carolina. They are fine, dark-colored muds brought down by the rivers and mixed with some decaying animal and vegetable matter. These contained 1.97 per cent and 2.17 per cent of alkalies; or, deducting all organic matter and water (which are temporary constituents), these numbers rise to 2.37 and 2.39 per cent. The mean given by these four modern silts and muds is 2.05 per cent of alkalies.

Without further investigation the facts recorded can only be advanced as suggestive.

[1] Bulletin U. S. Geological Survey, No. 148, 1897.

[2] Chemical and Physical Geology, p. 123.

[3] Loc. cit., p. 133.

IX.—THE SOLVENT DENUDATION OF THE OCEAN.

This subject, of course, closely concerns the matter discussed in this paper. To assume that no solvent action was exerted by sea water upon the coasts and the detrital remains continually being poured into it would, of course, be erroneous. We can only hope, in the present state of our knowledge, to find some clue as to the magnitude of the time allowance justified by marine solvent denudation.

In the first place, it is to be noticed that this denudation must be progressing chiefly along the immediate coast lines of the land areas. We can readily arrive at a rough estimate of the area involved. Measurement on a terrestrial globe shows that the coast lines of the continents and principal islands amount to 132×10^3 miles.[1] Much of this is rock bound. Along the rock-bound shores the rate of denudation apart from attrition is probably extremely slow. Soils can not here accumulate. Particles removed by attrition are carried out and quickly laid down in deep water. That the denudation here progressing is mainly mechanical is shown by the smooth surface of rock below water line. Limestones bordering the sea are often deeply pitted by the solvent action of the weather above high-water mark; beneath this line all is polished smooth.[2] Of course this does not show that no solution occurs. It merely connects the retreat and undercutting of seacoasts with the scouring action of hard silt in the water.

A large part of the coastal lines of the earth is, however, beach, where the waves are in perpetual motion and where the rounding of the larger stones more especially testifies to the activity of erosive action. But making no allowance for rock-bound coasts as a set-off against the neglect of the minor indentation of the shore line, and supposing the active motion of the waves to extend for a distance of 1,000 feet into the shallow water, we have an area of 25,000 square miles over which the sea is in active motion.

It is evident that even a very considerable rate of solution over this area would bear but a small proportion to that progressing over the forty-four millions of square miles exposed to chemical actions for a large part far more active than is exerted by sea water and generally in material finer in grain.

This last point may be considered set at rest by the experiments of Daubrée. Inclosing 3 kilograms of feldspar in fragments along with water containing 3 per cent of chloride of sodium in the rotating cylinders used in his well-known experiments, and making all the conditions the same as those obtaining in his experiments in which fresh water was used, he could not obtain, either in a vessel of iron or of

[1] Croll, allowing for bays and inlets and the smaller islands, estimates the cost line at 116×10^3 miles. Wallace takes 100×10^3 miles. See Island Life, p. 221.

[2] In the neighborhood of Dublin—at Donabate—this is clearly shown.

stoneware, any alkaline reactions except the most feeble: "Et incomparablement moindre que celle qui se manifeste dans l'eau distillée." The presence of the chloride of sodium appeared to arrest the decomposition.[1] To this inactive nature of sea water the prolonged preservation of feldspathic fragments on sea beaches has been ascribed.

There is interesting evidence bearing in this direction to be derived from the deep-sea deposits. The volcanic débris, whether wind or water borne, must be in a fine state of comminution in order to reach the central oceanic deposits.[2] Such particles must sink with extreme slowness through depths measured by miles. Their subsequent sojourn upon the bottom is of unknown duration. Yet it is remarkable that when these deposits are analyzed the alkali ratio is that of the igneous, not that of the sedimentary, rocks. This is a plain proof that the waters of the ocean do not affect them as would terrestrial rains and rivers.

Thus we find a deep-sea ooze from 5,422 meters deep between New Zealand and Tahiti to contain 4.92 per cent of Na_2O and 2.82 per cent K_2O. Another from a depth of 4,956 meters west of the Society Islands gave 1.83 per cent Na_2O and 1.74 per cent of K_2O.[3] In Murray and Renard's report of the *Challenger* results, it is suggested that some of this volcanic débris may come from submarine sources. In any case the pumice and glass of the ocean floor, even when decomposed, retain their igneous alkali ratio. Thus andesitic pumice contained Na_2O 2.34, K_2O 1.61 per cent; basaltic pumice, Na_2O 2.81 and K_2O 1.24 per cent. Other concordant examples are given.

Are we to make a correction for oceanic denudation? Are the solvent effects of a magnitude which would result in a noticeable fraction of our estimate of geological time being in excess? If we supposed that the solvent effect of the waves acting on the full coast line of the earth were not less, not even equal, but ten times as great as what is continuously progressing in an equal area of the soils, the disproportionality of areas reduces its present solvent effects to one one-hundred and seventy-seventh of the effectiveness of the land in supplying soluble materials to the sea. This would then be a correction of half a million of years on the time estimate.[4]

In the coastal effects of to-day this correction would be almost certainly excessive. To these effects must be added those progressing on the immense quantities of fine silt which the rivers pour annually into the oceans, and which has been estimated by Sir J. Murray as 2.5 cubic miles of sediment. Much of this rapidly finds a quiet resting place, and probably nearly perfect preservation, near the coasts. The remainder, borne into deeper water, must yield something to the ocean.

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[1] Géologie Experimentale, I. p. 275.

[2] See Wallace's Darwinism, p. 363, for facts as to these dimensions.

[3] Rosenbusch, loc. cit., p. 420.

[4] See also Island Life, p. 225, footnote.

We have, as we have seen, evidence that this may not be much. Possibly the half million years would more than cover the entire solvent effects of the ocean.

We have to consider, indeed, in this matter that the ocean was not always charged with its present dissolved salts. The primeval ocean, most probably after the free acids were satisfied in the solution of the silicates, carried chiefly chlorides indeed, but chlorides of lime, magnesia, and other metals. The subsequent changes were those of replacement for the greater part. We have no reason, however, to suppose that these salts could act substantially differently from the chlorides of sodium now constituting the larger part of the chlorides.[1]

We can only, from what we know, gather some idea of the order of magnitude of the correction for oceanic solvent denudation. It appears almost certain that this can not exceed a very few million years.

The allowances we felt justified in making in the earlier part of this paper left our estimate at eighty-nine millions of years. The least speculative part of our knowledge inclines us to believe that this is probably a major limit.[2] Taking into account our uncertainty in many particulars attending these corrections, and as to the constancy throughout the past of solvent denudation, and bearing in mind that any approximation to a correction for marine denudation must be attended with this same uncertainty, but that the latter correction will undoubtedly be subtractive, we think that it is at least justifiable to claim that our present knowledge of solvent denudation of the earth's surface points to a period of between eighty and ninety millions of years having elapsed since water condensed upon the earth, and rain and rivers and the actions continually progressing in the soils began to supply the ocean with materials dissolved from the rocks.

Appendix I.

To facilitate review of the numerical quantities adopted in the calculations involving the age of the earth, the chief data are here collected:

Area of land (Murray and Wagner)	55,814,000 square miles.
Ratio of oceanic to land area	2.54 : 1.
Hence, oceanic area	$141{,}767 \times 10^3$ square miles.
Mean depth of ocean (Murray)	2,076 fathoms = 2.393 miles.
Bulk of ocean	$339{,}248 \times 10^3$ miles.
Mass of a cubic mile of sea water	43×10^8 tons.

[1] A. Agassiz thinks the solvent power of the ocean during some of the earlier geological deposits was far less than during later times. See Three Cruises of the Blake, I, p. 147.

[2] See the summary of positive and negative errors contained in Appendix II, and, more especially, set off 1 and 2 of the errors going to make the estimate a maximum against 1 among those tending to render it a minimum.

Mass of ocean	1.460×10^{18} tons.
Mass of NaCl in ocean	$39,782 \times 10^{12}$ tons.
Mass of Na in ocean	$15,627 \times 10^{12}$ tons.
Mass of Cl combined with Na in ocean	$24,155 \times 10^{12}$ tons.
Mass of K_2SO_4 in ocean	$1,260 \times 10^{12}$ tons.
Mass of K in ocean	565×10^{12} tons.
Mass of $MgCl_2$ in ocean	$5,568 \times 10^{12}$ tons.
Mass of Cl combined with Mg in ocean	$4,161 \times 10^{12}$ tons.
Annual river discharge into oceans	6,524 cubic miles.
Mass of sodium in a cubic mile of river water	24,106 tons.
Annual river supply of Na	$15,727 \times 10^{4}$ tons.
Annual (calculated) Na_2O discharge of rivers	21×10^{7} tons.
Annual (calculated) K_2O discharge of rivers	7.3×10^{7} tons.
Estimated bulk of siliceous sedimentary detrital rock	=layer 1.1 mile thick over land.
Soda percentage of primitive rock	3.61.
Potash percentage of primitive rock	2.83.
Mean soda percentage of sedimentaries	1.47.
Mean potash percentage of sedimentaries	2.49.

Appendix II.

The errors possibly affecting the foregoing method of estimating geological time are of both signs, and are here enumerated.

Those tending to render the estimate a minimum are:

1. The abstraction of sodium chloride from the ocean by evaporation of sea water in bays or inlets cut off from the sea.
2. The deposition of sodium chloride as a constituent of submarine sediments and deposits.
3. Diminished meteorological activity in the past arising from diminished solar heat, very different distribution of land and water, glacial periods, or other causes.
4. Underestimate of the supply of sodium chloride to the rivers by rainfall.
5. Diminished river supply of sodium in the past due to lithological differences in rocks and soils exposed to denudation or diminished amounts of organic acids, etc.
6. Underestimate of the mass of sodium now in the ocean or overestimate of that delivered in the river supply to the ocean.
7. Overestimate of sodium supplied to the ocean by a probable primeval accelerated denudation.

Those tending to render the estimate a maximum are:

1. The supply of sodium to the ocean by direct marine solution of coast materials and sediments.
2. Certain sources of supply of chloride of sodium to the sea otherwise than by normal river supply, as volcanic emissions, denudation of inland rock salt deposits into the ocean by brine springs, etc.
3. Increased meteorological activity in the past arising from very different distribution of land and water, glacial periods, or other causes.
4. Overestimate of the supply of chloride of sodium to the rivers by rainfall.
5. Increased river supply of sodium in the past due to lithological differences in the rocks and soils exposed to denudation or to chemical effects of carbonic acid in rain or river water, etc.
6. Overestimate of the mass of sodium now in the ocean or underestimate of that delivered in the river supply to the ocean.
7. Underestimate of sodium supplied to the ocean by a probable primeval accelerated denudation.

THE PETRIFIED FORESTS OF ARIZONA.[1]

By LESTER F. WARD.

December 12, 1899.

SIR: In compliance with your instructions dated October 9, 1899, directing me "to visit what is known as the 'Petrified Forests of Arizona,' and, upon your arrival in Washington, render a detailed report of your investigations and observations concerning the same, including such information as may be of value touching the proposition to set aside the region embracing the Petrified Forests as a national park," I have the honor to make the following report:

In order to place the subject in as clear a light as possible, I will first give a brief historical account of the recent movement in favor of making a public reservation of the region embracing the Petrified Forests of Arizona.

In 1895 the legislative assembly of the Territory of Arizona adopted the following memorial to Congress:

HOUSE MEMORIAL No. 4.

To the Senate and House of Representatives of the United States of America in Congress assembled:

We, your memorialists, the eighteenth legislative assembly of Arizona, beg leave to represent to your honorable bodies:

First. That there is in the northern part of this Territory, lying within the borders of Apache County, near the town of Holbrook, a wonderful deposit of petrified wood commonly called the "Petrified Forest" or "Chalcedony Park."

This deposit or forest is unequaled for its extent, the size of the trees, and the beauty and great variety of coloring found in the logs.

The country 10 miles square is covered by the trunks of trees, some of which measure over 200 feet in length and from 7 to 10 feet in diameter.

Ruthless curiosity seekers are destroying these huge trees and logs by blasting them in pieces in search of crystals, which are found in the center of many of them, while carloads of the limbs and smaller pieces are being shipped away to be ground up for various purposes.

Second. Believing that this wonderful deposit should be kept inviolate, that future generations may enjoy its beauties and study one of the most curious and interesting effects of nature's forces,

[1] Reprint of Report on the Petrified Forests of Arizona, by Prof. Lester F. Ward, to the Director of the U. S. Geological Survey, published by the Department of the Interior. Further removal of the fossil trees has been restricted under regulations of the Interior Department.

We, your memorialists, most respectfully request that the Commissioner of the General Land Office be directed to withdraw from entry all public lands covered by this forest until a commission or officer appointed by your honorable bodies may investigate and report to you upon the advisability of taking this forest under the charge of the General Government and making a national park or reservation of it.

It is annually visited now by hundreds of scientific men and travelers from every State and country, and some such action by your bodies would preserve it from the vandalism it is now subjected to.

We would further state that at present there is no person living within the limits of the proposed park, so that no settlers will be disturbed by any such action on your part.

And be it resolved by the legislative assembly of the Territory of Arizona, That our Delegate in Congress be, and is hereby, instructed to use all honorable means to have some action taken by Congress to have this Chalcedony Park set aside and formed into a national park under the care and charge of the General Government.

Also that the secretary of the Territory be, and is hereby, requested to transmit a copy of this memorial to each House of Congress, our Delegate to Congress, and the United States Land Commissioner.

J. H. CARPENTER, *Speaker*.
A. J. DORAN, *President*.

[Indorsed.]

I hereby certify that the within memorial originated in the House and is known as House Memorial No. 4.

CHAS. D. REPPY, *Chief Clerk*.

Filed in the office of the secretary of the Territory of Arizona this 11th day of February, A. D. 1895, at 11 a. m.

CHAS. M. BRUCE, *Secretary of Arizona*,
By F. B. DEVEREUX, *Assistant*.

In June last the secretary of the Smithsonian Institution received the following letter from the honorable Commissioner of the General Land Office:

PETRIFIED FOREST, ARIZONA.

DEPARTMENT OF THE INTERIOR, GENERAL LAND OFFICE,
Washington, D. C., June 17, 1899.

SIR: I am in receipt of a certified copy of a memorial by the legislature of Arizona praying that certain lands in Apache County, Arizona, in the vicinity of the town of Holbrook, known as the "Petrified Forest," be withdrawn from entry with a view to creating a reservation or national park for the purpose of preserving the natural wonders and curiosities of the same.

I have the honor to request that you will kindly inform me whether the records of the Smithsonian Institution furnish any information respecting this locality indicating that the scenic features of the same are of such a nature as to render it desirable, in the interest of the public, to set these lands apart as a national park. I will be pleased to receive a full expression of your views on this subject, and also as to the importance of preserving the mineralized formations in that region.

Very respectfully,

BINGER HERMANN,
Commissioner.

The SECRETARY OF THE SMITHSONIAN INSTITUTION.

PETRIFIED FORESTS OF ARIZONA.

To this letter the following reply was made:

SMITHSONIAN INSTITUTION,
Washington, D. C., July 7, 1899.

SIR: I have the honor to acknowledge the receipt of your communication of the 17th ultimo requesting information concerning the Petrified Forest near Holbrook, in Arizona, as well as an expression of opinion concerning the desirability of setting aside these lands as a national park, and beg to furnish the following statement:

The region near Holbrook, Apache County, Arizona, known as the "Petrified Forest," "Chalcedony Park," and "Lithodendron (stone trees) Valley," is of great interest because of the abundance of its beautiful petrified coniferous trees, as well as of its scenic features. The trees lie scattered about in great profusion, but none stand erect in their original place of growth as do many in the Yellowstone National Park. The National Museum possesses three splendid trunks collected there by Lieutenant Hegewald at the request of General Sherman.

The best popular account of this region is given by Mr. George F. Kunz, and is as follows:

"Among the great American wonders is the silicified forest, known as Chalcedony Park, situated about 8 miles south of Carrizo, a station on the Atlantic and Pacific Railroad, in Apache County, Arizona. * * * The locality was noticed in 1853 by the Pacific Railroad Exploring Survey. * * * There is every evidence to show that the trees grew beside some inland sea. After falling they became water-logged, and during decomposition the cell structure of the wood was entirely replaced by silica from sandstone in the walls surrounding this great inland sea.

"Over the entire area, trees lie scattered in all conceivable positions and in fragments of all sizes, the broken sections sometimes resembling a pile of cart wheels. * * * A phenomenon perhaps unparalled and the most remarkable feature of the park is a natural bridge formed by a tree of agatized wood spanning a canyon 45 feet in width. In addition to the span, fully 50 feet of the tree rests on one side, making it visible for a length over 100 feet."

Lieutenant Hegewald writes:

"I rode down the valley to examine the thousands of specimens that lay scattered on each side of the valley along the slopes, which were perhaps 50 feet high; the valley of the Lithodendron, at its widest part, being scarcely a half mile. Along the slopes no vegetation whatever was to be seen, wood being very scarce; the soil was composed of clay and sand mostly, and these petrifactions, broken into millions of pieces, lay scattered all adown these slopes. Some of the large fossil trees were well preserved, though the action of heat and cold had broken most of them in sections from 2 to 20 feet long, and some of these must have been immense trees; measuring the exposed parts of several they varied from 150 to 200 feet in length, and from 2 to 4½ feet in diameter, the centers often containing most beautiful quartz crystals."

Dr. Walter Hough, of the Smithsonian Institution, who has visited the park, writes as follows:

"In the celebrated Petrified Forest, which is some 18 miles from Holbrook, Arizona, on the picturesque Santa Fe Railroad, there are ruins of several ancient Indian villages. These villages are small, in some cases having merely a few houses, but what gives them a peculiar interest is that they were built of logs of beautiful fossil wood. * * * The prehistoric dwellers of the land selected cylinders of uniform size, which were seemingly determined by the carrying strength of a man. It is probable that prehistoric builders never chose more beautiful stones for the construction of their habitations than the trunks of the trees which flourished ages before man appeared on the earth.

"This wood agate also furnished material for stone hammers, arrowheads, and knives, which are often found in ruins hundreds of miles from the forest."

This "wood agate" or "wood opal" is now cut and polished into floor tiling, mantels, clock cases, table tops, paper weights, etc. The silver testimonial to the French sculptor Bartholdi, made by Tiffany & Co., had for its base a section of this wood agate.

Prof. Lester F. Ward, an eminent paleobotanist, who, while officially attached to the staff of the United States Geological Survey, also holds the position of associate curator in the National Museum, expects to visit the Pacific coast this summer, and may return by the southern route. He tells me that if you so desire he would be pleased to visit the region in question for the special purpose of procuring further information regarding the features covered by your inquiry.

In conclusion I would say that all with whom I have consulted are agreed that the "Petrified Forest," or "Chalcedony Park," of Apache County, Arizona, should be preserved as a public park for the benefit of the American people. In no other area is there such a profusion of highly colored stone trees. Fossil wood is scattered over a very great area of Arizona, but the densest portion and chief place of interest is "Chalcedony Park," an area of less than 5 miles square. This region is about 20 miles south of Carrizo station.

A list of papers relating to the Arizona forest trees is appended.

Very respectfully,

RICHARD RATHBUN,
Acting Secretary.

Hon. BINGER HERMANN,
Commissioner General Land Office,
Department of the Interior,
Washington, D. C.

Growing out of the paragraph containing my offer to visit the locality, and a personal call at the General Land Office, the Hon. W. A. Richards, assistant commissioner, in the absence of the Commissioner, wrote me as follows:

DEPARTMENT OF THE INTERIOR, GENERAL LAND OFFICE,
Washington, D. C., August 19, 1899.

DEAR SIR: As requested by you yesterday, I have written a letter to Hon. Charles D. Walcott, Director of the Geological Survey, requesting that you be instructed to visit the "Petrified Forests" of Arizona, in order that you may make report as to the advisability of setting that locality apart as a national park.

I also have had prepared a copy of the memorial of the Arizona legislature relating to the subject, which I inclose herewith.

Very truly, yours,

W. A. RICHARDS.

Prof. LESTER F. WARD,
Paleontologist, Geological Survey.

Owing to your absence in Canada and my departure for the Pacific coast, where I remained during September and October, I did not receive your instructions, above quoted, until the middle of October. At about the same time I received the following letter from the secretary of the Smithsonian Institution:

SMITHSONIAN INSTITUTION,
Washington, October 13, 1899.

The Smithsonian Institution takes pleasure in introducing to its friends Prof. Lester F. Ward, Paleontologist of the United States Geological Survey, and associate curator

in the United States National Museum, who visits the region of the fossil forests in Arizona at the instance of the Hon. Binger Hermann, Commissioner of the General Land Office, with a view to obtaining information for the use of the Commissioner in connection with a request of the legislature of Arizona that certain lands in the vicinity of Holbrook, known as the "Petrified Forest," be set aside for a national park.

Any courtesy which may be extended to him will be duly appreciated by the Institution.

S. P. LANGLEY, *Secretary*.

Equipped with these instructions and credentials I left San Francisco on November 1, 1899, and proceeded direct to Arizona. After a week of general investigation in the western part of the Triassic terrane, I arrived at Holbrook on the 9th and entered the special area of the petrified forests on the 10th. I went over the ground with considerable thoroughness and visited about all the localities of interest, taking full notes of the scenic, geologic, and scientific features.

SCENIC FEATURES.

With regard to the first of these, viz, the scenic aspect, I can safely say that it has never been exaggerated by any who have attempted to describe this region. The pictures given in the letter of the Assistant Secretary of the Smithsonian Institution, above quoted, are not overdrawn, and the more or less glowing descriptions of Möllhausen, Marcou, Newberry, and other early explorers fall far short of what might be truly said from this point of view. These petrified forests may be properly classed among the natural wonders of America, and every reasonable effort should be made not only to preserve them from destructive influences but also to make their existence and true character known to the people.

Some of the most important considerations that may be urged in favor of the importance of this region compared with other petrified forests rest upon its geological relations. In the first place, it is much more ancient than the petrified forests of the Yellowstone National Park, of certain parts of Wyoming, and of the Calistoga deposits in California. These latter are of Tertiary age, while the Arizona forests belong far back in Mesozoic time, probably to the Triassic formation. The difference in their antiquity is therefore many millions of years. Scattered blocks of silicified wood do indeed occur in the Trias at other points, but this is the only region in which they are in such abundance as to deserve the name of a petrified forest.

In the second place, there is no other petrified forest in which the wood assumes so many varied and interesting forms and colors, and it is these that present the chief attraction for the general public. The state of mineralization in which much of this wood exists almost places them among the gems or precious stones. Not only are chal-

cedony, opals, and agates found among them, but many approach the condition of jaspar and onyx. The degree of hardness attained by them is such that they are said to make an excellent quality of emery.

Perhaps the most prominent of all the scenic features of the region is the well-known Natural Bridge, consisting of a great petrified trunk lying across a canyon and forming a footbridge over which anyone may easily pass. For reasons that will be obvious, the full treatment of this feature is deferred to a more appropriate place.

LOCATION OF THE PETRIFIED FORESTS.

It should be understood that petrified or silicified wood occurs in great quantities throughout the Triassic terrane of Arizona, New Mexico, and Utah, and there are hundreds of places where the logs are massed together or even piled one upon another; but the particular region known as the "Petrified Forest of Arizona" lies in the area between the Little Colorado and the Rio Puerco, 15 miles east of their junction, 17 miles east of Holbrook, and 6 miles south of Adamana station on the Santa Fe Pacific Railroad, which measurements terminate at the outer edge of the area on the west and north sides. It is about 8 miles square, and falls chiefly within township 17 north, range 24 east, but extends a short distance on the south into township 16 north, and on the west into range 23 east.

This region consists of the ruins of a former plain having an altitude above sea level of 5,700 to 5,750 feet. This plain has undergone extensive erosion to a maximum depth of nearly 700 feet, and is cut into innumerable ridges, buttes, and small mesas, with valleys, gorges, and gulches between. The strata consist of alternating beds of clays, sandstone shales, and massive sandstones. The clays are purple, white and blue, the purple predominating, the white and blue forming bands of different thickness between the others, giving to the cliffs a lively and pleasing effect. The sandstones are chiefly of a reddish brown color and closely resemble the brownstone of the Portland and Newark quarries, or the red sandstone of the Seneca quarries on the Potomac River and at Manassas in Virginia, but some are light brown, gray, or whitish in color. The mesas are formed by the resistance of the massive sandstone layers—of which there are several at different horizons—to erosive agencies, and vary in size from mere capstones of small buttes to tables several miles in extent, stretching to the east and to the northwest.

The drainage of the area is to the south, and in the middle of it, having a nearly due southern course, but winding much among the buttes, is the arroyo which has been mistaken for the famous Lithodendron Creek, so named by Lieutenant Whipple in 1853.[1] It is dry most of the year, but has a gravelly bed, often 20 feet in width, and

[1] See Twentieth Annual Report U. S. Geological Survey, Part II, p. 324.

PETRIFIED FORESTS OF ARIZONA.

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if holes are dug in this gravel to a depth of 4 or 5 feet water will accumulate and stand in them.

The valley of this creek is narrow in the northern and central parts of the area, and there are several short branches or affluents, but at the southern end it broadens out, and its rugged, spurred, and canyoned slopes are highly picturesque. Here is located its principal petrified forest, and this is the region that has been characterized by some as Chalcedony Park. The petrified logs are countless at all horizons and lie in the greatest profusion on the knolls, buttes, and spurs, and in the ravines and gulches, while the ground seems to be everywhere studded with gems, consisting of broken fragments of all shapes and sizes and exhibiting all the colors of the rainbow. When we remember that this special area is several square miles in extent some idea can be formed of the enormous quantity of this material that it contains.

Although much fossil wood occurs throughout the whole region, as above delimited, still for several miles to the north of this Chalcedony Park it is less abundant, and it is not until the northern end of the area is reached that another center of accumulation occurs. This lies between two mesas in a valley that opens out upon the general plain which stretches north to the Rio Puerco. It is much smaller in extent than the southern park, but substantially the same general features are presented.

There is still a third center of accumulation, called the "middle forest," which lies some 2 miles southeast of this last, and extends to the eastern margin of the general region. It occupies the western slope of the table-land on the east, and is very extensive, stretching a mile or more in a north and south direction and having a width of half a mile in places. It presents many interesting novelties.

GEOLOGICAL CONSIDERATIONS.

All the petrified forests thus described are, geologically speaking, entirely out of place, and the trunks bear every evidence of having dropped down to their present position from a higher horizon in which they were originally entombed and from which they have been subsequently washed out. Nor is their original position to be discovered by ascending the several mesas included in the area, although some of these rise 400 feet above the bed of the above-mentioned creek. It is not until the still higher plateau is reached which bounds the whole region and lies more than 700 feet above the valley that the stratum is at last found which actually holds the fossil wood. A geologist might therefore traverse the entire area from north to south, visit all three of the principal forests, and go out with the impression that everything was out of place and with no correct idea of the true source of the fossil

wood. Even on the east it would be difficult to settle this question on account of the paucity of the trunks in that direction, but it could doubtless be done by prolonged and careful search. On the west side, however, and directly west of the southernmost area, the plateau is only about 2 miles wide and has a western escarpment with another valley extending both south and west of it. This plateau or elongated mesa is highest on its western side, rising to the 5,750-foot contour line immediately above the escarpment, and here is exposed a fine series of petrified trunks fringing the mesa, with many weathered out on the slope or rolled down into the valley below. A few feet below the actual summit is a bed some 20 feet thick of coarse, gray, conglomeratic, cross-bedded sandstone, at many places in which were found firmly embedded logs and branches of the petrified wood, often projecting from it in the cliffs and clearly in place. This, then, is the true source of the fossil wood, and after several days study on all sides of the area I became convinced that no other layer holds any of it, at least in this region.

This bed was found at nearly all points where the requisite elevation can be attained, but the petrified logs do not occur in the same abundance throughout. They are massed or collected together in groups or heaps at certain points, and may be altogether absent at others. From their great abundance in the three areas above described, which may be called the upper, lower, and middle forests, respectively, but in all of which they are out of place and lie several hundred feet below their proper position, it must be inferred that the stratum which holds them was especially rich, and the trunks must have lain in heaps upon one another. This bed may have been considerably thicker in these areas than it is farther out on the margins, where it is now found in place.

Only at two points within the general petrified forest area did I find remnants of this bed which had not been broken down and disintegrated. One of these is at the extreme northern end, half a mile northeast of the upper forest. Here there is a small mesa, which lies at an elevation of nearly 5,700 feet, or about 400 feet above the valley which contains the upper forest. It is isolated and its nearly flat top, which is approximately circular, is about half a mile in diameter. The coarse conglomeratic sandstone stratum, 20 to 30 feet in thickness, occupies the summit of this mesa and is often hardened into rock, but in all essential respects it is identical with that of the elongated mesa on the southwest side of the area above described. The petrified wood is less abundant here, but sufficiently common, and is embedded in and often projects from the sandstone ledges.

The Natural Bridge.—Besides the fact that this bed lies wholly within the petrified forest area, there is another important circumstance which serves to give it special prominence. One of the most celebrated objects in this entire region is the well-known "Natural

NATURAL BRIDGE IN PETRIFIED FORESTS OF ARIZONA.

Bridge," mentioned by so many travelers and referred to in the documents quoted at the beginning of this report, consisting of a great petrified trunk lying across a canyon and forming a natural footbridge on which men may easily cross. This occurs on the northeast side of the above-mentioned mesa near its rim, and the bed in which it lies is the coarse sandstone which holds all the petrified wood. The Natural Bridge, therefore, possesses the added interest of being in place, which can be said of very few of the other petrified logs of this region.

It was observed in the southwestern exposure and at other points that all the petrified logs and blocks lying in the sandstone or only recently washed out of it are surrounded by a coating of the sandstone firmly cemented to the exterior. The absence of this coating from most of those in the principal forests is due to their long exposure to climatic influences which ultimately disintegrate and detach the sand rock adhering to them and strip them clean to the body of the trunks themselves. That this process requires ages of time is proved by the fact that the Natural Bridge is still coated over a large part of its surface by the remains of the cemented sand rock in which it was once completely imbedded. This is true chiefly of the lower portion, and farther up the trunk it has nearly all disappeared. The trunk is in an excellent state of preservation and is complete to the base, where it is abruptly enlarged and shows the manner in which the roots were attached. This portion still lies partially buried in the sandstone, which is the same in character as that which still adheres to the lower 20 feet. The canyon or gulch has a due north direction and is very precipitous, beginning only 200 yards above the bridge and rapidly broadening in its descent. At the point where the bridge crosses it is about 30 feet wide, but the trunk lies diagonally across and measures 44 feet between the points at which it rests on the sides of the canyon. The angle is nearly 45°, and the tree lies with its roots to the southeast and its top to the northwest. The canyon is here about 20 feet deep, and from its bottom and slopes several small trees are growing, some of which rise considerably above the bridge. The trees are mostly cedars, but there is one cottonwood (*Populus angustifolia*). The root is quite near the brink of the canyon, but rests on a solid ledge for a distance of 4 feet, so that there is no probability that in this dry region it will be endangered by further erosion. The total length exposed is 111 feet, so that more than 60 feet of the upper part lie out on the left bank of the canyon. At about the middle of the canyon, and above where the coating of sandstone still adheres, it measures 10 feet in circumference, giving a diameter of over 3 feet. At the base it is now 4 feet in diameter, but the thickness of the incrustation is not exactly known. At the extreme summit the diameter is reduced to 18 inches. As in the case of practically all the petrified logs of the region, there

are no indications of limbs or branches at the top. The significance of this fact will be noted later.

A conspicuous characteristic of all the petrified trunks, not only of this area and of the general Triassic terrane of Arizona and New Mexico but of all petrified forests, is their tendency to break across into sections or blocks of greater or less length. All travelers have remarked this, and the sketches given by Möllhausen and in the Pacific Railroad Reports show them thus divided. Some observers have noted the fact that the Natural Bridge has several of these transverse cracks, and all the good photographic views of it show them. I counted four, but most of them seem to be as yet only partial and do not probably extend entirely through the trunk. There is one, however, near the left bank of the canyon which has the appearance of doing so, and the trunk is probably only kept from parting at this point by the mechanical adjustment which causes the adjacent faces to perform the office of a keystone to an arch. Any considerable shrinkage due to climatic or other causes would overcome this influence and the entire bridge would crash to the bottom of the canyon and roll down the escarpment in a number of huge segments.

An examination of the relations of the Natural Bridge to the gulch which it spans shows clearly that the trunk was primarily entombed in the sandstone bed covering this entire region, and that, with the progress of erosion which ultimately carried away the entire plain to the north, as well as in other directions, leaving this small mesa, it was at last exposed, and lay for a great period near the rim of the escarpment. At first it was only partially buried and later came to lie on the surface of the ground. As the land rises somewhat to the south of it, rills were formed above, and in times of floods or heavy rain it obstructed the flow of the water, forming a sort of dam. The water lying against it long after it had ceased to overflow it, tended to disintegrate the rock upon which it lay, until eventually it found its way through beneath it at some one point. The smallest opening of this nature would soon become a free passage for the water, and a simple continuation of this process of local erosion would ultimately result in the formation of the entire gorge as it exists to-day.

The other case which I observed of the presence of the conglomeratic sandstone within the general petrified forest area occurs near its center, about midway between the upper and lower forests along the narrow portion of the valley of the creek described above, on both sides of the canyon and near the level of its bed, at an altitude of about 5,300 feet. The exposure was typical in all respects, and logs were seen projecting from the canyon walls, from one of which specimens were collected. As this exposure is 400 feet below that in which the Natural Bridge occurs and 450 feet below that on the southwestern mesa, its presence there can be accounted for only on one of two hypotheses, either that of the

xistence of another exactly similar stratum at this horizon or that of fault, or, what would amount to the same thing, a slide or slipping own of a large block of the uppermost beds in such a manner as not o disturb their stratigraphical arrangement.

The first of these hypotheses is rendered improbable by the fact that a careful study of the beds at the same horizon in other places revealed no such stratum, and it could scarcely be so local as not to be found elsewhere. The second hypothesis seems every way probable, as in such a much-disturbed region it would be easy for the erosive agencies to undermine a small outlier or mesa and cause it to sink down intact to a lower level. The question, however, requires more detailed investigation than I was able to give to it.

Leaving this phenomenon out of the account, therefore, and considering the two exposures, in which there is no question as to their natural position, we may use them as a means of determining whether the strata have any dip, and to some extent in ascertaining the amount and direction of the dip. The topographic map has a 250 feet contour interval, which is too large to be employed with any very great accuracy, and an aneroid can hardly be depended upon for measurements made six hours apart, as had to be done in this case, but as nearly as I could judge from all sources of information the Natural Bridge mesa seems to be between 50 and 100 feet lower than the southwestern mesa. As the distance from the one to the other is about 5 miles, the dip to the northeast is somewhere between 5 and 10 feet to the mile. As, however, the strike was not accurately determined, there is no certainty that this is the true dip of the strata, and more precise observations on a much larger scale will be necessary to settle this question.

Although there is no longer any question as to the true stratigraphical position of these profuse vegetable remains, there are many facts which stand in the way of the supposition that the trees actually grew where we now find them. Several accounts[1] profess that stumps occur erect with their roots in the ground, showing that they grew and were buried and petrified on the spot, but I was unable to confirm any such observations, and on careful inquiry of residents of the country, who had minutely examined every part of the area, I was unable to learn of a single indisputable instance of such an occurrence. The only trunk that I saw standing on end was one that was inverted and had its roots high in air! In fact, from the nature of the case, as I have just shown, there would be no use looking for any such phenomenon in any of the principal fossil forests, since they all lie from 100 to 400 feet below where they were originally deposited. It is only in the

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[1] Tagebuch einer Reise vom Mississippi nach den Küsten der Südsee, von Balduin Möllhausen. Leipzig, 1858, p. 300.

Résumé explicatif d'une carte géologique des États-Unis et des provinces anglaises de l'Amérique du Nord, etc., par Jules Marcou. Bulletin de la Société Géologique de France, 2e sér., Vol. XII, 1855, p. 871. Repeated in Geology of North America, etc., Zurich, 1858, p. 13.

beds of coarse sandstone that hold them, therefore, that the evidence need be sought. This I did with the utmost care, but even here I found no example of an upright trunk.

In this, as I was glad to learn after my return on looking the matter up, I was only confirming the observations of Dr. J. S. Newberry, made in 1858 and published in 1861.[1]

Although it is easy to find petrified limbs and small twigs among the other objects, still these occur sporadically and accidentally at any and all points. They are no more likely to be found beyond the termination of the tall trunks than anywhere else, as would be the case if the trees lay near where they grew. In fact, it happened that I never found small twigs in this position, although I searched in hundreds of cases. I found no petrified cones, but I heard vague reports of their having been found. It would be strange if none were preserved in such a vast mass of trunks of cone-bearing trees.

Finally the great abundance of the material would seem to negative the idea that it could have all grown on the same area. Even if every tree had been preserved, there are places where it would have been impossible for them to stand as thickly as they lie on the surface, not to speak of the space that trees in a forest require in order to thrive, as these trees evidently did thrive. And while there is now no place where they lie so thickly in the original bed of sandstone, still, even here they are not only all prostrate, but lie in little collections and huddles quite differently from what should be expected if they were precisely where they grew.

The preservation of a forest in situ with the trunks erect could scarcely take place except by some sudden, commonly eruptive agency. Such agencies have undoubtedly operated in the preservation of the petrified forests of the Yellowstone Park and of others that I have visited in Wyoming and elsewhere, in which the stumps and sometimes tall trunks do stand in position with their roots in the ground, but in the region under consideration there are only faint indications of eruptive agencies, certainly not sufficient to account for the phenomena.

The indications therefore all point to some degree of transportation of this material by water antecedent to petrifaction, and the great

[1] Report upon the Colorado River of the West, explored in 1857 and 1858, by Lieut. Joseph C. Ives, Washington, 1861, 4°. Part III. Geological Report, by J. S. Newberry, p. 80.

Dr. Newberry's statement is as follows:

"I examined these specimens with some care to determine, if possible, whether they had grown on the spot, as those of Lithodendron Creek are supposed to have done by the members of Captain Whipple's party, or whether they had been transported to their positions. In all that came under my observation, I failed to find any evidence that they had grown in the vicinity. All the trunks are stripped of their branches and exhibit precisely the appearance of those transported to some distance by the agency of water. In confirmation of this view I should also say I found in the marls, with the entire trunks, rounded and water-worn fragments of wood, in some instances silicified and in others converted into lignite.

I gathered the same impression from all the collections of silicified wood which I observed in this formation in western New Mexico, viz: that all had been *transported*, but not *far* removed from their place of growth."

amount of it at this particular place argues for the existence of such a condition as would arrest the process and cause the floating logs to accumulate in masses, as often happens in great eddies or the deltas of rivers. The character of the bed in which they occur further supports this view. The coarse sand and gravel, highly favorable to the process of silicification, denotes the proximity of the land, and the crossbedding bears witness to the existence of rapid and changing currents. As this stratum occupies the highest elevations in this region, the nature of the overlying beds is not revealed, and the question whether the period was followed by one of general subsidence can only be settled by a study of the higher plains lying some distance to the east and north, but it is probable that the bed sank and that finer deposits ultimately buried it at the bottom of the Mesozoic sea, there to remain until the Tertiary epeirogenic movement raised the entire country from 5,000 to 6,000 feet above sea level.

PRESERVATION OF THE PETRIFIED FORESTS.

It will be obvious from the above that the Petrified Forests of Arizona constitute an object of interest to all people of culture from both the æsthetic and the scientific points of view, and that the immediate region here considered embraces the most striking features that they anywhere present. As stated in the memorial of the Territorial legislature to Congress, and as confirmed by my inquiries and admitted by all, these natural wonders are attracting thousands of visitors annually, most of whom are drawn there by mere curiosity. This characteristic of human nature, however aimless it may sometimes seem, and however destructive it may often be, forms, under a broader culture, the true foundation of all discovery and progress. It needs encouragement and direction rather than suppression, and the policy should be to increase the attractions and to facilitate access to this as well as other extraordinary natural objects; but at the same time the destructive effects, especially such as tend to reduce the interest, mar the beauty, or lessen the instructiveness of the facts, should be prevented by every proper means.

No one denies that visitors to this region usually carry away with them as much as their means of transportation will permit, but this consists usually, of course, of the smaller objects that lie in such profusion on the ground. At first view it might seem that the immense quantity of such objects makes it impossible that any appreciable impression can ever be made upon the whole mass in this way. This is the same kind of reasoning, or rather unreasoning, that has led to the virtual extinction of the buffalo, and which threatens to exhaust the sources of natural gas; but the class of persons known as "relic hunters" is very large, and the number who will in future visit the

Petrified Forests is destined greatly to increase. They usually carry with them some concealed tools or instruments, and with these they are perpetually breaking off pieces of objects of which they wish to carry away souvenirs. In this way the finest trunks are being hacked to pieces and disfigured. For example, there are several places on the Natural Bridge where this process has been going on, until quite large holes or unsightly cavities have been dug in the upper side of the trunk. The small chips and blocks that lie detached on the ground in such quantities vary greatly in form and coloration, and it is, of course, always the most symmetrical and brilliant that are first picked up, and these will eventually be so culled out that only the plainer, unattractive pieces will be left.

It is said that a useful purpose is subserved by sending specimens of the petrified wood to educational institutions. This might no doubt be true under proper regulation, especially if the specimens were duly labeled and authenticated and placed in properly arranged cabinets, to be explained by the teacher. This will scarcely be accomplished by permitting the free access of all parties with the right to carry away specimens at will.

Besides this piecemeal method of making inroads upon the treasures of the Petrified Forests, there are ways in which the work may be and to some extent has already been accomplished on a much larger scale. Many years ago the firm of Drake & Co., of Sioux Falls, undertook the work of manufacturing table tops, mantels, clock cases, pedestals, paper weights, and other articles of furniture and decoration out of these sections of agatized wood, by polishing the smooth surfaces and cutting them into the desired forms. I understand that Tiffany & Co., of New York, obtained through this company the beautiful pieces used by them for such purposes. I visited their house at the time they were engaged in this work, and through the courtesy of Mr. George F. Kunz was shown some of the raw material that they then had in hand, consisting of several sections of immense trunks, of the most brilliant colors. While in the park the present season my teamster informed me that he was employed for a long time hauling these trunks out of the upper forest to Carrizo station. Although, according to all accounts, many carloads of it were shipped to the East, he said that there was a larger quantity left at the station that was not shipped than all that was removed at that time. As scarcely any of this remains at the station now, I asked him what had become of it, and he said it had been carried off little by little by anybody that wanted a piece.

At a later date the Armstrong Abrasive Company, of Denver, conceived the idea of grinding up these trunks to make emery, for which they are said, from their extreme hardness, to be an excellent material. They had a plant for this purpose in Chicago, which they moved

to Arizona, and it is now at Adamana station, the nearest point to the Petrified Forests on the Santa Fe Pacific Railroad. I was informed that the plant never was put into operation, and on inquiring the reason I was told that a Canadian company at about the same time commenced the manufacture of emery and reduced the price to a point below that at which it would be profitable to grind up the Arizona wood. So small a business consideration prevented the somewhat wholesale denudation of the Petrified Forests for commercial purposes. What the next inducement in the same direction may be can not be predicted, but there is always the danger that some powerful mercantile interest may do its destructive work.

LOCAL OPINION RESPECTING THE PROPOSED RESERVE.

In making the investigations above recorded, in compliance with your instructions, I endeavored to preserve a wholly neutral attitude on the general question as to the advisability of reserving the Petrified Forests as a national park, and, as a matter of fact, the subject presented many practical difficulties which I was not and am not now able to remove. However clear the general proposition may be that so important a scenic feature and so great a natural wonder ought to be cared for and preserved intact for the enjoyment and instruction of the people, the question as to how this can best be accomplished is somewhat complicated and requires for its solution practical rather than scientific qualities of mind and considerable familiarity with its popular aspects. I therefore considered it my duty to feel, as it were, the local pulse on the subject, and I lost no opportunity to obtain the opinion of leading citizens of Arizona. As a result of my inquiries in this direction I was able to make the generalization that within the Territory the acquaintance with and interest in the project were inversely proportional to the distance of the parties from the region to be affected. In the immediate or close vicinity scarcely any one had heard of the action of the Territorial legislature, and almost no one evinced any interest in the matter. Such was the condition of things in Holbrook and Winslow. At Flagstaff and Williams there was more of both information and interest. I did not go to Prescott nor Phœnix, but I met and heard of people in both these places who were familiar with the movement and deeply interested in its success. First and foremost among these should be mentioned Governor Murphy, whose letter on the subject I give in full below. I chanced to meet his brother, Mr. Frank M. Murphy, of Prescott, on the train while returning from the field, and we had a conversation on the subject, freely discussing all the principal points. He presented a number of cogent arguments in favor of the project, as set forth in the memorial.

At his suggestion, as well as with the advice of several other interested parties whom I met in the Territory, I addressed a letter to Governor Murphy on my return to Washington, asking him for a full expression of his views. In my letter to him I said among other things:

You will understand that my instructions do not indicate the nature of my report, which is expected to be entirely disinterested upon the question of the advisability of the action asked for by the memorial, but I have not concealed my personal interest in the preservation of this wonderful natural feature of the country, and while my investigations in the immediate locality, which were quite thorough, did not reveal any blameworthy action on the part of any particular individuals, still, with the rapid growth of the Territory and the increasing travel and interest in all such matters in this country, it is obvious that it can not be long before something will need to be done if the scenic features of this region are to be preserved. It is not perfectly clear to me in what way the Government would accomplish this purpose if the reservation were made, and I doubt whether it could be successfully done without the active cooperation of the people of Arizona, and this would have to be something more than could be expected of the local residents of that district.

I therefore write you in the hope that you may freely express your views on the general subject, so that I may avail myself of them in my report. From the geological standpoint I, of course, have all the data necessary for the report, as I spent sixteen days in that general region constantly in the field; but upon the practical question of what the Government should do in case the district is set apart, and especially after such action, I do not feel fully competent to advise and need the assistance of practical public-spirited people, to whom the subject comes nearer home.

He replied promptly to this letter as follows:

Office of the Governor,
Phœnix, Arizona, November 28, 1899.

Dear Sir: Your letter of November 23, in reference to the so-called Petrified Forest in northern Arizona, received. I know of no way the Government can preserve this natural curiosity except by having it set aside as a reserve and appointing a keeper to protect it against vandalism.

As you probably observed when there, it is not attractive in the way of natural scenery. It seems to me peculiarly valuable for scientific purposes. I shall be glad to cooperate in any appropriate way for the preservation and protection of the so-called forest, but much expense on the part of the Government in creating a reserve for scenic purposes does not seem to me justified. I do not apprehend that any very large proportion of the agatized material will be removed. Some of the wood has been removed and polished and was exhibited at the World's Fair in Chicago, and I understand an exhibition of it is proposed at Paris next year. I do not think any injury can result from small quantities of the material being used in this way. As the mineral is scattered over the sands in large quantities, some of it covered at considerable depth, and there being a lack of water there for improving the grounds, I do not know of anything that can be done except to guard against its removal, and it is likely that your personal observation will enable you to make the most appropriate recommendations to the Department in regard to the matter.

Yours, very truly,

N. O. Murphy, *Governor.*

Mr. Lester F. Ward,
Paleontologist, United States Geological Survey, Washington, D. C.

I also wrote to Mr. Thomas Bunch, of Flagstaff, member of the legislative committee of Arizona, whom I did not meet there, but who, as I learned from a friend of his, Mr. George W. Sturtevant, of Chicago, had taken a special interest in the Petrified Forests. Mr. Bunch also replied promptly and his letter is altogether to the point:

FLAGSTAFF, ARIZONA, *November 28, 1899.*

DEAR SIR: Yours of 23d instant received. I would like to see the Pretrified Forest preserved. If it can be preserved by setting it aside as a national park I hope it may be done at an early date. I have known it and been there often for the past sixteen years, and every time I go I can see the traces of the vandal. Inside of twenty years from now there will be but little left of interest. However, I would be pleased to see some of the best specimens placed in our public institutions, and for that purpose I would permit a limited amount to be removed. I desire to have brought here a nice section and to place it in the Northern Arizona Normal School, and for like purposes I think it advisable to allow the removal. I hope the Government will see that it is not further made the place of amusement with explosives and that no more is blown up with powder.

Yours, truly, THOS. S. BUNCH,
Member Legislative Committee of Arizona.

LESTER F. WARD, Esq., *United States Geologist, Washington, D. C.*

From all this it will be seen that leading citizens and prominent public men in Arizona are sincerely desirous of preserving this interesting spot from vandalism and wanton destruction, and that many of them think that this can best be done by making it a national reserve and appointing the proper guardians to take charge of it. As they show, the expense of this need not be large. A single mounted ranger, such as now patrol the forest reserves of the Colorado plateau, would probably be adequate to this purpose for some time to come.

As nearly all tourists and visitors must approach the Petrified Forests by way of the Santa Fe Pacific Railroad, it is clearly to the interest of that road that they be made as attractive as possible, and there is no doubt that the officers of the road will gladly cooperate with the Government in this matter. A few years ago the nearest railroad station was Corrizo, which is some six miles west of north of the upper forest. The inconvenience of this was apparent to the railroad authorities, and they have recently established a station due north of the forests, only 7 miles from the nearest margin and about 8 miles from the Natural Bridge. This is the station of Adamana, the name being modified from that of the only person living there, Mr. Adam Hanna, upon whom now falls the duty of conducting parties to the Petrified Forests. Mr. Hanna derives considerable revenue from this source, especially as it is usually necessary for parties to stay over night, and he takes care of them. But his house is not convenient to the station and is not adapted for a hotel, and as the number of visitors increases it will become necessary to provide more ample accomodations. There will need to be a hotel with civilized conveniences, and

it will eventually be to the interest of the railroad company to provide such, as also suitable conveyances and guides.

The importance of the cooperation of the railroad in this matter struck me so forcibly that I did not consider my mission completed until I had made some inquiries along this line. In returning from the field, therefore, on my arrival at Chicago I called on President E. P. Ripley, of the Santa Fe system, to obtain his views. It chanced that Mr. Frank M. Murphy, whom I had met on the train, as above mentioned, was in Mr. Ripley's office at the time I called, and quite a discussion of the general project was entered into. Mr. Ripley expressed a willingness to cooperate with the Government in the matter to the fullest extent in case the proposed action was taken. He was much interested in certain of the scientific aspects of the case that I presented. Mr. Murphy very correctly pointed out that the fact of setting the district apart by the Government as a national park would do more than anything else that could be done to make it known to the public, thus constituting a legitimate advertisement for the road, and working in the joint interest of the company and the people.

RECOMMENDATIONS.

After all that has been said it scarcely seems necessary to make specific recommendations. I have endeavored to set forth the facts somewhat fully, and they will doubtless carry with them their own recommendations. But it may not be wholly superfluous, by way of summing up, to specify the suggestions to which the facts seem to give rise in a more succinct form.

1. It seems desirable that the portion of country covered by this report—at least all that falls within the designation of petrified forests—be withdrawn from entry at once, pending further steps in the matter. The amount withdrawn might be considerably larger than that included in the boundaries above specified, in order that no important feature shall be excluded from the tract finally embraced in the park. At present, so far as I could learn, there is only one claim filed on the entire area. This is a claim to a quarter section in the upper forest, filed by Mr. Adam Hanna. At the time that it was proposed to manufacture emery out of the silicified wood several other claims were filed, but, on the failure of the scheme, they were abandoned. Several horse herders have located on the arroyo, or creek, in the center of the lower or principal forest, and have erected a cabin there, but I understand that they have filed no claims and are there merely by sufferance. They are, however, doing no damage to the forests.

2. There is needed a more extended and accurate survey than I was able to make of the proper boundaries of the park. The survey should be made in part by geologists and vegetable paleontologists, in

order that due weight be given to scientific considerations—such, for example, as that of causing the park to include points at which the fossil logs are actually in place and good exposures of the rock in which they are embedded.

3. The area fixed upon by this survey should be correctly described and made a public reserve by act of Congress, with proper provisions for its preservation. I need not further specify what I think these provisions should be. Those of other similar acts, with special modifications to suit the case, will probably suffice.

4. As early as possible after the boundaries shall have been fixed there should be made a new topographical survey, which need not, however, be limited to this area, but should include it. The topographic map resulting from this survey should be on a scale of 1 mile to the inch and the contour interval should be 50 feet. The parties making the survey should be instructed to give appropriate names to all the more prominent objects and features, and to locate and name them on the map for the future use of the public visiting the park. A geological map should also be prepared on this new base. The present topographic map, on a scale of 4 miles to the inch, with a contour interval of 250 feet, is very inconvenient, and of little value in the study of this region.

5. No time should be lost in taking measures to prevent the parting and collapse of the natural bridge. As I have shown, this is liable to occur at any time; and although it may last for hundreds of years, still the danger that it may give way, and thus ruin the most important feature of the park, justifies prompt attention. Engineers should at least examine it at once, and if they find it insecure, as stated, they should take steps to strengthen it and render it permanent, which could probably be done at little expense.

As the land on which the natural bridge is located is public land, this work might be done independently of any action in the direction of making it a public reserve, but it seems doubtful whether the authorities could be brought to the point of actually taking action in the matter unless attention be first concentrated upon it by such an act as that of creating a public reservation. But if steps in this direction could be taken in advance of such action, this would diminish the chances of the catastrophe.

I have the honor to be, very respectfully, yours,

LESTER F. WARD,

Paleontologist, United States Geological Survey.

HON. CHARLES D. WALCOTT,

Director United States Geological Survey.

PRESENT CONDITION OF THE FLOOR OF THE OCEAN; EVOLUTION OF THE CONTINENTAL AND OCEANIC AREAS.[1]

By Sir JOHN MURRAY, K. C. B., F. R. S.

I.

In his opening address to the members of the British association at the Ipswich meeting, the president cast a retrospective glance at the progress that had taken place in the several branches of scientific inquiry from the time of the formation of the association in 1831 down to 1895, the year in which were published the last two of the fifty volumes of reports containing the scientific results of the voyage of H. M. S. *Challenger*. In that very able and detailed review there is no reference whatever to the work of the numerous expeditions which had been fitted out by this and other countries for the exploration of the depths of the sea, nor is there any mention of the great advance in our knowledge of the ocean during the period of sixty-five years then under consideration. This omission may be accounted for by the fact that at the time of the formation of the British Association knowledge concerning the ocean was, literally speaking, superficial. The study of marine phenomena had hitherto been almost entirely limited to the surface and shallow waters of the ocean, to the survey of coasts and of oceanic routes directly useful for commercial purposes. Down to that time there had been no systematic attempts to ascertain the physical and biological conditions of those regions of the earth's surface covered by the deeper waters of the ocean; indeed, most of the apparatus necessary for such investigations had not yet been invented.

The difficulties connected with the exploration of the greater depths of the sea arise principally from the fact that, in the majority of cases, the observations are necessarily indirect. At the surface of the ocean direct observation is possible, but our knowledge of the conditions prevailing in deep water, and of all that is there taking place, is almost wholly dependent on the correct working of instruments, the action of which at the critical moment is hidden from sight.

[1] Address of the president of the geographical section of the British Association for the Advancement of Science, at the Dover meeting, 1899. Reprinted from Report of the British Association, 1899, pp. 789–802.

It was the desire to establish telegraphic communication between Europe and America that gave the first direct impulse to the scientific exploration of the great ocean basins, and at the present day the survey of new cable routes still yields each year a large amount of accurate knowledge regarding the floor of the ocean. Immediately before the *Challenger* expedition there was a marked improvement in all the apparatus used in marine investigations, and thus during the *Challenger* expedition the great ocean basins were for the first time systematically and successfully explored. This expedition, which lasted for nearly four years, was successful beyond the expectations of its promoters, and opened out a new era in the study of oceanography. A great many sciences were enriched by a grand accumulation of new facts. Large collections were sent and brought home, and were subsequently described by specialists belonging to almost every civilized nation. Since the *Challenger* expedition there has been almost a revolution in the methods employed in deep-sea observations. The most profound abysses of the ocean are now being everywhere examined by sailors and scientific men with increasing precision, rapidity, and success.

The recognition of oceanography as a distinct branch of science may be said to date from the commencement of the *Challenger* investigations. The fuller knowledge we now possess about all oceanic phenomena has had a great modifying influence on many general conceptions as to the nature and extent of those changes which the crust of the earth is now undergoing and has undergone in past geological times. Our knowledge of the ocean is still very incomplete. So much has, however, already been acquired that the historian will in all probability point to the oceanographical discoveries during the past forty years as the most important addition to the natural knowledge of our planet since the great geographical voyages associated with the names of Columbus, Da Gama, and Magellan, at the end of the fifteenth and the beginning of the sixteenth centuries.

It is not my intention on this occasion to attempt anything like a general review of the present state of oceanographic science. But, as nearly all the samples of marine deposits collected during the past thirty years have passed through my hands, I shall endeavor briefly to point out what, in general, their detailed examination teaches with respect to the present condition of the floor of the ocean, and I will thereafter indicate what appears to me to be the bearing of some of these results on speculations as to the evolution of the existing surface features of our planet.

DEPTH OF THE OCEAN.

All measurements of depth, by which we ascertain the relief of that part of the earth's crust covered by water, are referred to the sea sur-

face. The measurements of height on the land are likewise referred to sea level. It is admitted that the ocean has a very complicated undulating surface in consequence of the attraction which the heterogeneous and elevated portions of the lithosphere exercise on the liquid hydrosphere. In the opinion of geodesists the geoid may in some places depart from the figure of the spheroid by 1,000 feet. Still it is not likely that this surface of the geoid departs so widely from the mean ellipsoidal form as to introduce a great error into our estimates of the elevations and depressions on the surface of the lithosphere.

The soundings over the water surface of the globe have accumulated at a rapid rate during the past fifty years. In the shallow water, where it is necessary to know the depth for purposes of navigation, the soundings may now be spoken of as innumerable. The 100-fathom line surrounding the land can therefore often be drawn in with much exactness. Compared with this shallow-water region, the soundings in deep water beyond the 100-fathom line are much less numerous. Each year, however, there are large additions to our knowledge. Within the last decade over 10,000 deep soundings have been taken by British ships alone. The deep soundings are scattered over the different ocean basins in varying proportions, being now most numerous in the North Atlantic and Southwest Pacific, and in these two regions the contour lines of depth may be drawn in with greater confidence than in the other divisions of the great ocean basins. It may be pointed out that 659 soundings, taken quite recently during cable surveys in the North Atlantic, although much closer together than is usually the case, and yielding much detailed information to cable engineers, have, from a general point of view, necessitated but little alteration in the contour lines drawn on the *Challenger* bathymetrical maps, published in 1895. Again, the recent soundings of the German steamship *Valdivia* in the Atlantic, Indian, and Southern oceans have not caused very great alteration in the positions of the contour lines on the *Challenger* maps, if we except one occasion in the South Atlantic when a depth of 2,000 fathoms was expected and the sounding machine recorded a depth of only 536 fathoms, and again in the great Southern Ocean, when depths exceeding 3,000 fathoms were obtained in a region where the contour lines indicated between 1,000 and 2,000 fathoms. This latter discovery suggests that the great depth recorded by Ross to the southeast of South Georgia may not be very far from the truth.

I have redrawn the several contour lines of depth in the great ocean basins, after careful consideration of the most recent data, and these may now be regarded as a somewhat close approximation to the actual state of matters, with the possible exception of the great Southern and Antarctic oceans, where there are relatively few soundings, but where the projected antarctic expeditions should soon be at work. On the whole, it may be said that the general tendency of recent soundings is

to extend the area with depths greater than 1,000 fathoms and to show that numerous volcanic cones rise from the general level of the floor of the ocean-basins up to various levels beneath the sea-surface.

The areas marked out by the contour-lines of depth are now estimated as follows:

Between the shore and 100 fathoms, 7,000,000 square geographical miles, or 7 per cent of the sea-bed.

Between 100 and 1,000 fathoms, 10,000,000 square geographical miles, or 10 per cent of the sea-bed.

Between 1,000 and 2,000 fathoms, 22,000,000 square geographical miles or 21 per cent of the sea-bed.

Between 2,000 and 3,000 fathoms, 57,000,000 square geographical miles, or 55 per cent of the sea-bed.

Over 3,000 fathoms, 7,000,000 square geographical miles, or 7 per cent of the sea-bed.

Total, 103,000,000 square geographical miles, 100 per cent.

From these results it appears that considerably more than half of the sea-floor lies at a depth exceeding 2,000 fathoms, or over two geographical miles. It is interesting to note that the area within the 100-fathom line occupies 7,000,000 square geographical miles, whereas the area occupied by the next succeeding 900 fathoms (viz. between 100 and 1,000 fathoms) occupies only 10,000,000 square geographical miles. This points to a relatively rapid descent of the sea-floor along the continental slopes between 100 and 1,000 fathoms, and therefore confirms the results gained by actual soundings in this region, many of which indicate steep inclines or even perpendicular cliffs. Not only are the continental slopes the seat of many deposit-slips and seismic disturbances, but Mr. Benest has given good reasons for believing that underground rivers sometimes enter the sea at depths beyond 100 fathoms, and there bring about sudden changes in deep water. Again, the relatively large area covered by the continental shelf between the shore line and 100 fathoms points to the wearing away of the land by current and wave action.

On the *Challenger* charts all areas where the depth exceeds 3,000 fathoms have been called "deeps," and distinctive names have been conferred upon them. Forty-three such depressions are now known, and the positions of these are shown on the map here exhibited; 24 are situated in the Pacific Ocean, 3 in the Indian Ocean, 15 in the Atlantic Ocean, and 1 in the Southern and Antarctic oceans. The area occupied by these 39 deeps is estimated at 7,152,000 square geographical miles, or about 7 per cent of the total water surface of the globe. Within these deeps over 250 soundings have been recorded, of which 24 exceed 4,000 fathoms, including 3 exceeding 5,000 fathoms.

Depths exceeding 4,000 fathoms (or 4 geographical miles) have been recorded within eight of the deeps, viz: In the North Atlantic within the Nares Deep; in the Antarctic within the Ross Deep; in the Banda Sea within the Weber Deep; in the North Pacific within the Chal-

lenger, Tuscarora, and Supau deeps; and in the South Pacific within the Aldrich and Richards deeps. Depths exceeding 5,000 fathoms have been hitherto recorded only within the Aldrich Deep of the South Pacific, to the east of the Kermadecs and Friendly Islands, where the greatest depth is 5,155 fathoms, or 530 feet more than 5 geographical miles. being about 2,000 feet more below the level of the sea than the summit of Mount Everest in the Himalayas is above it. The levels on the surface of the lithosphere thus oscillate between the limits of about 10 geographical miles (more than 18 kilometers).

TEMPERATURE OF THE OCEAN FLOOR.

Our knowledge of the temperature on the floor of the ocean is derived from observations in the layers of water immediately above the bottom by means of deep-sea thermometers, from the electric resistance of telegraph cables resting on the bed of the great ocean basins, and from the temperature of large masses of mud and ooze brought up by the dredge from great depths. These observations are now sufficiently numerous to permit of some general statements as to the distribution of temperature over the bottom of the great oceans.

All the temperatures recorded up to the present time in the subsurface waters of the open ocean indicate that at a depth of about 100 fathoms seasonal variation of temperature disappears. Beyond that depth there is a constant, or nearly constant, temperature at any one place throughout the year. In some special positions, and under some peculiar conditions, a lateral shifting of large bodies of water takes place on the floor of the ocean at depths greater than 100 fathoms. This phenomenon has been well illustrated by Professor Libbey off the east coast of North America, where the Gulf Stream and Labrador Current run side by side in opposite directions. This lateral shifting can not, however, be called seasonal, for it appears to be effected by violent storms or strong offshore winds bringing up colder water from considerable depths to supply the place of the surface drift, so that the colder water covers stretches of the ocean's bed which under normal conditions are overlaid by warmer strata of water. Sudden changes of temperature like these cause the destruction of innumerable marine animals, and produce very marked peculiarities in the deposits over the areas thus affected.

It is estimated that 92 per cent of the entire sea floor has a temperature lower than 40° F. This is in striking contrast to the temperature prevailing at the surface of the ocean, only 16 per cent of which has a mean temperature under 40° F. The temperature over nearly the whole of the floor of the Indian Ocean in deep water is under 35° F. A similar temperature occurs over a large part of the South Atlantic and certain parts of the Pacific, but at the bottom of the North Atlantic basin and over a very large portion of the Pacific the temperature

is higher than 35° F. In depths beyond 2,000 fathoms the average temperature over the floor of the North Atlantic is about 2° F. above the average temperature at the bottom of the Indian Ocean and South Atlantic, while the average temperature of the bed of the Pacific is intermediate between these.

It is admitted that the low temperature of the deep sea has been acquired at the surface in polar and subpolar regions, chiefly within the higher latitudes of the Southern Hemisphere, where the cooled surface water sinks to the bottom and spreads slowly over the floor of the ocean into equatorial regions. These cold waters carry with them into the deep sea the gases of the atmosphere which are everywhere taken up at the surface, according to the known laws of gas absorption. In this way myriads of living animals are enabled to carry on their existence at all depths in the open ocean. The nitrogen remains more or less constant at all times and places, but the proportion of oxygen is frequently much reduced in deep water, owing to the processes of oxidation and respiration which are there going on.

The deep sea is a region of darkness as well as of low temperature, for the direct rays of the sun are wholly absorbed in passing through the superficial layers of water. Plant life is in consequence quite absent over 93 per cent of the bottom of the ocean, or 66 per cent of the whole surface of the lithosphere. The abundant deep-sea fauna, which covers the floor of the ocean, is, therefore, ultimately dependent for food upon organic matter assimilated by plants near its surface, in the shallower waters near the coast lines, and on the surface of the dry land itself.

As has been already stated, about 7,000,000 square geographical miles of the sea floor lies within the 100-fathom line, and this area is in consequence subject to seasonal variations of temperature, to strong currents, to the effects of sunlight, and presents a great variety of physical conditions. The planktonic plant life is here reinforced by the littoral seaweeds, and animal life is very abundant. About 40 per cent of the water over the bottom of this shallow-water area has a mean temperature under 40° F., while 20 per cent has a mean temperature between 40° and 60° F., and 40 per cent a temperature of over 60° F.

It follows from this that only 3 per cent of the floor of the ocean presents conditions of temperature favorable for the vigorous growth of corals and those other benthonic organisms which make up coral reefs and require a temperature of over 60° F. all year round. On the other hand, more than half of the surface of the ocean has a temperature which never falls below 60° F. at any time of the year. In these surface waters, with a high temperature, the shells of pelagic mollusks, foraminifera, algæ, and other planktonic organisms are secreted in great abundance and fall to the bottom after death.

It thus happens that, at the present time, over nearly the whole floor of the ocean we have mingled in the deposits the remains of organisms which had lived under widely different physical conditions, since the remains of organisms which lived in tropical sunlight, and in water at a temperature above 80° F. all their lives, now lie buried in the same deposit on the sea floor, together with the remains of other organisms which lived all their lives in darkness and at a temperature near to the freezing point of fresh water.

MARINE DEPOSITS ON THE OCEAN FLOOR.

The marine deposits now forming over the floor of the ocean present many interesting peculiarities according to their geographical and bathymetrical position. On the continental shelf, within the 100-fathom line, sands and gravels predominate, while on the continental slopes beyond the 100-fathom line, blue muds, green muds, and red muds, together with volcanic muds and coral muds prevail, the latter two kinds of deposits being, however, more characteristic of the shallow water around oceanic islands. The composition of all these terrigenous deposits depend on the structure of the adjoining land. Around continental shores, except where coral reefs, limestones, and volcanic rocks are present, the material consists principally of fragments and minerals derived from the disintegration of the ancient rocks of the continents, the most characteristic and abundant mineral species being quartz. River detritus extends in many instances far from the land, while off high and bold coasts, where no large rivers enter the sea, pelagic conditions may be found in somewhat closer proximity to the shore line. It is in these latter positions that green muds containing much glauconite, and other deposits containing many phosphatic nodules, have for the most part been found; as, for instance, off the eastern coast of the United States, off the Cape of Good Hope, and off the eastern coasts of Australia and Japan. The presence of glauconitic grains and phosphatic nodules in the deposits at these places appears to be very intimately associated with a great annual range of temperature in the surface and shallow waters, and the consequent destruction of myriads of marine animals. As an example of this phenomenon may be mentioned the destruction of the tile fish in the spring of 1882 off the eastern coast of North America, when a layer 6 feet in thickness of dead fish and other marine animals was believed to cover the ocean floor for many square miles.

In all the terrigenous deposits the evidences of the mechanical action of tides, of currents, and of a great variety of physical conditions, may almost everywhere be detected, and it is possible to recognize in these deposits an accumulation of materials analogous to many of the marine stratified rocks of the continents, such as sandstones, quartzites, shales, marls, greensands, chalks, limestones, conglomerates, and volcanic grits.

With increasing depth and distance from the continents the deposits gradually lose their terrigenous character, the particles derived directly from the emerged land decrease in size and in number, the evidences of mechanical action disappear, and the deposits pass slowly into what have been called pelagic deposits at an average distance of about 200 miles from continental coast lines. The materials composing pelagic deposits are not directly derived from the disintegration of the continents and other land surfaces. They are largely made up of the shells and skeletons of marine organisms secreted in the surface waters of the ocean, consisting either of carbonate of lime, such as pelagic mollusks, pelagic foraminifera, and pelagic algæ, or of silica, such as diatoms and radiolarians. The inorganic constituents of the pelagic deposits are for the most part derived from the attrition of floating pumice, from the disintegration of water-logged pumice, from showers of volcanic ashes, and from the débris ejected from submarine volcanoes, together with the products of their decomposition. Quartz particles, which play so important a rôle in the terrigenous deposits, are almost wholly absent, except where the surface waters of the ocean are affected by floating ice, or where the prevailing winds have driven the desert sands far into the oceanic areas. Glauconite is likewise absent from these abysmal regions. The various kinds of pelagic deposits are named, according to their characteristic constituents, pteropod oozes, globigerina oozes, diatom oozes, radiolarian oozes, and red clay.

The distribution of the deep-sea deposits over the floor of the ocean is shown on the map here exhibited, but it must be remembered that there is no sharp line of demarcation between them; the terrigenous pass gradually into the pelagic deposits, and the varieties of each of these great divisions also pass insensibly the one into the other, so that it is often difficult to fix the name of a given sample.

On another map here exhibited the percentage distribution of carbonate of lime in the deposits over the floor of the ocean has been represented, the results being founded on an extremely large number of analyses. The results are also shown in the following table:

	Square geographical miles.	Percentage.
Over 75 per cent $CaCO_3$	6,000,000	5.8
50 to 75 per cent $CaCO_3$	24,000,000	23.2
25 to 50 per cent $CaCO_3$	14,000,000	13.5
Under 25 per cent $CaCO_3$	59,000,000	57.5
Total	103,000,000	100

The carbonate-of-lime shells derived from the surface play a great and puzzling rôle in all deep-sea deposits, varying in abundance according to the depth of the ocean and the temperature of the surface waters.

In tropical regions removed from land, where the depths are less than 600 fathoms, the carbonate of lime due to the remains of these organisms from the surface may rise to 80 or 90 per cent; with increase of depth, and under the same surface conditions, the percentage of carbonate of lime slowly diminishes, till, at depths of about 2,000 fathoms, the average percentage falls to about 60, at 2,400 fathoms to about 30, and at about 2,600 fathoms to about 10, beyond which depth there may be only traces of carbonate of lime due to the presence of surface shells. The thin and more delicate surface shells first disappear from the deposits; the thicker and denser ones alone persist to greater depths. A careful examination of a large number of observations shows that the percentage of carbonate of lime in the deposits falls off much more rapidly at depths between 2,200 and 2,500 fathoms than at other depths.

The red clay which occurs in all the deeper stretches of the ocean far from land, and covers nearly half of the whole sea floor, contains—in addition to volcanic débris, clayey matter, the oxides of iron and manganese—numerous remains of whales, sharks, and other fishes, together with zeolitic crystals, manganese nodules, and minute magnetic spherules, which are believed to have a cosmic origin. One haul of a small trawl in the central Pacific brought to the surface on one occasion, from a depth of about 2½ miles, many bushels of manganese nodules, along with 1,500 sharks' teeth, over 50 fragments of earbones and other bones of whales. Some of these organic remains, such as the *Carcharodon* and *Lamna* teeth and the bones of the Ziphioid whales, belong apparently to extinct species. One or two of these sharks' teeth, earbones, or cosmic spherules may be occasionally found in a globigerina ooze, but their occurrence in this or any deposits other than red clay is extremely rare.

Our knowledge of the marine deposits is limited to the superficial layers; as a rule the sounding tube does not penetrate more than 6 or 8 inches, but in some positions the sounding tube and dredge have been known to sink fully 2 feet into the deposit. Sometimes a red clay is overlaid by a globigerina ooze, more frequently a red clay overlies a globigerina ooze, the transition between the two layers being either abrupt or gradual. In some positions it is possible to account for these layers by referring them to changes in the condition of the surface waters, but in other situations it seems necessary to call in elevations and subsidences of the sea floor.

If the whole of the carbonate of lime shells be removed by dilute acid from a typical sample of globigerina ooze, the inorganic residue left behind is quite similar in composition to a typical red clay. This suggests that possibly, owing to some hypogene action, such as the escape of carbonic acid through the sea floor, a deposit that once was a globigerina ooze might be slowly converted into a red clay. However, this is not the interpretation which commends itself after an examina-

tion of all the data at present available; a consideration of the rate of accumulation probably affords a more correct interpretation. It appears certain that the terrigenous deposits accumulate much more rapidly than the pelagic deposits. Among the pelagic deposits the pterapod and globigerina oozes of the tropical regions, being made up of the calcareous shells of a much larger number of tropical species, apparently accumulate at a greater rate than the globigerina oozes in extratropical areas. Diatom ooze, being composed of both calcareous and siliceous organisms, has again a more rapid rate of deposition than radiolarian ooze. In red clay the minimum rate of accumulation takes place. The number of sharks' teeth, of earbones and other bones of Cetaceans, and of cosmic spherules in a deposit may indeed be taken as a measure of the rate of deposition. These spherules, teeth, and bones are probably more abundant in the red clays, because few other substances there fall to the bottom to cover them up, and they thus form an appreciable part of the whole deposit. The volcanic materials in a red clay having, because of the slow accumulation, been for a long time exposed to the action of the sea water, have been profoundly altered. The massive manganese-iron nodules and zeolitic crystals present in the deposit are secondary products arising from the decomposition of these volcanic materials, just as the formation of glauconite, phosphatic, and calcareous and barytic nodules accompanies the decomposition of terrigenous rocks and minerals in deposits nearer continental shores. There is thus a striking difference between the average chemical and mineralogical composition of terrigenous and pelagic deposits.

It would be extremely interesting to have a detailed examination of one of those deep holes where a typical red clay is present, and even to bore some depth into such a deposit, if possible, for in these positions it is probable that not more than a few feet of deposit have accummulated since the close of the Tertiary period. One such area lies to the southwest of Australia, and its examination might possibly form part of the programme of the approaching antarctic explorations.

LIFE ON THE OCEAN FLOOR.

It has already been stated that plant life is limited to the shallow waters, but fishes and members of all the invertebrate groups are distributed over the floor of the ocean at all depths. The majority of these deep-sea animals live by eating the mud, clay, or ooze, or by catching the minute particles of organic matter which fall from the surface. It is probably not far from the truth to say that three-fourths of the deposits now covering the floor of the ocean have passed through the alimentary canals of marine animals. These mud-eating species, many of which are of gigantic size when compared with their allies living in the shallow coastal waters, become in turn the prey of numerous rapacious animals armed with peculiar prehensile and tactile organs.

Some fishes are blind, while others have very large eyes. Phosphorescent light plays a most important rôle in the deep sea, and is correlated with the prevailing red and brown colors of deep-sea organisms. Phosphorescent organs appear sometimes to act as a bull's-eye lantern to enable particles of food to be picked up, and at other times as a lure or a warning. All these peculiar adaptations indicate that the struggle for life may not be much less severe in the deep sea than in the shallower waters of the ocean.

Many deep-sea animals present archaic characters; still the deep sea can not be said to contain more remnants of faunas which flourished in remote geological periods than the shallow and fresh waters of the continents. Indeed, king-crabs, Lingulas, Trigonias, Port Jackson sharks, *Ceratodus*, *Lepidosiren*, and *Protopterus* probably represent older faunas than anything to be found in the deep sea.

Sir Wyville Thompson was of the opinion that, from the Silurian period to the present day, there had been as now a continuous deep ocean with a bottom temperature oscillating about the freezing point of fresh water, and that there had always been an abyssal fauna. I incline to the view that in Paleozoic times the ocean basins were not so deep as they are now; that the ocean then had throughout a nearly uniform high temperature, and that life was either absent or represented only by bacteria and other low forms in great depths, as is now the case in the Black Sea, where life is practically absent beyond 100 fathoms, and where the deeper waters are saturated with sulphureted hydrogen. This is not, however, the place to enter on speculations concerning the origin of the deep-sea fauna, nor to dwell on what has been called "bipolarity" in the distribution of marine organisms.

II.

EVOLUTION OF THE CONTINENTAL AND OCEANIC AREAS.

I have now pointed out what appear to me to be some of the more general results arrived at in recent years regarding the present conditiion of the floor of the ocean. I may now be permitted to indicate the possible bearing of these results on opinions as to the origin of some fundamental geographical phenomena; for instance, on the evolution of the protruding continents and sunken ocean basins. In dealing with such a problem much that is hypothetical must necessarily be introduced, but these speculations are based on ascertained scientific facts.

The well-known American geologist, Dutton, says:

"It has been much the habit of geologists to attempt to explain the progressive elevation of plateaus and mountain platforms, and also the folding of strata, by one and the same process. I hold the two processes to be distinct, and having no necessary relation to each other.

There are plicated regions which are little or not at all elevated, and there are elevated regions which are not plicated."

Speaking of great regional uplifts, he says further:

"What the real nature of the uplifting force may be is, to my mind, an entire mystery, but I think we may discern at least one of its attributes, and that it is a gradual expansion or diminution of density of the subterranean magmas. * * * We know of no cause which could either add to the mass or diminish the density, yet one of the two must surely have happened. * * * Hence I infer that the the cause which elevates the land involves an expansion of the underlying magmas, and the cause which depresses it is a shrinkage of the magmas. The nature of the process is at present a complete mystery."

I shall endeavor to show how the detailed study of marine deposits may help to solve the mystery here referred to by Dutton.

The surface of the globe has not always been as we now see it. When, in the past, the surface had a temperature of about 400° F., what is now the water of the ocean must have existed as water vapor in the atmosphere, which would thereby—as well as because of the presence of other substances—be increased in density and volume.

Life, as we know it, could not then exist. Again, science foresees a time when low temperatures, like those produced by Professor Dewar at the Royal Institution, will prevail over the face of the earth. The hydrosphere and atmosphere will then have disappeared within the rocky crust, or the waters of the ocean will have become solid rock, and over their surface will roll an ocean of liquid air about 40 feet in depth. Life, as we know it, unless it undergoes suitable secular modifications, will be extinct. Somewhere between these two indefinite points of time in the evolution of our planet it is our privilege to live, to investigate, and to speculate concerning the antecedent and future condition of things.

When we regard our globe with the mind's eye, it appears at the present time to be formed of concentric spheres, very like, and still very unlike, the successive coats of an onion. Within is situated the vast nucleus or centrosphere; surrounding this is what may be called the tektosphere,[1] a shell of materials in a state bordering on fusion, upon which rests and creeps the lithosphere; then follow hydrosphere and atmosphere, with the included biosphere.[2] To the interaction of these six geospheres, through energy derived from internal and external sources, may be referred all the existing superficial phenomena of the planet.

The vast interior of the planetary mass, although not under direct observation, is known, from the results of the astronomer and physicist, to have a mean density of 5.6, or twice that of ordinary surface rock. The substances brought within the reach of observation in vein-

[1] τηκτός, molten. [2] βίος.

stones, in lavas, and hypogene rocks—by the action of water as a solvent and sublimant—warrant the belief that the centrosphere is largely made up of metals and metalloids with imprisoned gases. It is admitted that the vast nucleus has a very high temperature, but so enormous is the pressure of the superincumbent crust that the melting point of the substances in the interior is believed to be raised to a higher value than the temperature there existing—the centrosphere in consequence remains solid, for it may be assumed that the melting point of rock-forming materials is raised by increase of pressure. Astronomers, from a study of precession and nutation have long been convinced that the centrosphere must be practically solid.

Recent seismological observations indicate the transmission of two types of waves through the earth—the condensational-rarefactional and the purely distortional—and the study of these tremors supports the view that the centrosphere is not only solid, but possesses great uniformity of structure. The seismological investigations of Professors Milne and Knott point also to a fairly abrupt boundary or transition surface, where the solid nucleus passes into the somewhat plastic magma on which the firm upper crust rests.

In this plastic layer or shell—named the tektosphere—the materials are most probably in a state of unstable equilibrium and bordering on fusion. Here the loose-textured solids of the external crust are converted into the denser solids of the nucleus or into molten masses, at a critical point of temperature and pressure; deep-seated rocks may in consequence escape through fissures in the lithosphere. Within the lithosphere itself the temperature falls off so rapidly toward the surface as to be everywhere below the melting point of any substance there under its particular pressure.

Now, as the solid centrosphere is slowly contracted from loss of heat, the primitive lithosphere, in accommodating itself—through changes in the tektosphere—to the shrinking nucleus, would be buckled, warped, and thrown into ridges. That these movements are still going on is shown by the fact that the lithosphere is everywhere and at all times in a slight but measurable state of pulsation. The rigidity of the primitive rocky crust would permit of considerable deformations of the kind here indicated. Indeed, the compression of mountain chains has most probably been brought about in this manner, but the same can not be said of the elevation of plateaus, of mountain platforms, and of continents.

From many lines of investigation it is concluded, as we have seen, that the centrosphere is homogeneous in structure. Direct observation, on the other hand, shows that the lithosphere is heterogeneous in composition. How has this heterogeneity been brought about? The original crust was almost certainly composed of complex and stable silicates, all the silicon dioxide being in combination with bases.

Lord Kelvin has pointed out that, when the solid crust began to form, it would rapidly cool over its whole surface, the precipitation of water would accelerate this process, and there would soon be an approximation to present conditions. As time went on the plastic or critical layer—the tektosphere—immediately beneath the crust would gradually sink deeper and deeper, while ruptures and readjustments would become less and less frequent than in earlier stages. With the first fall of rain the silicates of the crust would be attacked by water and carbon dioxide, which can at low temperatures displace silicon dioxide from its combinations. The silicates, in consequence, have been continuously robbed of a part or the whole of their bases. The silica thus set free goes ultimately to form quartz veins and quartz sand on or about the emerged land, while the bases leached out of the disintegrating rocks are carried out into the ocean and ocean basins. A continuous disintegration and differentiation of materials of the lithosphere accompanied by a sort of migration and selection among mineral substances, is thus always in progress. Through the agency of life, carbonate of lime accumulates in one place; through the agency of winds, quartz sand is heaped up in another; through the agency of water, beds of clay, of oxides of iron and of manganese are spread out in other directions.

The contraction of the centrosphere supplies the force which folds and crumples the lithosphere. The combined effect of hydrosphere, atmosphere, and biosphere on the lithosphere gives direction and a determinate mode of action to that force. From the earliest geological times the most resistant dust of the continents has been strewn along the marginal belt of the sea floor skirting the land. At the present time the deposits over this area contain on the average about 70 per cent of free and combined silica, mostly in the form of quartz sand. In the abysmal deposits far from land there is an average of only about 30 per cent of silica, and hardly any of this in the form of quartz sand. Lime, iron, and the other bases largely predominate in these abysmal regions. The continuous loading on the margins of the emerged land by deposits tends by increased pressure to keep the materials of the tektosphere in a solid condition immediately beneath the loaded area. The unloading of emerged land tends by relief of pressure to produce a viscous condition of the tektosphere immediately beneath the denuded surfaces. Under the influence of the continuous shakings, tremors, and tremblings always taking place in the lithosphere the materials of the tektosphere yield to the stresses acting on them, and the deep-seated portions of the terrigenous deposits are slowly carried toward, over, or underneath the emerged land. The rocks subsequently reformed beneath continental areas out of these terrigenous materials, under great pressure and in hydrothermal conditions, would be more acid than the rocks from which they were originally derived,

and it is well known that the acid silicates have a lower specific gravity than the intermediate or basic ones. By a continual repetition of this process the continental protuberances have been gradually built up of lighter materials than the other parts of the lithosphere. The relatively light quartz, which is also the most refractory, the most stable, and the least fusible among rock-forming minerals, plays in all this the principal rôle. The average height of the surface of the continents is about 3 miles above the average level of the abysmal regions. If now we assume the average density of the crust beneath the continents to be 2.5, and of the part beneath the abysmal regions to be 3, then the spheroidal surface of equal pressure—the tektosphere—would have a minimum depth of 18 miles beneath the continents and 15 miles beneath the oceans, or, if we assume the density of the crust beneath the continents to be 2.5, and beneath the abysmal regions to be 2.8, then the tektosphere would be 28 miles beneath the continents and 25 miles beneath the oceans. The present condition of the earth's crust might be brought about by the disintegration of a quantity of quartz-free volcanic rock, covering the continental areas to a depth of 18 miles, and the reformation of rocks out of the disintegrated materials.

When the lighter and more bulky substances have accumulated there has been a relative increase of volume, and in consequence bulging has taken place at the surface over the continental areas. Where the denser materials have been laid down there has been flattening, and in consequence a depression of the abysmal regions of the ocean basins. It is known that, as a general rule, where large masses of sediment have been deposited, their deposition has been accompanied by a depression of the area. On the other hand, where broad mountain platforms have been subjected to extensive erosion the loss of altitude by denudation has been made good by a rise of the platform. This points to a movement of matter on to the continental areas.

If this be anything like a true conception of the interactions that are taking place between the various geospheres of which our globe is made up then we can understand why, in the gradual evolution of the surface features, the average level of the continental plains now stands permanently about 3 miles above the average level of those plains which form the floor of the deep ocean basins. We may also understand how the defect of mass under the continents and an excess of mass under the oceans have been brought about, as well as deficiency of mass under mountains and excess of mass under plains. Even the local anomalies indicated by the plumb line, gravity, and magnetic observations may in this way receive a rational explanation. It has been urged that an enormous time—greater even than what is demanded by Darwin—would be necessary for an evolution of the existing surface features on these lines. I do not think so. Indeed,

in all that relates to geological time, I agree, generally speaking, with the physicists, rather than with the biologists and geologists.

PROGRESS OF OCEANIC RESEARCH.

I have now touched on some of the problems and speculations suggested by recent deep-sea explorations, and there are many others equally attractive to which no reference has been made. It is abundantly evident that for the satisfactory explanation of many marine phenomena further observations and explorations are necessary. Happily, there is no sign that the interest in oceanographical work has is any way slackened. On the contrary, the number of scientific men and ships engaged in the study of the ocean is rapidly increasing. Among all civilized peoples and in all quarters of the globe the economic importance of many of the problems that await solution is clearly recognized.

We have every reason to be proud of the work continually carried on by the officers and ships attached to the hydrographic department of the British navy. They have surveyed coasts in all parts of the world for the purposes of navigation, and within the past few years have greatly enlarged our knowledge of the sea bed and deeper waters over wide stretches of the Pacific and other oceans. The samples of the bottom which are procured, being always carefully preserved by the officers, have enabled very definite notions to be formed as to the geographical and bathymetrical distribution of marine deposits.

The ships belonging to the various British telegraph cable companies have done most excellent work in this as well as in other directions. Even during the present year Mr. R. E. Peake has in the steamship *Britannia* procured 477 deep soundings in the North Atlantic, besides a large collection of deep-sea deposits and many deep-sea temperature and current observations.

The French have been extending the valuable work of the *Talisman* and *Travailleur*, while the Prince of Monaco is at the present moment carrying on his oceanic investigations in the arctic seas with a large new yacht elaborately and specially fitted out for such work. The Russians have recently been engaged in the scientific exploration of the Black Sea and the Caspian Sea, and a special ship is now employed in the investigation of the arctic fisheries of the Murman coast under the direction of Professor Knipowitsch. Admiral Makaroff has this summer been hammering his way through arctic ice, and at the same time carrying on a great variety of systematic observations and experiments on board the *Yermak*—the most powerful and most effective instrument of marine research ever constructed. Mr. Alexander Agassiz has this year recommenced his deep-sea explorations in the Pacific on board the U. S. S. *Albatross*. He proposes to cross the Pacific in several directions, and to conduct investigations among the

Paumotu and other coral-island groups. Professor Weber is similarly employed on board a Dutch man-of-war in the East Indian seas. The Deutsche Seewarte at Hamburg, under the direction of Dr. Neumayer, continues its praiseworthy assistance and encouragement to all investigators of the ocean, and this year the important German deep-sea expedition, in the steamship *Valdivia*, arrived home after most successful oceanographical explorations in the Atlantic, Indian, and great Southern oceans.

The *Belgica* has returned to Europe safely with a wealth of geological and biological collections and physical observations, after spending, for the first time on record, a whole winter among the ice fields and icebergs of the Antarctic. Mr. Borchgrevink in December last again penetrated to Cape Adare, successfully landed his party at that point, and is now wintering on the Antarctic continent. The expeditions of Lieutenant Peary, of Professor Nathorst, of Captain Sverdrup, and of the Duke of Abruzzi, which are now in progress, may be expected to yield much new information about the condition of the Arctic Ocean. Mr. Wellman has just returned from the north of Franz-Josef Land with observations of considerable interest.

Some of the scientific results obtained by the expeditions in the Danish steamer *Ingolf* have lately been published, and these, along with the results of the joint work pursued for many years by the Swedes, Danes, and Norwegians, may ultimately have great economic value from their direct bearing on fishery problems and on weather forecasting over long periods of time.

Largely through the influence of Prof. Otto Pettersson, an international conference assembled at Stockholm a few months ago for the purpose of deliberating as to a programme of conjoint scientific work in the North Sea and northern parts of the Atlantic, with special reference to the economic aspect of sea fisheries. A programme was successfully drawn up and an organization suggested for carrying it into effect. These proposals are now under the consideration of the several states. The Norwegian Government has voted a large sum of money for building a special vessel to conduct marine investigations of the nature recommended by this conference. It is to be hoped the other North Sea powers may soon follow this excellent example.

The various marine stations and laboratories for scientific research in all parts of the world furnish each year much new knowledge concerning the ocean. Among our own people the excellent work carried on by the Marine Biological Association, the Irish fisheries department, the Scottish fishery board, the Lancashire fisheries committee, the Cape and Canadian fisheries department, is well worthy of recognition and continued support. Mr. George Murray, Mr. H. N. Dickson, Professor Cleve, Prof. Otto Pettersson, Mr. Robert Irvine, and others have, with the assistance of the officers of the mercantile marine, accu-

mulated in recent years a vast amount of information regarding the distribution of temperature and salinity, as well as of the planktonic organisms at the surface of the ocean. The papers by Mr. H. C. Russell on the icebergs and currents of the great Southern Ocean and of Mr. F. W. Walker on the density of the water in the Southern Hemisphere show that the Australian colonies are taking a practical interest in oceanographical problems.

PROPOSED ANTARCTIC EXPLORATIONS.

The great event of the year, from a geographical point of view, is the progress that has been made toward the realization of a scheme for the thorough scientific exploration in the near future of the whole south polar region. The British and German governments have voted or guaranteed large sums of money to assist in promoting this object, and princely donations have likewise been received from private individuals, in this connection the action of Mr. L. W. Longstaff in making a gift of £25,000, and of Mr. A. C. Harmsworth in promising £5,000, being beyond all praise.

There is an earnest desire among the scientific men of Britain and Germany that there should be some sort of cooperation with regard to the scientific work of the two expeditions, and that these should both sail in 1901, so that the invaluable gain attaching to simultaneous observations may be secured. Beyond this nothing has, as yet, been definitely settled. The members of the association will presently have an opportunity of expressing their opinions as to what should be attempted by the British expedition, how the work in connection with it should be arranged, and how the various researches in view can best be carried to a successful issue.

I have long taken a deep interest in antarctic exploration, because such exploration must necessarily deal largely with oceanographical problems, and also because I have had the privilege of studying the conditions of the ocean within both the Arctic and Antarctic circles. In the year 1886 I published an article on the subject of antarctic exploration in the Scottish Geographical Magazine. This article led to an interesting interview, especially when viewed in the light of after events, for a few weeks after it appeared in type a young Norwegian walked into the *Challenger* office in Edinburgh to ask when the proposed expedition would probably start, and if there were any chance of his services being accepted. His name was Nansen.

When at the request of the president I addressed the Royal Geographical Society on the same subject in the year 1893, I made the following statement as to what it seemed to me should be the general character of the proposed exploration:

"A dash at the South Pole is not, however, what I advocate, nor do I believe *that* is what British science at the present time desires.

It demands rather a steady, continuous, laborious, and systematic exploration of the whole southern region with all the appliances of the modern investigator."

At the same time I urged further that these explorations should be undertaken by the royal navy in two ships, and that the work should extend over two winters and three summers.

This scheme must now be abandoned, so far at least as the royal navy is concerned, for the government has intimated that it can spare neither ships nor officers, men nor money, for an undertaking of such magnitude. The example of foreign powers—rather than the representations from our own scientific men—appears to have been chiefly instrumental in at last inducing the government to promise a sum of £45,000, provided that an equal amount be forthcoming from other sources. This resolve throws the responsibility for the financial administration, for the equipment, and for the management of this exploration on the representative scientific societies, which have no organization ready for carrying out important executive work on such an extensive scale. I am doubtful whether this state of matters should be regarded as a sign of increasing lukewarmness on the part of the government toward marine research, or should rather be looked on as a most unexpected and welcome recognition of the growing importance of science and scientific men to the affairs of the nation. Let us adopt the latter view, and accept the heavy responsibility attached thereto.

Anyone who will take the trouble to read, in the Proceedings of the Royal Society of London, the account of the discussion which recently took place on "the scientific advantages of an antarctic expedition," will gather some idea of the number and wide range of the subjects which it is urged should be investigated within the antarctic area; the proposed researches have to do with almost every branch of science. Unless an earnest attempt be made to approach very near to the ideal there sketched out, widespread and lasting disappointment will certainly be felt among the scientific men of this country. The proposed expedition should not be one of adventure. Not a rapid invasion and a sudden retreat, with tales of hardships and risks, but a scientific occupation of the unknown area by observation and experiment should be aimed at in these days.

I have all along estimated the cost of a well-equipped antarctic expedition at about £150,000. I see no reason for changing my views on this point at the present time, nor on the general scope of the work to be undertaken by the proposed expedition, as set forth in the papers I have published on the subject. There is now a sum of at most £90,000 in hand, or in view. If one ship should be specially built for penetrating the icy region, and be sent south with one naturalist on board, then such an expedition may, it will be granted, bring back

interesting and important results. But it must be distinctly understood that this is not the kind of exploration scientific men have been urging on the British public for the past fifteen or twenty years. We must, if possible, have two ships, with landing parties for stations on shore, and with a recognized scientific leader and staff on board of each ship. Although we can not have the royal navy, these ships can be most efficiently officered and manned from the mercantile marine. With only one ship many of the proposed observations would have to be cut out of the programme. In anticipation of this being the case, there are at the present moment irreconcilable differences of opinion among those most interested in these explorations as to which sciences must be sacrificed.

The difficulties which at present surround this undertaking are fundamentally those of money. These difficulties would at once disappear and others would certainly be overcome should the members of the British Association at this meeting agree to place in the hands of their president a sum of £50,000, so that the total amount available for antarctic exploration would become something like £150,000. Although there is but one central Government, surely there are within the bounds of this great Empire two more men like Mr. Longstaff. The Government has suddenly placed the burden of upholding the high traditions of Great Britain in marine research and exploration on the shoulders of her scientific men. In their name I appeal to all our well-to-do fellow-countrymen in every walk of life for assistance, so that these new duties may be discharged in a manner worthy of the empire and of the well-earned reputation of British science.

RELATION OF MOTION IN ANIMALS AND PLANTS TO THE ELECTRICAL PHENOMENA WHICH ARE ASSOCIATED WITH IT.[1]

By J. Burdon-Sanderson, M. A., M. D., F. R. S.

In a Croonian lecture which I delivered to the Royal Society in 1867—more than thirty years ago—I exhibited a number of diagrams of graphic records, in evidence of the mechanical relations which I then sought to establish between the movements of the heart and those of respiration in the higher animals.

I have to-day to bring before you results which have also been obtained by a graphic method, which, however, differs from the other in that the records are written by light, and not by pen on paper; that the time taken in recording is measured in thousandths of seconds, not tenths; and finally, that the events recorded are not the movements of the chest or heart, but the electrical changes which, as will be shown, are found to associate themselves with all manifestations of functional activity in living organisms, whenever these take place under conditions which admit of their being investigated.

Our purpose is to consider the relation of two coincident and concurrent processes, with reference to which we make at the outset the assumption that one is functional, the other concomitant, using the word "function" in the biological sense to imply the doing by an organ of the work for which it is adapted. In the observations which I have made from time to time during the last twenty years relating to the electrical phenomena of plants and animals, it has always been my endeavor to regard them exclusively in relation to the functional activity of the structures in which they manifest themselves. In investigating the function of a living organ or organism, you have to do with a machine that you can not take to pieces, and it is often the best way to observe how, after its action has been arrested, it goes on again. To do this under experimental conditions is one of the most frequently used methods of the physiologist. The possibility of employing it

[1] Croonian lecture before Royal Society of London, March 16, 1899. Reprinted from Proceedings of the Royal Society, Vol. LXV., No. 413, pp. 37–64.

depends on the circumstance that all animal organisms and certain parts of plants possess the faculty of being awakened from a state of rest to normal activity.

Even under the most favorable conditions the observation of this transition is attended with difficulties which arise from the complexity of the chemical and mechanical changes, and the shortness of the time spent in their accomplishment. It is this crowding together of chemical, thermal, and electrical phenomena into a very brief period which determines the method for their elucidation, a method consisting to a large extent of a determination of time relations, i. e., of the order of succession of phenomena; for it is evident that when you have to do with a number of events which appear to be simultaneous, the most effectual way to determine their causal relations is to ascertain the order of their occurrence. For, inasmuch as one event which follows another can not be its cause, the proof of their sequence which accurate time measurement affords may be of infinite value in indicating where the starting point in a complex series of changes is to be sought for.

The inquiry as to the relation between functional activity and the electrical phenomena accompanying it can only be entered upon by finding instances in which both processes can be observed together. Among these, those are to be preferred in which the question presents itself in its simplest form, the experimental conditions can be most easily controlled, and the observations can be made with the greatest exactitude.

It might at first sight seem desirable to begin by describing the electrical manifestations of functional activity in the simplest organisms and organs. There are, however, important reasons for following the reverse order. To do so is in conformity with the general rule that a problem can be most easily solved when it presents itself in its simplest form. In the lowest organisms the relation of function to structure, so far from being simple, is necessarily very complex, for functions of the most varied kind have to be discharged by one and the same mechanism, and often in default of any mechanism at all that we can discover; whereas in the higher plants and animals we find for the most part that every kind of work has its instrument, every action its agent. It is in the highest organisms, therefore, that elementary physiological questions must be studied, and it is in them that they have been most studied.

Of the elementary vital functions, motion was the one fixed upon as the subject of this lecture by its founder. Its fitness for our purpose is preeminent. Motion, in the physiological sense, is simple, controllable, measurable. It is, moreover, a function of paramount importance as the means by which the animal organism maintains its relation to the external world. In the higher animals muscle is the instrument of motion and therefore claims our consideration. It has, in addition,

the advantage of being a structure of which the chemical, thermal, and mechanical properties are better known than those of any other. This advantage applies particularly to the muscles of the frog, which on that account, as well as on the grounds which have been the occasion of their being most studied, are to be preferred for our present investigation. What we have first to do, therefore, this afternoon is to determine the relation between the electrical concomitants of muscular action and muscular action itself; but before entering upon it I must occupy you for a few minutes in stating what is at present most certainly known as to the nature of that action.

When a muscle is roused to activity by the presence of an exciting cause, its mechanical properties suddenly change. It shortens, and if the shortening is resisted becomes tight. If the resistence is such as can not be overcome it tightens without shortening. With reference to this mechanical change, we know that it is dependent on chemical change, and that that change is oxidation. Admitting these propositions, we must necessarily believe that the oxidation is sudden, i. e., explosive, because its mechanical effect (the tension or tautness I have mentioned) attains its maximum at a very short period after the moment at which the process begins.

At ordinary temperature we find that in a whole muscle the tension which is induced by an excitation attains its maximum in about three one-hundredths second. But if we fix our attention on a single muscular element, i. e., on one of the infinite number of molecular mechanisms by the cooperation of which the mechanical change consequent on excitation is brought about, it can be shown that in each taken separately a much shorter duration than three one-hundredths second must be assigned to the process of transition. According to Bernstein, less than one one-hundredth second must be assigned to the chemical process which takes place in a muscular element in response to a single stimulus.[1]

This chemical process of extreme suddenness is followed without measurable loss of time by the conversion of chemical energy of the oxidizable material into mechanical energy, which immediately manifests itself either in shortening or in the effort to shorten. The way in which this transference takes place must for the present be left an open question, for, as Professor Engelmann explained in the Croonian lecture for 1895, the transformation of chemical into mechan-

[1] See Pflüger's Archiv, vol. 67, p. 211, 1897, where Bernstein describes his method of measuring the period of latency. As in the method described by me in the Journal of Physiology in 1895, a magnified image of the moving surface of a muscle excited directly is received on a slit, behind which a sensitive plate passes. From the curve so obtained Bernstein determines the moment at which the rate of expansion increases most rapidly, and regards this as the moment at which the moving force is at its maximum. This conclusion, of course, applies to the part of the muscle immediately excited.

which constitute muscular action before us, it will be our purpose to compare with this order that of its concomitant electrical phenomena. Before I proceed with this comparison it is desirable to say that it should be understood that no reference will be made to electrical theory. We merely derive our modes of observation and of measurement from the exact sciences and aim at the utmost attainable precision; but the phenomena have their chief interest as outward and visible signs of intimate vital processes, of which they afford us the only knowledge that is within our reach.

We choose as our subject of observation a muscle of nearly symmetrical form—a band of parallel fibers. We explore its electrical state by a conducting arch containing a galvanoscope, the ends of the arch being in contact with its surface. If the muscle is no longer living, the galvanoscope gives no evidence of current. If it is living, there is again no current, provided that the two surfaces are in the same physiological state. If one is less living than the other, the fact is indicated by a difference of potential between them, a current flowing through the galvanoscope from the more living to the less living. Vitality is, therefore, here indicated by difference of potential. By vitality we mean nothing more than the capacity for discharging function. This capacity diminishes by discharge, i. e., by activity. Accordingly we find that when, for any reason, the muscular substance at one part becomes more active than the muscular substance at another the former becomes negative to the latter.

Every observation of the electrical phenomena of muscle (or of any other excitable structure) relates either to the state of capacity for action (called in physiology "rest") or to the state of action or discharge. In either case it consists in comparing the states of two contacts,[1] i. e., of two parts to which electrodes of a galvanoscope are applied. It is obviously desirable for the investigation of the changes at either that those which take place at the other should be annulled during the period of observation. On this consideration a rule is based, to the mode of carrying out of which I will advert presently.

Most of the results which I shall place before you were obtained with the aid of the capillary electrometer, of the use of which as an aid to electrophysiological investigations I brought before the Royal Society some instances nearly twenty years ago. Its application has since been studied with great completeness by Mr. Burch, to whose skill I am indebted for the instruments which I have used for my work during the last ten years, and more particularly for the one which has enabled me to submit to you the photographic results I am now about to exhibit. These photographs, I need scarcely explain, express the

[1] It may be well to note that the contacts referred to here and elsewhere are made by means of nonpolarizable electrodes of the kind originally devised by Du Bois-Reymond and always used in physiological work.

excursions of the meniscus of the mercury column as a sensitive plate moves rapidly past the slit on which it is projected, each upward movement of the image indicating that the surface of contact connected with the mercury has become at that moment positive to the other.

I do not propose to give this afternoon even the shortest description of the instrument, and I should not occupy time in explaining why it answers my purpose so perfectly were it not that with the exception of Professor Einthoven and Dr. R. Du Bois-Reymond, the leading authorities on the other side of the Channel, and particularly Professor Hermann, have condemned it as an instrument of which the defects are essential and irremediable. As I have answered these criticisms elsewhere, I need only say here that for the investigation of the order and duration of a rapid succession of electrical changes such as these with which we are now concerned the instrument surpasses all others and that by means of it my colleague, Professor Gotch, has, with Mr. Burch's aid, successfully photographed phenomena in nerve of which the very existence could not be demonstrated a few years ago.[1]

The purposes to which we apply it are (1) for the measurement of intervals of time between electrical changes which succeed each other with great rapidity, and (2) the obtaining an estimate of their relative intensities. The properties which make it so invaluable to us are (1) that it responds to the action of a current promptly, beginning when the current is closed, and indicating every change in its strength or direction without measurable loss of time; (2) that *coet. par.*, the rate of ascent is proportional to the electromotive force of the current which produces it, and (3) that the instrument can be graduated, and its graduation verified by comparison with instruments of greater precision, and thus used for the measurement of differences of potential of longer duration.

The diagrams 1, 2, and 4 illustrate the bearing of these three properties on the cases we have to investigate. As we shall see, a muscle can be brought into action either by an instantaneous stimulus, by a series of stimuli, or by continuous stimulation.. Each of these has its mechanical and its electrical response. I will anticipate so far as to say that the three forms of electrical response correspond to the three forms of mechanical. They correspond to the changes indicated by the black lines in the three diagrams. I will further premise that all

[1] Full information relating to the instrument will be found in Mr. Burch's work. The Capillary Electrometer in Theory and Practice, and his papers in the Proceedings (vol. 48, 1890) and Transactions (A, vol. 183, 1892) of the Royal Society. A very perfect method of recording the excursions of the electrometer photographically and of interpreting the curves was described by Professor Einthoven in Pflüger's Archiv in 1894, and applied by him to the investigation of the electromotive phenomena of the human heart. It need scarcely be added that the two methods are the same in principle. An important paper has also recently been published by Dr. R. du Bois-Reymond in the Archiv f. An. u. Physiol., 1898, p. 516.

known excitatory responses—all electrical changes which are concomitants of action—may be compared with one of these types.

Case 1.—Response to a continuous stimulation. A difference of potential comes into existence at the contacts at the time t, and persists long enough to produce its full effect on the column. (Diagram 1.)

Case 2.—Series of short continuous stimulations. The column moves in alternately opposite directions. (Diagram 2.)

Case 3.—Response to a single instantaneous stimulation. A difference of potential comes into existence abruptly, and subsides abruptly at first, afterwards less rapidly. (P′ in diagram 4.)

Now, I have found that in the study of my experimental results it is of great advantage to proceed *a priori*. Let us assume that there are three types of stimulation, and that each has its form of response. We can best begin by inquiring to which of these three forms the

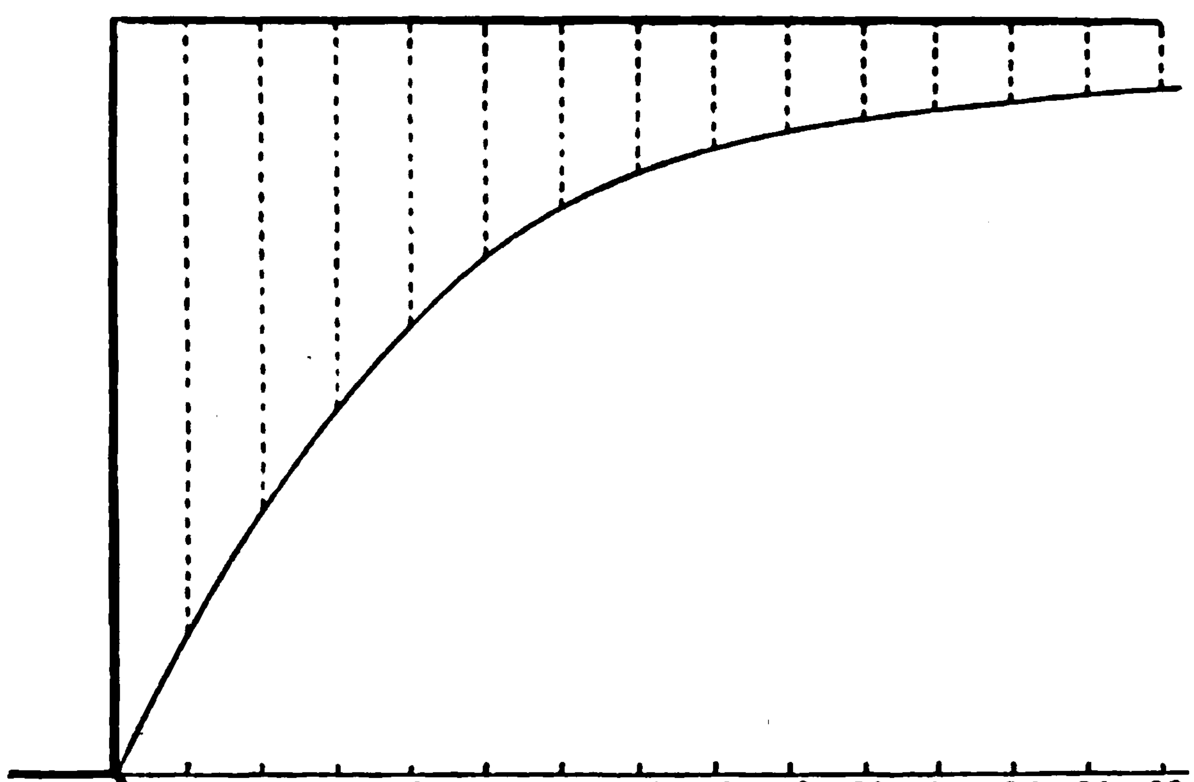

Diagram 1.

observed variation belongs, and then determine in what respects it conforms with, or differs from, the type.

In the diagrams, I have shown the types of photographic curves which correspond to the three forms of response to stimulation I have indicated. The faint lines represent photographic curves; the strong, variations of potential difference. In each diagram the strong and the faint lines have been drawn in their true mathematical relation to each other, i. e., so that the vertical distance apart of strong from faint is everywhere proportional to the gradient or slope of the photographic curve, the proportion being such that if the electro-motive force of the current acting on the electrometer varied according to the strong line, the movement of the head of the mercury column would be expressed by the faint line. We shall see as we proceed that one or other of the three forms of photographic curve, which correspond to the three forms of electrical change, just designated as typical, presents itself

in every excitatory response we have to investigate, provided that, as I mentioned just now, the changes under one contact only are recorded.

To insure this the exploring contacts must be so arranged and the muscle itself so prepared as to enable us to separate the part of the surface we desire to investigate from the rest, so far as concerns its effect on the instrument we are using as indicator. It is obvious that when we apply our leading-off electrodes to two parts of the surface, both of which are at the same time undergoing change, there must always be a difficulty in determining how far the effect is due to changes at the one or at the other contact. It is therefore essential for the correct observation of an electrical change at one of them that the other should be protected from disturbing influences.

THE FIRST FUNDAMENTAL EXPERIMENT.

An experiment will show how this may be accomplished. It will also bring us face to face with a phenomenon which is, perhaps, the most fundamental of those which at present concern us—the phenomenon of the wave of excitation, or, to use the designation given to it by its discoverer, the Reizwelle. The nature of the experiment is illustrated by diagram 3, in which the band of parallel fibers represents the sartorius muscle. It is excited (instantaneously) at *r*. A change occurs there which is propagated first to the proximal contact *p*, and then onward to the distal contact *d*, at a rate which in our preparation may be 150 centimeters per second. This change is essentially a vital one, but it is attended by a mechanical change represented by the muscle curve and an electrical change, which we record photographically. Diagram 4 will serve to explain what (as will be immediately seen) actually happens at the moment the wave passes under *p*. It means that a current suddenly appears there, of which the direction is from *p* to *d*. When the wave reaches *d*, a second effect of the same kind (*D'*) occurs, of which the direction is opposed to the first. What the galvanoscopic effect of this must be is easily understood from diagram 4, in which the two curves *P'* and *D'* are placed in a relative position to each other which expresses their time relation. The two effects sum together. In the diagram the curve *S'* expresses the result of that summation, i. e., the actual variations of difference of potential between the contacts which occurred while the wave was

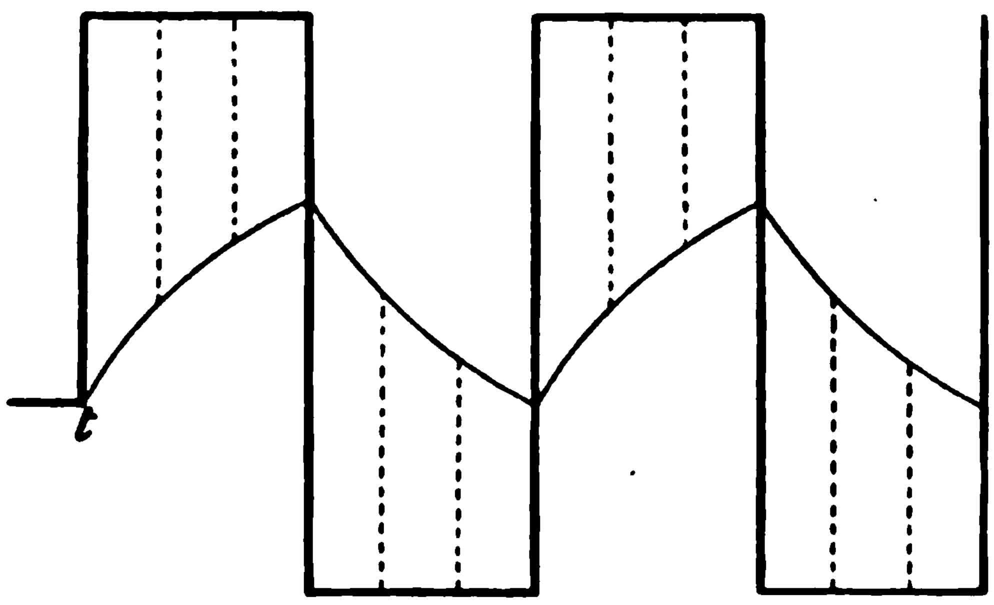

Diagram 2.

passing from p to d. It will be seen at once why we call this effect the diphasic variation.

I explained before that in accordance with the fundamental properties of our instrument the curve P' would have as its photographic expression the curve P. Similarly the combination curve S' would have for its photographic counterpart the curve S. May I emphasize the point that, if you have the curve P' of a parallel-fibered muscle, you can calculate from it S' and consequently S, but that from S alone you can not deduce the others. In other words, if you know the form of P', you know everything as to the form of the electrical response—the Reizwelle.

Let us now take the actual result. As before stated, the two contacts are at p and d, and the muscle is excited at r. The wave affects the muscle first at p, then at d, and the consequent movement of the column is photographed (photograph 1, Pl. I).

You recognize that it is the counterpart of the deduced curve S. In other words, it is the expression of the effects of two similar processes

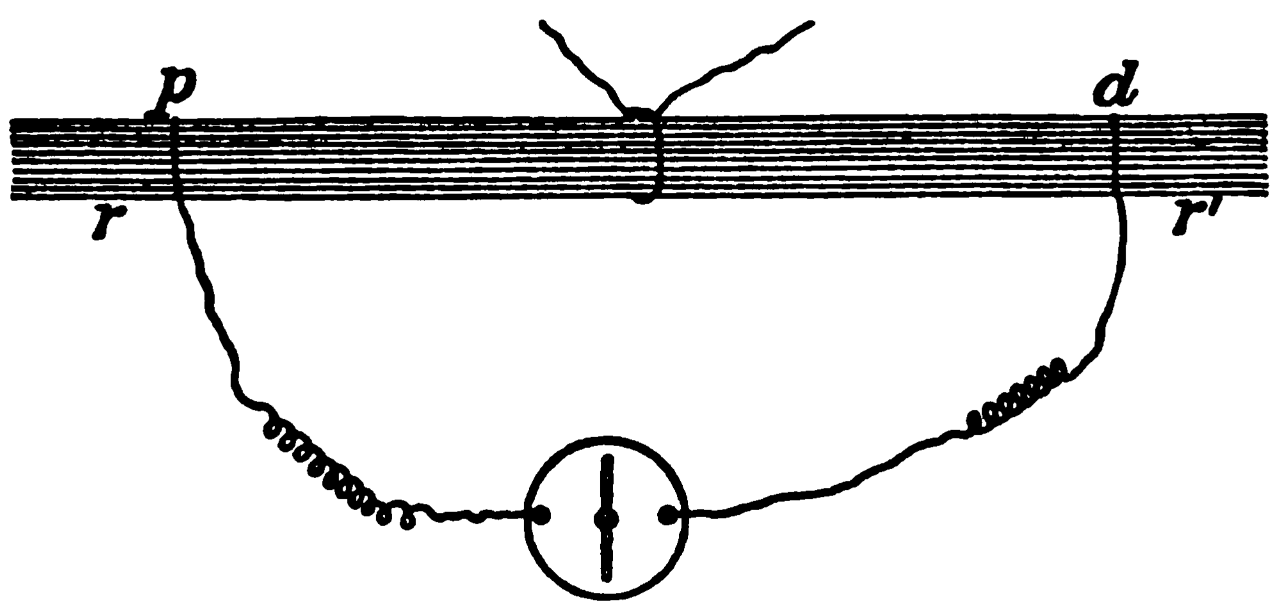

Diagram 8.

having their seats at the two contacts. Our aim must now be, as I have explained, to annul or suspend the effect of one of them, leaving the other intact. The method is simple. After having obtained the record I have shown you, I tie a fine thread around the muscle between p and d. I tighten the ligature so as to constrict the muscle and again record the variation. There is no change of effect, for the wave is still able to pass the constriction. I tighten again; it still passes. I then draw the ends of the ligature hard, and again photograph. I find the photographic curve is no longer S but P, i. e., it has assumed the characteristic form of the monophasic electrometer curve (photograph 2, Pl. I).

What has the ligature effected? It has exercised no influence on either contact, but it has arrested the progress of the excitatory wave, so that its effect at p only is manifested, and not that at d. The relation between the two curves (P and S) is obvious enough when they are seen in succession. It will be still more obvious if I place them on the

screen together, in such a way that they are in synchronic relation to each other (photograph 3, Pl. I).

The experiment may be further varied by altering the seat of excitation from r to r'. You thus obtain a photographic record which represents what happened at d in the unligatured muscle. If the muscle is in a normal state, this is an exact reversed counterpart of photograph 2.

If instead of placing the ligature half way between p and d, we place it close to the distal electrode d, the proximal may then be placed in a succession of experiments at different distances from the seat of excitation without altering the form of the recorded variation; the time at which it begins depends in each case on the distance of the proximal contact from the seat of excitation.[1]

In all of these instances the ligature acts as a block. Without interfering with the condition of any other parts it kills the part which it grasps and makes it incapable of transmitting the excited state from the living structural elements on one side to those on the other; but if we compare the condition of the unexcited preparation immediately before and after the application of the ligature we find evidence that breach of continuity of function is not the only effect produced by it. If the one contact is placed on the ligatured part it is found that, irrespectively of any excitation, there exists a large difference of potential between the contacts, which may amount to four or six hundredths of a volt.

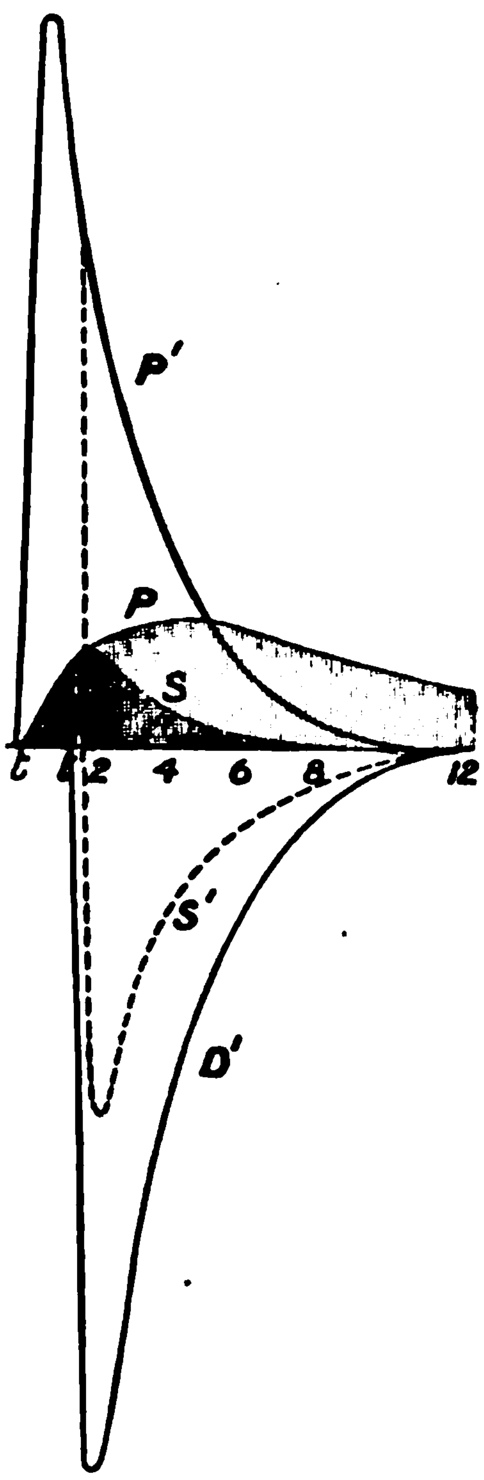

Diagram 4.

THE MUSCLE CURRENT.

Now it is easy to prove that this difference is not due to breach of continuity, for if you shove

Explanation of Diagram 4.—The horizontal line is that of equipotentiality of the two surfaces of contact p and d. The curve P' expresses the relative negativity (negative difference of potential) of the surface p; the curve D', the corresponding relative negativity of the surface d. S is a curve of which the ordinates are the algebraic sums of the corresponding ordinates of P' and D'. S is the photographic curve which expresses S'; P' is the photographic curve which expresses P. The numbers under the horizontal line indicate hundredths of a second. The distance $t\,t'$ expresses the time taken by the wave in its progress from p to d.

[1] An experiment of this kind is by far the most exact method which we possess of measuring the conduction-rate in muscle. This rate is most correctly expressed by the quotient:

$$\frac{\text{Difference between the distances}}{\text{Difference between the times}}$$

as measured in two experiments in which the distances are different.

the electrode away from the ligature in either direction it disappears. The phenomenon which is thus brought to light is that to which the great founder of animal electricity, Du Bois-Reymond, applied the term "muscle current," and when the method I have described is employed it presents itself in its utmost simplicity, for by the act of tightening the ligature previously applied under an electrode you at once bring into existence a state of things in which the constricted part is negative to the living parts on either side.

What happens in this case? What is the difference between the state of the surface of contact immediately before and immediately after the tightening of the ligature? Nothing more can be said than that a certain process which was going on there and which provisionally we call "life," being ignorant of its nature, has been annulled. What we actually observe may be represented diagrammatically thus:

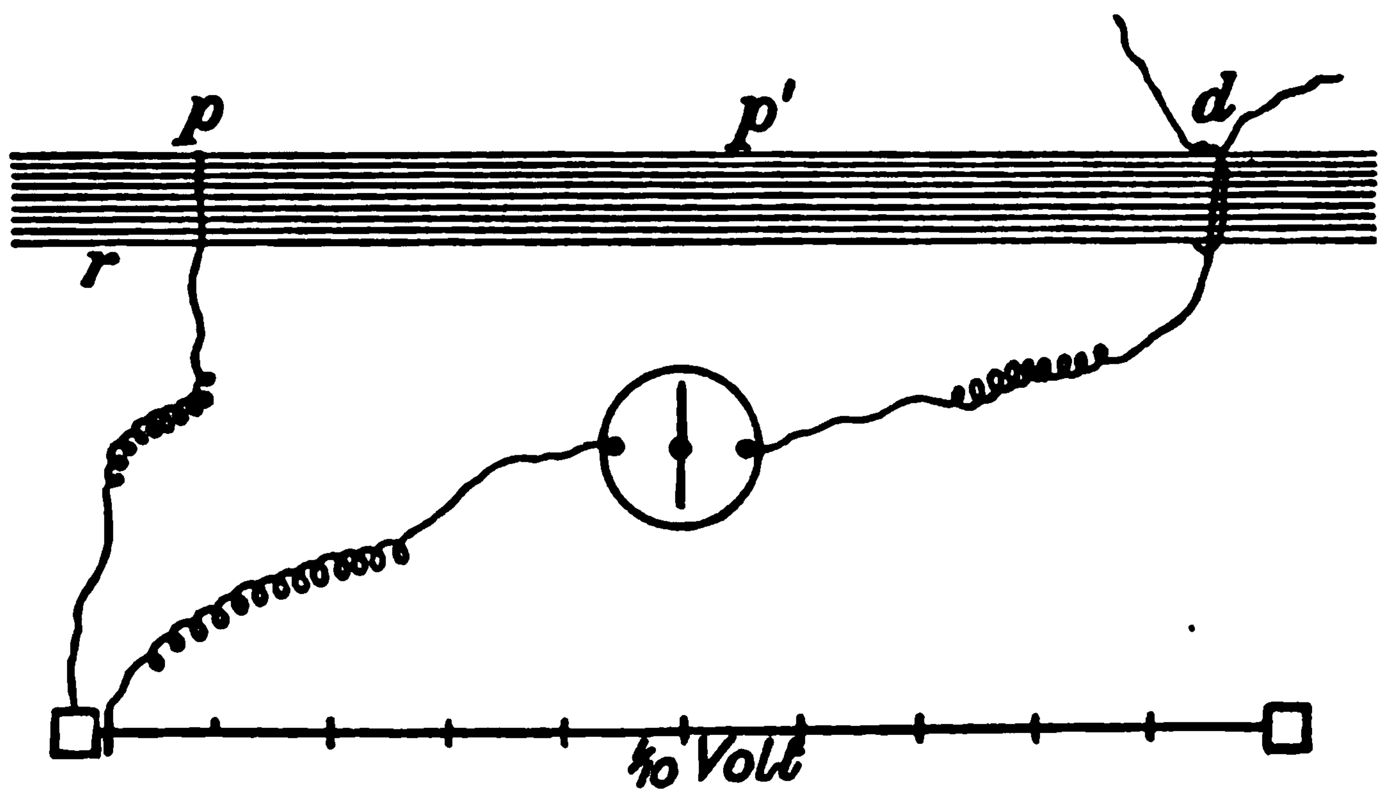

Diagram 5.

The divided line represents the graduated wire of a potentiometer; at *d* is a ligature as yet not tightened round a muscle; *p* and *d* are equipotential. The galvanometer is at zero and the slider of the potentiometer is up to the block. The ligature is tightened; at once the needle indicates a current directed from *d* to *p*, but can be brought by the slider again to zero.

The contacts are as shown in the diagram. Before tightening the ligature between them they are equipotential, because they both rest on muscle in the same physiological state. I represent the electrical concomitant of that state by an arrow, by which I mean nothing more than that if it were possible to connect *p* with some other part of the muscle, without passing through another electromotive surface, there would be a current in that circuit from *p* to the galvanometer. But inasmuch as the actual circuit passes through *d* where the same conditions exist as at *p*, but opposed in direction, there is no current. If by tightening the ligature I annul the effect of *d*, the effect of *p* comes into evidence. This statement is simple, and seems to arise naturally from the observed facts, but can not be received without question, for

Photograph 1.

Photograph 2.

Photograph 3.

1, 2, 3. Curarised Sartorius kept for about twenty-four hours in 0.6 per cent solution of chloride of sodium. Temperature during observation 9° C. Contacts, etc., as in diagram 5, but p much nearer to d.

it suggests that what we call the "demarcation current" has its seat, not at the surface of demarcation, but at the living surface, so that we should have to consider the state of "Stromlosigkeit" not as a state of electrical inaction, but as a state of balance.

A similar question would arise as regards the response to excitation; for when, as we have seen, the Reizwelle passes under the proximal contact (exp. 1), what happens there (during the one-hundredth of a second that it is passing) is analogous to what I have just described as the effect of suddenly tightening a ligature at that spot. The moment before excitation a state of balance existed between p and d. As the wave passes under p it upsets that balance by annulling the outgoing current, then pursues its course until it is extinguished by the ligature. From the moment that the tail of the wave has left the edge of the surface of contact behind it has no action whatever on the indicating instrument. We have evidence of this in the curve of variation itself, for the form of the curve is the same whether the wave is blocked by the ligature at one centimeter from the point of observation or at three, which could not be the case if, as I once imagined, something happened at the moment of extinction.

The complete proof that this is so is, however, obtained by another form of experiment in which the seat of excitation (r) is shifted from the proximal side of p to the proximal side of d. The unligatured and, therefore, equipotential muscle is excited in the two positions successively. The results show (1) that the excitation wave is propagated in both directions, and (2) that the form of the curve varies according to the order in which the electrodes are reached. This having been determined, the progress of the wave is stopped by a ligature under the distal contact d and the excitations in the two positions repeated. It is now seen that the form of the wave is the same whatever the direction from which it approaches the point of observation p. When the excitation is proximal to d, it is not now anticipated by a variation at d, and there is consequently a long delay (see photograph 4, Pl. II), during which the galvanometer is unaffected. The experiment affords direct evidence that, although the whole muscle is in circuit, the presence of the wave cannot reveal itself until it is under the electrode. As regards the action current, therefore, the electromotive source is always the surface of contact of the leading off electrode with living substance, not the surface of contact between dead and living.

We may now [illegible] of the form of the propagated monophasic [illegible] wave. It would be easy to prove by the ex[illegible] photographs of the monophasic variation relating [illegible] the same charac[illegible] teristic features [illegible] change culminates [illegible] of a [illegible] excitation, according [illegible]

time it has been kept, subsiding at first abruptly, afterwards more gradually, so that its whole duration (i. e., to the summit of the electrometer curve) amounts to from two to six hundredths of a second.

The discoverer of the Reizwelle, Professor Bernstein, assigned to it a very different duration. "In every element of muscular structure the variation lasts between one two-hundred-and-fiftieth and one three-hundredth second, and coincides with the period of latent stimulation." At first sight this statement seems irreconcilable with fact, but it is much less so than it appears to be. We have only to assume that Bernstein's method of estimating a small and transitory difference of potential between two surfaces was not sufficiently delicate to enable him to appreciate those which exist during the period of decline, and that what he regarded as the duration of the whole variation was in reality the duration of its summit only. However this may be, it is clear that we may divide the period of variation into two parts, which we may call, respectively, the initial rise and the decline, of which the latter lasts eight to ten times as long as the former, and that we may regard the first as a period of upset, the second as a period of restoration. Taking the period of upset as equivalent to Bernstein's "negative Schwankung," we can accept all he says as to its coincidence in time with the moment of greatest intensity of the process by which chemical is transformed into mechanical energy—the moment in the shortening of an unloaded muscle at which its rate of change increases most rapidly. As regards the period of decline, it might suggest itself that the return of each element to its previous state is in every instance the expression of an anabolic process, not merely a result of the cessation of the opposite process. The facts we are considering, however, lead us for the present to regard the whole variation as the concomitant of one and the same chemical process, and we are confirmed in this view by the observation that, as we shall see immediately, the modifications which the monophasic variation undergoes under external or accidental conditions affect both stages equally.

Of these conditions one of the most important is temperature, particularly when muscles which have been kept for some time in physiological salt solution are used.[1] We have hitherto had in view the Sartorius which has been kept for some twenty-four hours and is at the temperature of about 10° C. By placing it in a cooled chamber at a temperature some 6° C. lower and allowing it to remain there until it has acquired the temperature of its environment its mode of responding is not changed, but only in its relation to time. In shortening it takes a longer time to attain its minimum length, and if its contraction is resisted its period of effort is of longer duration. Consequently it is able to do more external work in a single effort than before, although it is not able to support a heavier weight or maintain

[1] Journ. of Physiol., vol. 23, p. 332.

a greater tension in a continuous effort. Now, all these modifications depend, so far as I have been able to ascertain, on diminution of the rate of propagation of the excitatory wave. As has been already stated, we are able to measure this rate with great facility and accuracy. By alternately cooling and warming our chamber we can determine in any number of instances the change of rate which a difference of 2°, 4°, or 6° C. produces, and compare the data so obtained with the effects of the same changes on the duration of the monophasic variation and on that of the mechanical effort which it accompanies.

Up to this point the phenomena we have had under consideration have been associated with the response of a muscle to a single instantaneous excitation, i. e., the monophasic variation and the monetary contraction which it ushers in. We must now pass on to the consideration of the electrical concomitants of those forms of contraction which more obviously resemble the natural action of muscles.

Physiologists have for half a century taught that natural muscular action, whether reflex or voluntary, is made up of single contractions of definite duration, such as those we have been considering, i. e., of a rhythmical series of such contractions of definite frequency. This doctrine—that voluntary motion is a well-organized system of twitches—is now commonly expressed by calling it a tetanus, a word which was some fifty years ago diverted from its medical signification to be adopted as a technical term in physiology, but not precisely in its present sense. What is now meant by it is that every contraction, however continuous it may appear to be, is in reality discontinuous. This conclusion was arrived at by a method which, though sometimes of great value to the physiologist, does not always lead to the discovery of truth—the method which consists in first imitating a natural process and then mentally transferring the characteristics of the imitation process to the natural process which it represents. In the present instance the study of artificial tetanus has taught us a large proportion of what we know as to the properties of muscle, but not much about voluntary contraction. In assuming the identity of the latter with experimental tetanus physiologists have perhaps minimized certain fundamental difficulties and assigned undue value to certain analogies.

Of the difficulties, the most obvious one is that discontinuity could not, if it existed, be of any advantage. For if we regard the muscular system as the mere instrument of the central nervous system and every muscular fiber as the instrument of the motor cell which governs it, it is difficult to see how subjecting that muscular fiber to a rhythm of its own could have any other effect than to interfere with its efficiency. Of the analogies, the chief is, first, that just as when you listen to a muscle in artificial tetanus you hear a musical sound of which the frequency of vibration corresponds to that of the stimuli, so a muscle when contracting voluntarily gives out the quasi-musical

sound which Wollaston compared to the rumble of wheels over pavement; the other analogy relates to the reflex spasm of strychnine, which is not only rhythmical in itself, but is accompanied by a series of electrical changes which are as rhythmical as if they were evoked by a series of stimuli. The discussion of the muscle sound lies outside of our present inquiry; the spasm of strychnine will be considered after we have examined the electrical concomitants of artificial tetanus.

SECOND FUNDAMENTAL EXPERIMENT.

The point to which I have first to draw your attention is the form of photographic curve which is obtained when the sartorius, injured under one electrode by a ligature, is excited by a series of stimuli of which the frequency is about 60 per second. The photograph (6 Pl. II) shows that the column rises at first abruptly, but afterwards in such a way that the rate at which it rises is at any moment proportional to its distance from the point to which it will eventually arrive, i. e., to the distance between the corresponding point of the curve and its asymptote. The electrical state, therefore, which comes into existence when a muscle is tetanized (i.e., subjected to a frequent series of excitations) corresponds to diagram 1. In other words, the electrometer is acted on by the same difference of potential between its terminals throughout, with the exception that the effect of the first, or first couple, of excitations is often greater than that of the succeeding ones. Although this hardly needs proof, it can be easily verified by direct experiment. With this view our circuit is so arranged that we can, without altering the resistance, project onto a second photographic plate the effect of allowing a constant difference of potential to act on the mercury column just as the plate is passing behind the slit. On comparing this curve with the tetanus curve they are found to be nearly identical.

Let us now take the case in which a muscle is tetanized in the same way as in the last instance for a succession of periods of one-fifth second, alternating with equal periods of rest (photograph 7, Pl. III). The complete correspondence of the photographic curve with that represented in diagram 2 indicates that the conditions correspond with those which are there theoretically represented. During each period of excitation (tetanus) the movement upward of the meniscus is determined by the difference of potential. During the intervals it follows in its fall the similar curve of depolarization.

From this we may now proceed to other forms of experimental tetanus in which the excitations are less frequent. Provided that the frequency is not much less than 40 per second, the general contour of the curve resembles the other one, with the exception that the effect of each excitation is seen separately (photograph 8, Pl. III). If the frequency is diminished to 20 per second, the undulations are more ample,

while the curve rises to a lower level, the reason obviously being that the electrometer is acted on by a smaller number of excitations in a given time.

Diminishing the frequency still further (to 14 per second), we obtain a curve (photograph 9, Pl. III) of which the character is that of a series of equal and similar monophasic variations.

THE REFLEX ELECTRICAL RESPONSE.

We now go on to compare the variation curve of artificial tetanus with the nearest approach to a normal contraction we can obtain for investigation, viz, the reflex response of the motor apparatus of the spinal cord to an instantaneous stimulation of the cutaneous surface. A ligature is applied as before to the tibial end of the sartorius under the distal contact; but inasmuch as the muscle must now be excited through its nerve, the proximal leading-off contact is on the hilus. The mode of excitation is the same as before, but in this case the effect has first to be communicated to the motor cells of the spinal cord through the sensory apparatus, a process which occupies a relatively considerable length of time. The motor cells then deal with it automatically, responding to it in their own way and inducing in the muscles under their control an action which is the faithful and exact expression of the changes going on in themselves.

As is well known, it is not possible in a normal preparation to obtain an unfailing response to an instantaneous stimulus applied to the cutaneous surface, but the previous injection of a trace of a strychnine salt (e. g., one-thirtieth milligram of the sulphate) is sufficient to give to the motor apparatus of the cord the required degree of excitability. A single induction current applied to the skin then evokes in the sartorius and other muscles first a twitch which resembles the response of the same muscle to a similar stimulus applied to its nerve; a little later this twitch is replaced by a short, sometimes thrilling, spasm resembling a short tetanus. What I have to show you is that, although the reflex spasm resembles a short artificial tetanus as regards the way in which the muscle contracts, the contractions are shown by their electrical concomitants to be of a different nature. The strychnine spasm, as it is rightly called, is seen not to be a tetanus, i. e., not to consist of a series of single twitches, but to be a succession of continuous contractions, the rhythm of which depends on the spinal cord, not on the muscle.

The grounds on which this conclusion is founded appear to me to be unequivocal. The observation is a simple one. The automatic mechanism, which carries the photographic plate, liberates as before, at the beginning of the period of exposure, an induction current which pricks the skin of the preparation. After an interval which may be

about a tenth of a second (during which a quasi psychological process is going on in the spinal cord) the muscle responds. A curve is drawn simultaneously by the writing lever to which the end of the muscle is attached, which indicates that it is in spasm;[1] but it is the photographic curve which tells us the nature of that spasm. Each ascent of the meniscus is seen to be the response, not to a single instantaneous, but to a short continuous, stimulation, of which the duration can be easily deduced by measuring the time interval between the beginning and the culmination of an excursion. By subjecting the muscle artificially to series of excitations of similar duration with corresponding intervals of inactivity one can produce an imitation of the strychnine spasm which, both in its mechanical and electrical characters, resembles the natural one. (See photograph 7.)

Before leaving the subject of the strychnine reflex I must refer very briefly to such previous observations as bear on our present inquiry. The phenomenon is of interest as being one which could not have been discovered had we not possessed the capillary electrometer. Its discovery was, indeed, the outcome of the first attempt made by Prof. C. Lovén to use that instrument for the investigation of the electrical properties of muscle just twenty years ago. He was good enough to make for me the electrometer which was used in some of my own earliest experiments. Shortly afterwards Mr. Page devised the method of obtaining photographic records of our own results and, amongst others, of those of Lovén relating to the strychnine spasm. Lovén's observation has served ever since as a support for the doctrine of discontinuity. No one would be more willing than he would, if he were with us this afternoon, to recognize its true meaning.

The conclusion to which all the facts we have had before us up to this moment lead is that normal muscular action is the manifestation of what happens in the motor nervous system. If this motor impulse is so short that we are obliged to call it "instantaneous," the response is correspondingly brief; if it lasts longer, we call it "continuous," recognizing that the difference between the two is merely one of duration. In either case it is of the essence of the response that it is terminable. There is no difficulty in understanding on teleological grounds why a muscle must relax; but of the mechanism by which it is brought about we know little, excepting that it is localized in the muscular structure. Each element—each tagma—returns to its status quo in the same way in a curarized muscle as in a normal one; but whether this power of recovery is a process by itself, as some physiologists hold, is a question which is at this moment debated, but by no means settled. It is only in so far as it relates to the electrical con-

[1] The curve is often toothed, the teeth corresponding in frequency with the electrical undulations.

Photograph 7.

7. Frequency of excitation as in photograph 6. The original shows similar undulations in the ascents, which the copy, by inadvertence, does not show.

Photograph 8.

8. The first four undulations have been imperfectly copied.

Photograph 9.

comitants that it here concerns us. Without prejudice to the question whether, as Fick and Gad maintain, the relaxation of a muscle is dependent on a special chemical process or not, it falls within our present scope to inquire whether by comparing with a normal muscle one which not only does not relax, but has been deprived of the faculty of relaxing, we can arrive at any electrical indication of such a process. Fortunately we have within reach a means by which this experiment can be made.

THE CONTINUOUS RESPONSE OF A VERATRINIZED MUSCLE.

The alkaloid veratrine[1] is an agent by which a muscle excited by an instantaneous stimulus is deprived of its power of recovering itself. The quantity of the alkaloid required to produce the effect is extremely small. The addition of one part in a million of veratrine to the physiological salt solution in which a muscle has been kept for several hours is sufficient to give it this property, or, as it may be expressed, to "veratrinize" it thoroughly. The alteration of the properties of a muscle by veratrine in such a way that it must continue an effort once begun has been long known. It is an example of perfectly continuous contraction. Normal muscular contraction being regarded, as I have said, as discontinuous, the relation between it and the continuous contraction of veratrinized muscle has not been sufficiently considered. When, therefore, we set to work to measure the maximum contractile effect of a "veratrine spasm," I was both surprised and gratified to discover that the tension of a veratrinized muscle when excited by a single instantaneous stimulus was as great as that of a similar but unveratrinized muscle when subjected to a succession of stimuli, i. e., when artificially tetanized. It can also lift as great a load and hold it up for several (10–20) seconds at as great a height. (Tracings shown.)

We then proceeded to investigate the electrical concomitant of the veratrine "tetanus," if I may so call it (photograph 11, Pl. IV), and found it to be identical with that of an artificial tetanus produced by a succession of stimuli of sufficient frequency. Its true character can be best judged of by comparing it with photograph 12 (Pl. V), which was obtained by introducing into the unchanged circuit a constant difference of potential in the way before explained.

The fact that the veratrine spasm has the mechanical and electrical character of a continuous contraction is of value, not from its bearing on the mode of action of a particular chemical substance, but from the evidence it affords that discontinuity is not essential to energic display of contractile force. In this respect it would be wholly irrelevant to object that the data derived from experiments on a poisoned

[1] The veratrine used was kindly prepared by my friend Professor Dunstan, F. R. S.

muscle can not be applied to a normal one. All that it is required to prove is that it is possible for a spasm which is not discontinuous to be as effectual for the doing of external work as a normal contraction. It can hardly be disputed that the contraction of a veratrinized muscle is continuous. It is, therefore, no longer possible to assert that discontinuity is essential to functional capacity.

That our results differ from those of other observers is to be attributed to the mode of using the alkaloid and to the homeopathic minuteness of the dose. We estimate the quantity of veratrine which actually enters the muscle not to exceed one ten-thousand of a milligram.

THE HEART.

We now turn from the skeletal muscles to the organ by the rhythmical contractions of which the circulation is maintained. The mechanical response of cardiac, like that of skeletal, muscle can be evoked either directly or indirectly, but the heart has this peculiarity, that each part of it has attributes which we are accustomed to regard as nervous rather than muscular. It has, above all, the property which belongs, as we have seen from our experiments with strychnine, to the motor cells of the spinal cord—that of discharging itself rhythmically when in a state of continuous excitation. It is characteristic of heart muscle that it exhibits alternating periods of rest and activity, and we have now the clearest evidence that it is not in virtue of its possessing an intrinsic nervous system that it has this property. In another important respect it resembles the motor apparatus of the cord, namely, that its relations to stimuli are governed by what has been called the "all or not at all" principle. It either does not respond or, if at all, responds completely. In these respects, therefore, the action of the heart is comparable neither with that of muscle acting independently nor even with that of the muscle nerve preparation, but rather with that of muscle acting under the direction of the motor neuron which governs it.

I began the investigation of the electrical phenomena of the heart's beat in 1881 with Mr. Page. We made out two new facts, namely, that the electrical change which is evoked by excitation of the surface is propagated at a rate dependent on temperature, not in one direction only, but in all, as Engelmann had already shown to be the case with regard to the wave of contraction; and secondly, that the monophasic variation is not, as had been supposed by previous observers, an instantaneous change, but lasts during the whole period of energetic systole. But neither Mr. Page nor I understood then the nature of the initial "spike," which is so striking a feature in the photographic record of the variation in the uninjured heart. For its explanation I am indebted to Mr. Burch, whose investigations on the use of the capillary electrometer for measuring the electromotive force of cur-

Photograph 10.

10. Freshly prepared Sartorius attached to pelvis and connected to spinal cord by its nerve. Leading-off electrodes on hilus and tibial end. Exciting electrodes applied close together to skin of flank of decapitated preparation.

Photograph 11.

11. Electrical response of [illegible] and [illegible] [illegible] stimulation. Leading-off [illegible] trodes near [illegible] end. [illegible] the curve is steeper than [illegible] the comparison curve [illegible]

rents of short duration have been of so much value to physiologists. The moment it was understood that the spike indicated a diphasic variation analogous to that of the muscle I felt that I had the key to the complete understanding of my own previous observations. I was, moreover, able to bring these into complete harmony with those of Professor Engelmann made about the same time with the rheotome and galvanometer.

Let me ask your attention to the photographic curves of the diphasic and monophasic variations which I have placed one above the other in synchronic relation to each other. It is to be noticed that the movement of the recording surface is very slow—about a centimeter a second only. To obtain the monophasic curve you have to place the distal electrode on a spot which has been devitalized by scorching, and which is consequently physiologically inactive, the proximal electrode on the living surface near the junction between auricle and ventricle. The instantaneous stimulation is applied to the auricle some couple of millimeters distant from the proximal leading-off electrode. The Reizwelle is propagated from the auricle to the base of the ventricle and then on to the devitalized spot, so that before it arrives at the contact it is extinguished.[1] Consequently, the change which is expressed by the electrometer curve takes place exclusively at the proximal contact surface. It differs only from the monophasic variation of skeletal muscle in the longer duration of the period which intervenes between culmination and decline, and consequently bears a greater resemblance to the effect of a short continuous excitation of muscle than to that of an instantaneous one. (Photographs 13, 14, Pl. V.)

Turning to the diphasic variation obtained when the surface underlying the distal contact is not devitalized, we see that during the whole intervening period just referred to the two contact surfaces are approximately equipotential. This of course does not mean that both are physiologically inactive, but simply that the influence of the one exactly balances that of the other. This meaning of the diphasic variation is (with the exception of the initial spike) that which was assigned to it in 1882. It results from the mutual interference of two monophasic variations, the dip of the curve at the end indicating that the effect of the distal contact overlasts that at the proximal.

The general result of these observations is that, just as from the mechanical point of view the systole of the ventricle has lately been shown to be entirely analogous to the response of a muscle to an instantaneous stimulus, provided that we substitute volume for length and lateral pressure for tension,[2] so as regards the electrical phenomena there is a complete analogy between the monophasic and diphasic

[1] This mode of observation corresponds to the first fundamental experiment in muscle.

[2] O. Frank, Zur Dynamik des Herzmuskels, Zeits. f. Biol., vol. 32, p. 370.

variation of the heart and of muscle, respectively, provided that we bear in mind that the one is a response to a short continuous stimulation, the other to an instantaneous one.

DIONÆA.

My last example of motion and its accompanying electrical phenomena I will take from the plant. As everyone knows, there are certain parts of some of the higher plants which respond to stimulation like the motor organs of animals. These instances have been regarded as indications of the close relationship which exists between plants and animals as regards their elementary physiology. The subject attracted the attention of Mr. Darwin in relation to certain insectivorous plants, and it was at his suggestion that the observations to which I am now about briefly to refer were made. The electrical changes can be most easily studied, and appear in the most striking way, in the leaf of Dionæa. The leading-off contacts are applied to the opposite surfaces of one lobe of the leaf. In the resting state the one surface is found to be positive to the other. At a certain moment a hair on one lobe some 10 or 12 millimeters away from the place under investigation is touched by a camel-hair pencil or excited by an induction current. The surface which was before positive becomes less so, and the curve described resembles, as you see, the monophasic heart curve.

It is not necessary on the present occasion to do more than refer to this typical experiment, by which it was shown for the first time that the migration of liquid, and consequent sudden closure of the lobes on excitation, is accompanied by an electrical change analogous to that in contracting muscle, and that in the leaf this is propagated at a rate varying with temperature. Although the experiment is one of extreme simplicity the method of investigation has not, so far as I know, been pursued by any plant physiologist. The criticisms which were bestowed on it by animal physiologists I was able to answer in my second communication to the Royal Society, and have now the satisfaction to find that the experimental data set forth in that paper are given in full in Biedermann's important treatise on Electro-Physiology.

I have now, though in a very incomplete way, described the phenomena bearing on my subject, so far as I have been able to observe them. May I be permitted to submit to you the indications which they seem to me to afford?

In striated muscle, the primary effect of every excitation, is a process of oxidation having its seat at the excited part. It may be surmised that this consists of two stages, namely, liberation of previously intramolecular oxygen and actual oxidation. In a single element of muscular structure the duration of this process, when induced by an instantaneous stimulus, must be exceedingly short, and corresponds

Photograph 12.

12. Comparison curve obtained by leading off from the compensator a current of electro-motive force equal to that of the "action current," leaving the unexcited muscle in circuit.

Photographs 13, 14.

13, 14. Ventricle of heart of *R. esculenta* arrested by Stannius's ligature. Exciting electrodes on auricle. Leading-off contacts at base and apex. In 13 apex surface devitalized by heat; in 14, both surfaces uninjured.

with that of the excitatory variation, but in the whole organ may last until the development of tension has reached its maximum.

We have further learned that the monophasic variation is a phenomenon of great regularity, and may be taken as the type from which all other forms of response to stimulation may be derived, either by repetition, prolongation, or interference.

Although no attempt has been made to settle the question whether the natural contraction of muscle is discontinuous, it has been shown that the electrical phenomena of reflex contraction afford no ground for supposing that it is so. The efficiency of the veratrine spasm seems, at least, to justify us in doubting whether discontinuity is an essential quality of muscular contraction.

Finally, reasons have been given for thinking that the phenomena known as the "muscle current" and the "demarcation current" are manifestations of processes which have their seat at the surface of contact between electrode and living muscle.

THE TRUTH ABOUT THE MAMMOTH.[1]

By Frederic A. Lucas.

[Editor's Note.—In the October number of McClure's Magazine was published a short story, "The killing of the mammoth," by H. Tukeman, which, to the amazement of the editors, was taken by many readers not as fiction, but as a contribution to natural history. Ever since the appearance of that number of the magazine the authorities of the Smithsonian Institution, in which the author had located the remains of the beast of his fancy, have been beset with visitors to see the stuffed mammoth, and our daily mail, as well as that of the Smithsonian Institution, has been filled with inquiries for more information and for requests to settle wagers as to whether it was a true story or not. The contribution in question was printed purely as fiction, with no idea of misleading the public, and was entitled a story in our table of contents. We doubt if any writer of realistic fiction ever had a more general and convincing proof of success. The very general interest that has been shown in the subject has convinced us that our readers would be glad to know the truth about the mammoth, and accordingly we have asked Mr. F. A. Lucas, of the National Museum, to prepare the following article. If the mammoth, as Mr. Lucas knows him, is less in size and belongs to an earlier date than the mammoth as Mr. Tukeman painted him, we believe our readers will find him no less interesting.]

About three centuries ago, in 1696, a Russian, one Ludloff by name, described some bones belonging to what the Tartars called a "mamantu;" later on, Blumenbach pressed the common name into scientific use as "mammut," and Cuvier gallicized this into "mammouth," whence by an easy transition we get our familiar "mammoth." We are so accustomed to use the word to describe anything of remarkable size that it would be only natural to suppose that the name "mammoth" was given to the extinct elephant because of its extraordinary bulk. Exactly the reverse of this is true, however, for the word came to have its present meaning because the original possessor of the name was a huge animal. The Siberian peasants called the creature "mamantu," or "ground-dweller," because they believed it to be a gigantic mole, passing its life beneath the ground and perishing when by any accident it saw the light. The reasoning that led to this belief was very simple and the logic very good; no one had ever seen a live mamantu, but there were plenty of its bones lying at or near the surface; consequently, if the animal did not live above the grouud, it must dwell below.

[1] Reprinted, with permission, from McClure's Magazine for February, 1900.

To-day nearly everyone knows that the mammoth was a sort of big, hairy elephant, now extinct, and nearly everyone has a general idea that it lived in the north. There is some uncertainty as to whether the mammoth was a mastodon, or the mastodon a mammoth, and there is a great deal of misconception as to the size and abundance of this big beast. It may be said in passing that the mastodon is only a second or third cousin of the mammoth, but that the existing elephant of Asia is a very near relative, certainly as near as a first cousin, possibly a very great grandson. Popularly, the mammoth is supposed to have been a colossus somewhere from 12 to 20 feet in height, beside whom modern elephants would seem insignificant; but as "trout lose much in dressing," so mammoths shrink in measuring, and while there were doubtless Jumbos among them in the way of individuals of exceptional magnitude, the majority were decidedly under Jumbo's size. The only mounted mammoth skeleton in this country, that in the Chicago Academy of Sciences, is one of the largest, the thigh bone measuring 5 feet 1 inch in length, or a foot more than that of Jumbo; and as Jumbo stood 11 feet high, the rule of three applied to this thigh bone would give the living animal a height of 13 feet 8 inches. The height of this specimen is given as 13 feet in its bones, with an estimate of 14 feet in its clothes; but as the skeleton is obviously mounted altogether too high, it is pretty safe to say that 13 feet is a good, fair allowance for the height of this animal when alive. As for the majority of mammoths, they would not average more than 9 or 10 feet high. Sir Samuel Baker tells us that he has seen plenty of wild African elephants that would exceed Jumbo by a foot or more, and while this must be accepted with caution, since, unfortunately, he neglected to put a tape line on them, yet Mr. Thomas Baines did measure a specimen 12 feet high. This, coupled with Sir Samuel's statement, indicates that there is not so much difference between the mammoth and the elephant as there might be. This applies to the mammoth par excellence, the species known scientifically as *Elephas primigenius*, whose remains are found in many parts of the Northern Hemisphere and occur abundantly in Siberia and Alaska. There were other elephants than the mammoth, and some that exceeded him in size;[1] but even the largest can not positively be asserted to have exceeded a height of 13 feet, and it is to be greatly doubted if any one of them could have tossed a 25-foot log over his shoulder. Tusks offer convenient terms of comparison, and those of an average fully grown mammoth are from 8 to 10 feet in length; those of the famous St. Petersburg specimen and those of the huge specimen in Chicago measuring, respectively, 9 feet 3 inches and 9 feet 8 inches. So far as the writer is aware, the largest tusks

[1] Notably *Elephas meridionalis* of southern Europe and *Elephas columbi* of the southern United States and Mexico. It is extremely probable that the Chicago skeleton belongs to this latter species, which ranged northwesterly almost to Alaska.

REPRODUCTION OF A PAINTING OF THE MAMMOTH BY C. R. KNIGHT, MADE EXPRESSLY FOR MCCLURE'S MAGAZINE, AND PRESENTED BY THE EDITOR TO THE SMITHSONIAN INSTITUTION.

actually measured are two from Alaska, one 12 feet 10 inches long, weight unknown, reported by Mr. Jay Beach, and another 11 feet long, weighing 200 pounds, noted by Mr. T. L. Brevig. Compared with these we have the big tusk that used to stand on Fulton street, New York, just an inch under 9 feet long and weighing 184 pounds, or the largest shown at Chicago in 1893, which was 7 feet 6 inches long and weighed 176 pounds.[1]

For our knowledge of the external appearance of the mammoth we are indebted to the more or less entire examples which have been found at various times in Siberia, but mainly to the noted specimen found in 1799 near the Lena, embedded in the ice, where it had been reposing, so geologists tell us, anywhere from ten thousand to fifty thousand years. How the creature gradually thawed out of its icy tomb, and the tusks were taken by the discoverer and sold for ivory; how the dogs fed upon the flesh in summer, while bears and wolves feasted upon it in winter; how the animal was within an ace of being utterly lost to science when, at the last moment, the mutilated remains were rescued by Mr. Adams, is an old story, often told and retold. Suffice it to say that, besides the bones, enough of the beast was preserved to tell us exactly what was the covering of this ancient elephant, and to show that it was a creature adapted to withstand the northern cold and fitted for living on the branches of the birch and hemlock.

The exact birthplace of the mammoth is as uncertain as that of many other great characters, but his earliest known resting place is in the Cromer forest beds of England, a country inhabited by him at a time when the German Ocean was dry land and Great Britain part of a peninsula. Here his remains are found to-day, while from the depths of the North Sea the hardy trawlers have dredged hundreds, aye, thousands, of mammoth teeth in company with soles and turbot. If, then, the mammoth originated in western Europe, and not in that great graveyard of fossil elephants, northern India, eastward he went, spreading over all Europe north of the Pyrenees and Alps, save only Scandinavia, whose glaciers offered no attractions, scattering his bones abundantly by the wayside to serve as marvels for future ages. Strange indeed have been some of the tales to which these and other elephantine remains have given rise when they came to light in the good old days when knowledge of anatomy was small and credulity was great. The least absurd theory concerning them was that they were the bones of the elephants which Hannibal brought from Africa. Occasionally they were brought forward as irrefutable evidences of

[1] Since this was written a pair of enormous tusks of the African elephant have been received in New York and deposited with Messrs. Tiffany & Co., to whom I am indebted for the following information as to their size and weight: Right tusk, 10 feet $\frac{3}{4}$ inch along outer curve, 23 inches in circumference, weight 224 pounds. Left tusk, 10 feet $3\frac{1}{2}$ inches along outer curve, $24\frac{1}{4}$ inches in circumference, weight 239 pounds.

the Deluge; but usually they figured as the bones of giants, the most famous of them being known as Teutobochus, king of the Cimbri, a lusty warrior said to have had a height of 19 feet. Somewhat smaller, but still of respectable height, 14 feet, was "Littell Johne," of Scotland, whereof Hector Boece wrote, concluding in a moralizing tone, "Be quilk (which) it appears how extravegant and squaire pepill grew in oure regioun afore they were effeminat with lust and intemperance of mouth." More than this, these bones have been venerated in Greece and Rome as the remains of pagan heroes, and later on worshiped as relics of Christian saints. Did not the church of Valencia possess an elephant tooth which did duty as that of St. Christopher, and, so late as 1789, was not a thigh bone, figuring as the arm bone of a saint, carried in procession through the streets in order to bring rain?

Out of Europe eastward into Asia the mammoth took his way, and having peopled that vast region, took advantage of a land connection then existing between Asia and North America and walked over into Alaska, in company with the forerunners of the bison and the ancestors of the mountain sheep and Alaskan brown bear. Still eastward and southward he went, until he came to the Atlantic coast, the latitude of southern New York roughly marking the southern boundary of the broad domain over which the mammoth roamed undisturbed.[1] Not that of necessity all this vast area was occupied at one time, but this was the range of the mammoth during Pleistocene time, for over all this region his bones and teeth are found in greater or less abundance and in varying conditions of preservation. In regions like parts of Siberia and Alaska, where the bones are entombed in a wet and cold, often icy soil, the bones and tusks are almost as perfectly preserved as though they had been deposited but a score of years ago, while remains so situated that they have been subjected to varying conditions of dryness and moisture are always in a fragmentary state. As previously noted, several more or less entire carcasses of the mammoth have been discovered in Siberia, only to be lost; and while no entire animal has so far been found in Alaska, some day one may yet come to light. That there is some possibility of this is shown by the discovery, recorded by Mr. Dall, of the partial skeleton of a mammoth in the bank of the Yukon with some of the fat still present,

PRIMITIVE PICTURE OF A MAMMOTH ENGRAVED ON A FRAGMENT OF MAMMOTH'S TUSK.

The picture is so well done that one must believe that the artist, a cave dweller in Southern France, had seen the animal, if he did not make the drawing from real life. The original engraving, which is about three times the size of the reproduction, is in the Muséum d'Histoire Naturelle, Paris.

PARTS OF THE SKELELON OF A MAMMOTH THAT WERE FOUND IN 1897 IN THE URAL MOUNTAINS.

Now preserved in the museum at Ekaterinburg, Russia. The photograph was taken in the yard of one of the peasants who made the find, and it is he who appears in the picture.

approach to finding a live mammoth in Alaska; and a small piece of fat, obtained by Mr. Dall, is the nearest the United States National Museum has come to securing a stuffed mammoth.

As to why the mammoth became extinct, we *know* absolutely nothing, although various theories, some much more ingenious than plausible, have been advanced to account for their extermination—they perished of starvation; they were overtaken by floods on their supposed migrations and drowned in detachments; they fell through the ice, equally in detachments, and were swept out to sea. But all we can safely say is that long ages ago the last one perished off the face of the earth. Strange it is, too, that these mighty beasts, whose bulk was ample to protect them against four-footed foes and whose woolly coat was proof against the cold, should have utterly vanished. They ranged from England eastward to New York, almost around the world; from the Alps to the Arctic Ocean; and in such numbers that to-day their tusks are articles of commerce, and fossil ivory has its price current as well as wheat. That many were swept out to sea by the flooded rivers of Siberia is certain, for some of the low islands off the coast are said to be formed of sand, ice, and bones of the mammoth, and thence, for hundreds of years, have come the tusks which are sold in the market beside those of the African and Indian elephants.

That man was contemporary with the mammoth in Southern Europe is fairly certain, for not only are the remains of the mammoth and man's flint weapons found together, but in a few instances some primeval Landseer graved on slate, ivory, or reindeer antler a sketchy outline of the beast, somewhat impressionistic perhaps, but still like the work of a true artist, preserving the salient features. We see the curved tusks, the snaky trunk, and the shaggy coat that we know belonged to the mammoth, and we may feel assured that if early man did not conquer the clumsy creature with fire and flint, he yet gazed upon him from the safe vantage point of some lofty tree or inaccessible rock, and then went home to tell his wife and neighbors how the animal escaped because his bow missed fire. That man and mammoth lived together in North America is uncertain; so far there is no evidence to show that they did, although the absence of such evidence is no proof that they did not. That any live mammoth has for centuries been seen on the Alaskan tundras is utterly improbable, and on Mr. C. H. Townsend seems to rest the responsibility of having, though quite unintentionally, introduced the Alaskan live mammoth into the columns of the daily press. It befell in this wise: Among the varied duties of our revenue marine is that of patrolling and exploring the shores of arctic Alaska and the waters of the adjoining sea, and it is not so many years ago that the cutter *Corwin*, if memory serves aright, held the record of farthest north on the Pacific side. On one of these

northern trips to the Kotzebue Sound region, famous for the abundance of its deposits of mammoth bones,[1] the *Corwin* carried Mr. Townsend, then naturalist to the United States Fish Commission. At Cape Prince of Wales some natives came on board bringing a few bones and tusks of the mammoth, and upon being questioned as to whether or not any of the animals to which they pertained were living, promptly replied that all were dead, inquiring in turn if the white men had ever seen any, and if they knew how these animals, so vastly larger than a reindeer, looked.

Fortunately, or unfortunately, there was on board a text-book of geology containing the well-known cut of the St. Petersburg mammoth, and this was brought forth, greatly to the edification of the natives, who were delighted at recognizing the curved tusks and the bones they knew so well. Next the natives wished to know what the outside of the creature looked like, and as Mr. Townsend had been at Ward's establishment in Rochester when the first copy of the Stuttgart restoration was made, he rose to the emergency and made a sketch. This was taken ashore, together with a copy of the cut of the skeleton that was laboriously made by an Innuit sprawled out at full length on the deck. Now, the Innuits, as Mr. Townsend tells us, are great gadabouts, making long sledge journeys in winter and equally long trips by boat in summer, while each season they hold a regular fair on Kotzebue Sound, where a thousand or two natives gather to barter and gossip. On these journeys and at these gatherings the sketches were no doubt passed about, copied, and recopied, until a large number of Innuits had become well acquainted with the appearance of the mammoth, a knowledge that naturally they were well pleased to display to any white visitors. Also, like the Celt, the Alaskan native delights to give a "soft answer," and is always ready to furnish the kind of information desired. Thus in due time the newspaper man learned that the Alaskans could make pictures of the mammoth, and that they had some knowledge of its size and habits; so with inference and logic quite as good as that of the Tungusian peasant, the reporter came to the conclusion that somewhere in the frozen wilderness the last survivor of the mammoths must still be at large. And so, starting on the Pacific coast, the live mammoth story wandered from paper to paper, until it had spread throughout the length and breadth of the United States, when it was captured by Mr. Tukeman, who, with much artistic color and some realistic touches, transferred it to McClure's Magazine, and, unfortunately for the officials thereof, to the Smithsonian Institution.

And now, once for all, it may be said that *there is no mounted mammoth* to awe the visitor to the national collections; and yet there seems

[1] Elephant Point, at the mouth of the Buckland River, is so named from the numbers of mammoth bones which have accumulated there.

THE WARD RESTORATION OF THE MAMMOTH IN THE MUSEUM OF NATURAL HISTORY, UNIVERSITY OF VIRGINIA, CHARLOTTESVILLE, VA.

From a photograph by L. W. Humphreys.

THE MAMMOTH IN THE ROYAL MUSEUM OF NATURAL HISTORY AT ST. PETERSBURG.

This is the specimen found entire on the banks of the Lena River, Siberia. Although it is believed to have lain embedded in the ice from 10,000 to 50,000 years, some of the skin still adheres to the skull.

no good and conclusive reason why there should not be. True, there are no live mammoths to be had at any price; neither are their carcasses to be had on demand; still there is good reason to believe that a much smaller sum than that said to have been paid by Mr. Conradi for the mammoth which is *not* in the Smithsonian Institution, would place one there. It probably could not be done in one year; it might not be possible in five years; but should any man of means wish to secure enduring fame by showing the world the mammoth as it stood in life a hundred centuries ago, before the dawn of even tradition, he could probably accomplish the result by the expenditure of a far less sum than it would cost to participate in an international yacht race. Who will be the first to dispatch an expedition to seek a frozen mammoth?

MAMMOTH IVORY.[1]

By R. Lydekker

In spite of all their ingenuity and skill, there are two animal products of high commercial value which our manufacturers have not hitherto imitated with such success as to make the substitutes equivalent in utility and beauty to the originals. These products are elephant ivory and whalebone, and although the imitations in the former case make a much nearer approach to the true article than has been found practicable in the latter, they still leave much to be desired. Consequently the demand for natural ivory is not only likely to be maintained on the same level as heretofore, but would undoubtedly increase if an adequate supply were forthcoming.

True ivory, to which the name should properly be restricted, is the constituent of the tusks of elephants of different species, and is found in no other animals. In making this statement it must not be assumed that its presence in mastodons is denied, since those extinct animals are nothing more than elephants in a wider sense of the term. From other so-called ivory, such as that of hippopotamus tusks, sperm-whale teeth, and narwhal "horns," elephant ivory is readily distinguished at a glance by the "engine-turned" pattern—similar to that on the back of a watch case—which it displays in cross section, as may be seen by looking at the butt end of the handle of a table knife. And it is probably due to this peculiarity of internal structure that elephant ivory displays the elasticity which forms one of its most valuable properties.

As all the readers of this journal are doubtless aware, there are only two living species of elephants at the present day, namely, the Indian, or, as it might with more propriety be called, Asiatic, and the African. As regards the production of ivory, the latter is, or perhaps was, much the more valuable animal of the two. In the first place, till within the last few years, it existed in almost incredible numbers in many parts of its habitat; and in the second place it produced more ivory, animal for animal, this being due to the circumstance that

[1] Reprinted from Knowledge, in Scientific American Supplement, No. 1228, July 15, 1899.

whereas in the African species both sexes are furnished with tusks of large size, in its Asiatic cousin they are generally restricted to the male sex, and even then in certain cases may be but very poorly developed.

Again, it appears that in modern times, at all events, much of the ivory yielded by the Asiatic elephant is worked up in the land of its birth, comparatively little reaching Europe in the raw state. Consequently, for recent ivory, the European market is very largely dependent upon the product of the African species, for which the great commercial emporia are London and Antwerp. Now, although a few years ago elephant hunting was a profitable trade in the remoter districts of southeast Africa, the herds have been so reduced in number that comparatively little ivory is obtained at the present day. Moreover, the great stocks of ivory formerly possessed by the native chiefs have been largely reduced or exhausted over the greater portion of the country. It is true, indeed, that in the Congo district elephants are still locally abundant, while the opening up of the Egyptian Soudan may very probably introduce to the market a supply of tusks from Kordofan, Dafur, and the Bahr-el-Gazal districts. But if these regions prove productive in ivory it is only too likely, unless proper precautions are taken, that they will comparatively soon be shot out. And if the production be not placed under restriction, it is evident that the annual supply will be relatively small.

It is clear, therefore, that African ivory is likely to become gradually scarcer and scarcer, and if there were no other source of supply this beautiful substance would apparently soon reach a prohibitive price.

As a matter of fact there exists, however, in the frozen tundras of Siberia a supply of ivory which will probably suffice for the world's consumption for many years to come.

This ivory is the product of the mammoth (*Elephas primigenius*), a species nearly allied to the Indian elephant, but protected from the cold of the Arctic regions by a coat of long, coarse hair, with a finer woolly underfur at the base. The tusks, too, of the mammoth were larger and more curved than those of its living Asiatic relative, being sometimes twisted into a spiral almost recalling that formed by the horns of the African kudu. From the abundance of these tusks it is further probable that they were developed in both sexes.

In addition to dwelling on the Arctic tundras of the Lena, Yenisei, and Obi valleys, as well as extending to the New Siberian Islands (which in past times evidently formed a portion of the Asiatic mainland), and Alaska, the mammoth roamed over a large portion of Europe in Pleistocene times. And in the gravels and brick earths of our English river valleys its tusks, teeth, and bones are of comparatively common occurrence, while quantities of similar remains are dredged

from the Dogger bank by the North Sea trawlers. If, however, the ivory turner expected to find a workable commodity in British mammoth tusks, he would be grievously disappointed. All those found in the gravels and brick earths, as well as the specimens hauled up from the Dogger bank, have lost the greater part of their animal matter, in consequence of which they crumble more or less completely to pieces when exposed to the influence of the atmosphere, and for the purpose of preservation and exhibition have to be copiously treated with size or gelatin.

Not so the mammoth ivory of the Siberian tundras, which, in the best preserved specimens, retains the whole of the original animal matter, and, except when stained by earthy infiltrations, is as suitable for the purposes of the turner as the best product of the African elephant. This remarkable state of preservation has been produced by entombment in the frozen soil of the tundras. In many instances, as is well known, entire carcasses of the mammoth have been found thus buried, with the hair, skin, and flesh as fresh as in frozen New Zealand sheep in the hold of a steamer. And sleigh dogs, as well as Yakuts themselves, have often made a hearty meal on mammoth flesh thousands of years old. In instances like these it is evident that the mammoths must have been buried and frozen almost immediately after death; but as the majority of the tusks appear to be met with in an isolated condition, often heaped one atop of another, it would seem that the carcasses were often broken up by being carried down the rivers before their final entombment. Even then, however, the burial, or at least the freezing, must have taken place comparatively quickly as exposure in their ordinary condition would speedily deteriorate the quality of the ivory.

The retention of their animal matter and their unaltered condition have led some writers to object to the application of the term fossil to the Siberian mammoth tusks and to restrict its use to the altered and partially petrified specimens met in the superficial deposits of warmer countries. This, however, is quite illogical, seeing that a fossil must be defined as including the remains or traces of any animal or vegetable buried in the earth by natural causes. And we may, therefore, with perfect propriety speak of the Siberian mammoth tusks as fossil in contradistinction to petrified ivory.

How the mammoths were enabled to exist in a region where their remains became so speedily frozen, and how such vast quantities of these became accumulated in certain spots, are questions which do not at present seem capable of being satisfactorily answered; and their discussion would accordingly be useless, not to say out of place, on the present occasion. It will suffice to say that such accumulations do exist and that the soil of certain portions of the tundras seems to be almost crammed with such remains.

It may, however, be remarked that the contents of the stomachs of the frozen mammoths, as also those of the two species of rhinoceroses which were their fellow-inhabitants of the tundras, contain remains of pine needles and other vegetable substances. And from this it may be inferred that the tundras themselves were clothed with forest during the mammoth epoch, since the theory that the carcasses were carried down by the rivers flowing from warmer southern regions into the Arctic Ocean can scarcely merit serious attention. Possibly some light may be thrown upon the subject by the great accumulations of bones of large recent mammals which have been met with in certain districts of East Africa.

Although outside scientific and commercial circles comparatively little is known with regard to the subject in England, mammoth ivory, in place of being a modern discovery, was known to the ancients, and has for centuries been an article of trade and manufacture. It is, however, only recently that the history of the subject has been worked out; and for this we are largely indebted to the labors of Sir H. H. Howorth[1] and Dr. Trouessart,[2] of Paris. Baron Nordenskiöld has likewise contributed important information on the subject in the Voyage of the Vega. And it is from these sources that the following paragraphs are mainly compiled.

If we may take the "buried ivory" mentioned by Pliny on the authority of Theophrastus, a disciple of Aristotle, to be the same as mammoth ivory, we may regard this substance as known to the Western world in the time of Alexander. But apart from this, mammoth ivory was evidently familiar at a very remote time to the Chinese, who spoke of the animal by which it was yielded as "thien-shu" (the mouse that hides). This mythical creature, which was compared in size to an elephant, was reported to lead a subterranean existence like a mole, with bones as white as ivory, and the flesh cold, but pure and wholesome, this reference to the coldness of the flesh apparently pointing to their acquaintance with frozen mammoth carcasses.

In Europe, Eginhard, the historian of Charlemagne, states that among the presents sent to the Emperor of the West by the Kalif Haroun-al-Raschid in the year 807 were the horn of a "licorne" and the claw of a griffon. These rarities were long preserved in the royal treasury at St. Denis; and, from a description given in a work dated 1646, it appears that while the former was a mammoth tusk, the latter was the horn of the woolly Siberian rhinoceros.

During the ninth or tenth century[3] Arab traders appear to have established a trade route from northern Russia or Siberia to Persia or Syria; and their records refer to the occurrence of buried ivory near the city of Bolghari, on the Volga, which was probably situated on or

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[1] The Mammoth and the Flood, Chap. III (1887).

[2] Le Mammoth et l'Ivroire de Siberie, Bull. Soc. Acclim. Paris, 1898.

[3] Dr. Trouessart gives the former and Sir H. Howorth the latter.

near the site of the modern Nijni-Novgorod. The first Siberian mammoth tusk imported into Western Europe in modern times was brought to London in the year 1611 by one Josias Logan, by whom it had been purchased from the Samoyedes of the Pechora district. Concerning this specimen, Baron Nordenskiöld writes that as Englishmen at that time visited Moscow frequently, and for long periods, a remark occurring in Purchas's history appears to indicate that fossil ivory first became known in the capital of Russia some time after the conquest of Siberia.

Be this as it may, it is in evidence from the account of Avril, who traveled in Russia during 1685, that fossil ivory was at that time imported into China and other Asiatic countries, where it was highly esteemed; and it is stated that it was largely employed by Turks and Persians for ornamenting sword and dagger hilts, being preferred to Indian ivory on account of its whiter color and finer grain. And here it may be incidentally mentioned that, according to the same author, the Russian term "mammout" is a corruption of the Hebrew behemot, or behemoth, which the Arabs make mehemot. Canon Tristram is, however, of opinion that in the Bible behemoth often refers to the hippopotamus; and if this be correct, a transference of the name would appear to have been made by the Arabs, this being the less improbable since it is stated in Hebrew to be applicable to any large beast.

Apart from this there is a record that about 1722 Peter the Great ordered the collection of tusks and other remains of the mammoth for the museum at St. Petersburg. And between 1750 and 1770 a Russian trader named Liakhoff established an extensive importation of mammoth ivory from the districts lying between the Khotanga and Anadyr rivers, and likewise from one of the southernmost islands of the New Siberian group, which still bears his own name. Surveys subsequently made by the government in those islands indicated that the soil is teeming with the bones, tusks, and teeth of mammoths, while the adjacent mud banks exposed at low tide are equally prolific. Some idea of their abundance may be gathered from the account given by Dr. Bunge, who visited Liakhoff Island from 1882 to 1884, and in the course of three short summers collected no less than 2,500 selected specimens. In the New Siberian Islands the thermometer now often falls to 50° C. below freezing point, so that collecting is an impossibility during the winter.

With regard to the amount of mammoth ivory that comes into the market accounts are by no means so numerous nor so accurate as might be desired. It is stated, however, that in 1821 a Yakut brought back 500 puds (40 pounds to the pud) from the New Siberian Islands, and between the years 1825 and 1831 the amount annually sold in Yakutsk ranged between 1,500 and 2,000 puds in addition to that disposed of at other towns. Many writers speak of seeing boat loads of tusks on the Lena and Yenisei, a steamer which carried Baron

Nordenskiöld in 1875 having a cargo of over 100. About the year 1840 Dr. Middendorff, who visited the country, estimated that the annual output of Siberian ivory reached 110,000 pounds, representing at least a hundred individual mammoths, so that the total number of animals whose remains have been exported since the conquest of Siberia must be between 20,000 and 30,000. And since Middendorff's estimate probably errs on the side of being too low, the numbers may have been considerably in excess of this.

In the London market, according to Mr. Westendarp, 1,635 mammoth tusks were sold during the year 1872 and 1,140 in the following year, the weight of these varying from 140 to 160 pounds each. Only a small percentage of these were, however, fit for the turner of ivory of high quality, about 14 per cent being of the best description, 17 per cent of inferior quality but still useful, while 54 per cent were bad, and the remaining 15 per cent rotten and worthless.

According to Dr. Trouessart, the price of mammoth ivory in the market at Yakutsk is 25 francs per pud for the highest quality, 17½ francs for the second, and from 5 to 7 francs for the third quality. A small quantity is worked up locally into ornamental and fancy articles of various kinds; but this industry seems to be a waning one, and more and more of the raw material goes direct to the foreign market. Yakutsk, which is situated on the Lena about midway between its mouth and the frontier of China and has about 5,000 inhabitants, has long been the acknowledged center of the trade, but it is considered probable that at present the great bulk of the ivory goes to China and that only a comparatively small portion finds its way into the more distant markets of Europe. The opening up of the country by the Siberian railway may, however, lead to a revolution in this respect and also inaugurate a new era of prosperity for Yakutsk and the other Siberian towns.

With regard to the future development of the trade and the persistence of the supply it may be remarked that only a small portion of Siberia has hitherto been explored at all, and that other deposits remain to be discovered. Of those already worked, Dr. Trouessart writes as follows:

"It is difficult to believe that the enormous quantity of tusks indicated by the masses of bones spoken of by travelers who have visited the archipelagos of northern Siberia can have been accumulated in the course of only a few centuries. It is most probable that only the surface of these vast bone deposits has hitherto been exploited, and that by excavating the soil to a greater depth, and, if necessary, employing the aid of dynamite to break up the frozen strata, good results will be obtained.

"If this idea be well founded, and if, as is unfortunately only too probable, the supply of African ivory comes practically to an end at no very distant date, there is every hope of finding a precious reserve in the fossil ivory of Siberia."

ON THE SENSE OF SMELL IN BIRDS.[1]

By M. Xavièr Raspail.

Four of the five senses are usually considered to be more or less obtuse in birds. Their sight alone is acknowledged to be more perfect and complicated than that of mammals. In regard to smell, some authors maintain that it is very slightly developed in animals of this class, those which feed on carrion being guided to their prey exclusively by sight. Others go so far as to say that this sense scarcely exists among birds. It is, however, generally admitted that nocturnal birds of prey have a pretty fine sense of smell.

The truth is that it is extremely difficult to observe birds in such a way as to make out precisely what their olfactory capacity is, since their sight and hearing are sufficient of themselves to put them on their guard against anything that may happen anywhere near them; and when they seek their food it is not easy to ascertain that their sight has not informed them of its whereabouts.

But if these circumstances have made it possible to overlook the sense of smell in birds I can not see what can have given rise to the notion that their hearing is imperfect. It is, at any rate, easy enough to convince oneself of the contrary, and so to rectify one at least of the numerous errors of accepted science.

Many a time have I seen a bird fly suddenly away for nothing but the noise of cocking a gun more than 4 rods (20 meters) off, the sportsman being quite invisible behind the peephole in the face of the blind. All gamekeepers know how prudently one has to approach trees where crows are perching, even at night, and this is still more true of ringdoves (ramiers). The smallest twig cracking under foot is enough to send the whole flock away long before the sportsman would have succeeded in finding them had they stayed. And the proof of the fineness of their hearing is that they know whether the cracking of the dead wood has been caused by man, whom they particularly dread, or by a prowling beast. I have observed that the passage of a herd of deer under the trees where crows are passing the night does not disturb them.

[1] Translated from a communication to the Zoological Society of France.

Moreover, they hear their note of recall at great distances, and everybody who has observed them at liberty must agree that in their notes, which appear to us so uniform, there are nevertheless differences which escape our ears, and which for them constitute a sort of language to apprise each other of danger.

My observations show that the sense of smell is also highly developed among birds, and that it not only puts them upon their guard against danger, but also directs them in the choice of food which they would be unable to recognize by sight.

The majority of wild mammals take care to snuff the air as they go about, so as to collect any emanations which might reveal the presence of an enemy. Their nostrils are as wide open as their ears. It is easy to show this by experiment whenever one happens to be in the neighborhood of a wood inhabited by rabbits and hares. The most favorable season is in the early part of September, an hour before sunset. This is the time when these animals come out to go to pasture, the hares farther from the wood, the rabbits only on its skirts. If at such a time you station yourself as silently as possible in the middle of the length of the border of the wood, well concealed in the thorny ditch that commonly limits it, you will soon be able to judge of the effect your presence has. On the windward side the rabbits will come out without suspecting anything, often to within a few yards of you, while on the leeward side you will not see a single one, and so it will be until you change your place. Every poacher is perfectly aware that, above all, he must beware of the flair of the game, and when he recognizes the passage of a hare or roe he chooses his place so as to have the wind, or, in other words, so that the animal shall come out in the quarter whence the wind blows.

Now this equally applies to pheasants and partridges; that is to say, to birds who spend the day on the ground and only occasionally resort to flight. They are rendered equally distrustful by scent. One can readily convince oneself of this by waiting for them at the hours of the day when the pheasants come out of the wood to feed in the open, and in the evening when the partridges quit the covers, where they have taken refuge from pursuit. Neither the one species nor the other will show themselves on the side where the wind will enable them to scent your presence.

Ringdoves have furnished me with a not less characteristic example. The observation dates from February, 1888, during which month the ground remained covered with a thick layer of snow. The consequence was that these birds, being famished, approached the houses to try to get Brussels sprouts, which are almost their only food in hard winters. A flock of some thirty of them remained about my vegetable garden, where they lighted several times a day on a bed of these Brussels sprout plants. Being tempted to take a few shots at them, I set up a

portable hut within range and installed myself there, calculating upon the prompt return of the doves, which my arrival had driven into the neighboring wood. Accordingly, after a short time I saw them light on an oak close by and then successively descend to the fruit trees of the garden, and one of them even alighted directly on the head of a Brussels sprout cabbage. But instead of pecking at the leaves, as I had always seen them do, while watching them from a window of the house, he kept still with his head up as if disquieted at something unusual. Then all of a sudden he flew away at the very moment when several of his companions were coming to join him. This gave the whole flock notice to depart, and away they all flew, going back to the oak, and directly after leaving altogether. I was much surprised, because the day before I had several times seen these birds when they had been dispersed by the coming of the gardener return as soon as he was gone, so that their present fright must have had some other reason. No bird of prey could have been about, because if there had been any the barnyard cocks would have given the signal. But I noticed that the wind was from the northeast, and I had placed my hut in the best place to hide it, but precisely so that the wind blew from it to the bed of Brussels sprouts. So I concluded that the first dove had scented me and had let the others know it, and that they had taken warning.

That evening I carried mv hut over to the west side, where it was much more prominently in sight, but so as to be to the leeward of the bed of Brussels cabbages. The next day, at dawn, in going to my hut, I scared away three doves which had already lit there to feed. Snow had fallen during the night, and the thermometer stood at 14° F. I had not been in my place more than half an hour when the three doves came back and exposed themselves to my fire without the smallest distrust.

No more came till 3 o'clock. Losing patience, I was just leaving the hut, when I saw that a large flock had passed over the garden, and having described a curve, was lighting on the oaks of a wood some hundreds of yards away. I went back in haste, and putting my eye to the loophole, set myself to watching the birds, which I could indistinctly see among the frosty branches. I guessed that they had spied the terminals of the Brussels sprout plants sticking out of the snow, and that that was the reason of their halt; and I was right, for the doves soon came to the nearest trees, and very shortly the whole flock, which was larger than that of the day before, one after another, took their places on the sprouts. The very first comers set to feeding quite calmly, without the smallest symptom of hesitation, and I could soon choose the most compact group to aim at from among fifty or more.

I fear I may have dwelt at too great length upon details more interesting to a hunter than to a scientific man; but they seem to me well

adapted to proving the sense of smell, acting under the same circumstances as among pheasants and partridges.

I come now to observations proving that smell as well as sight serves to direct birds to their food, and I shall limit myself to those in which this inference is least liable to error.

Pheasants go for breeding to the woods of the Plaine-Basse, which continue the forest of the Lys[1] toward Gouvieux. These woods are separated from my place by worked fields about a hundred yards wide. I had never known the setting birds to cross the little plain and come to my place when they left the nest for food, until some years ago, when, having made a little basin where birds could drink in summer, the gardener told me, a few days after, that the pheasants were coming every day to drink at this basin in the interior of the park, nearly 90 yards from the quickset hedge shutting us off from the farm land between us and the woods.

Had they found the water by chance? Possibly; yet it was difficult to imagine that the setting birds, who usually quit the nest only just long enough to take their food, had come so far for nothing. The following year I wished to make sure about the matter and used the following simple plan: When the setting season came, I let the basin drain, and had all the graveled walks around it carefully raked, so that the claws of the birds should leave easily visible traces. For a fortnight no pheasant came.

It was the second week of May. The weather was dry and fine, and the wind was from the north. I had the basin filled with water, and the next day but one I found that a pheasant had come in a direct line to drink, and had returned by the same route. There could be no doubt about its sex, which was shown by the droppings close to the basin in the form that setting birds, especially among the gallinaceæ, produce so copiously on rising from the nest.

It is incontestible that this pheasant, like all the pheasants, male and female, which I have seen, discover water put in no matter how hidden a place; had perceived its emanations at a distance of at least 200 yards, even supposing her nest were on the very border of the wood.

In order to show that, in the observation which I am now to record, sight could not have played any part, I will begin with describing the place where it was made.

In the midst of a great lawn surrounded by wooded parts which make a thick screen on the side of the tilled fields, there is a thicket of lilacs mixed in with Austrian black pines and with pitch pines (*épicéas*). In the center of this thicket there is a little open place, and it was there that, in the hard winter of 1890–91, I chose a spot, well sheltered by the evergreens from snow squalls, to scatter wheat and other grain, which was at once appreciated by the feathered tribe, who were able

[1] A river of Flanders, rising in Picardy.—TRANSLATOR.

there to fill their crops decently in a season of want and famine. Every afternoon I used to go to renew the provision, when one day I was surprised by the noise of five partridges whom my coming had scared away from the grain. The question which at once puzzled me was how they had ever discovered it.

It was certainly the first time they had come; for I should not have failed to remark their footprints, as I saw them at that moment all about the place where grain had been scattered. Following their traces, plainly marked in the snow, all along their path back to the hedge of the inclosure, I found they had come straight from the tilled fields. They had, therefore, been attracted to this very point, and nothing but its smell could have revealed the presence of this food, which they could not have expected to find at a time when they were reduced to seeking under the crusted snow for leaves of wheat or rye.

Every other hypothesis was excluded; for if chance had brought them to the place, they would not have followed so straight a path. Sight could not have guided them, since in order to see that there was grain they must have flown directly over the little opening, which was closely sheltered by trees; and to fly that way would be to fly straight toward a cluster of houses. Besides, if they had seen the grain in that way, instead of flying back to the field to return to it on foot, they would simply have lit close by. They had, then, scented the grain while they were in the tilled field seeking for possible food.

Tomtits (*Parus major*) are particularly fond of Swiss cheese. Now, in their wild condition they can so seldom find it that most of them can have never tasted it, and consequently it can only be its smell that attracts them to it. I first found this out in a way as unfortunate as it was unexpected.

For a long time I had used Swiss cheese to bait traps intended to destroy marauding cats, as well as those other nocturnal malefactors, the hedgehogs. The latter destroy the ground nests even of the pheasants and partridges—nests not so likely to be found by cats because of their dislike for walking in dewy grass or other crops. Now, I had often found this bait gnawed by some animal—as I supposed, by a field mouse—when, one morning, the trap having snapped, I found under its striking part a crushed titmouse, a victim of imprudent assaults upon the bait of a feather-triggered trap that brings down a load of about 90 pounds. After that, to avoid destroying so valuable an insect eater, I used, especially at the setting season, as it happened then to be, to uncock the trap at dawn and take away the stick with the Swiss cheese.

Last year,[1] contrary to custom, no tomtit nested in my place. From the beginning of spring I had not seen one of these birds. Conse-

[1] See "La diminutior des Oiseaux en 1897," in La Feuille des jeunes naturalistes, for December, 1897.

quently, my trap, always functioning through the summer, when the safety of the settings makes it the most needed, I had relaxed my precautions a little, so that the trap often remained set until quite late in the morning. But one day I found the trap had gone off, and that without catching a cat or a hedgehog, since there was no room for an animal; and, on lifting it, I was disagreeably surprised to find a female tomtit whose nest was certainly not in that neighborhood. The delicacy of her scent had caused her death.

I am now going to give instances which will remove all doubt, if any remains, of the great power of scent among birds. These observations relate to several kinds of birds which destroy slugs by searching for them in the soil and making way with them, after boring into the earth with their beaks till they come to the villainous larva.

Among these species I may mention the rook (*corbean freux*), the magpie, and the blackbird (*merle noir*), the only ones which I have actually seen at this work. There are, very likely, others.[1] The services that the two first thus render to agriculture are generally acknowledged. In the case of the magpie they attentuate his misdeeds.

In September, 1898, I used at all hours of the day to surprise several magpies who always flew away from the same point on the lawn. My attention was soon attracted to quantities of holes that they had evidently made in the turf. Some strokes of the spade revealed young larvæ of May bugs (*hannetons*) several months old, 1898 being the year of the Uranian cycle in the Department of the Oise. I counted up to ten under one turf, and to-day there is an acre or so of the lawn where the grass, eaten at the roots, has rotted and made a horrid spot visible at a great distance.

The magpies had been at work there, as the rooks work in the meadows. Neither of them dig the holes they do with their strong beaks on the mere chance of finding something; they go straight for the larva they want; so that we have to admit that they scent it, notwithstanding the layer of earth that covers it.

I have said that the blackbird does the same thing, a fact which I believe was entirely unknown before 1897. But there always remains an abundance of new facts for the observer of nature to discover.

In the year named, during the month of June, I remarked in a walk bordered on both sides with lilacs, numbers of little heaps of earth that had been taken from holes, at the bottoms of several of which could be seen a print agreeing perfectly in its dimensions with the bodies of larvæ of May bugs. At this time of the summer it is not unusual to find these larvæ reascending nearly to the surface of the ground before redescending in July to the depth where they are to remain until their nympheal metamorphosis is accomplished.

[1] I have strong reason to believe that the green woodpecker (*? gécine vert*) hunts for slugs in the same way; but I have, as yet, no positive proof of it.

At first sight I thought a magpie, having discovered this mine of slugs, had exploited it to his own profit as well as to the great advantage of the vegetation. But the footprints that a little more attention brought to my notice on the little heaps of freshly stirred earth could only be due to some much smaller bird, such as a blackbird.

The fact was of sufficient interest to engage my further attention, and the next day I posted myself so as to get a sight of the avenue, and it was not long before I heard a blackbird and recognized the presence in the bushes of young ones just out of the nest. Soon after I saw the mother come out from under the lilacs and hop into the avenue here and there and then suddenly stop and fall to picking at the ground with her beak while she shoveled away the loosened earth with her claws. She very soon took out of the hole she had dug a slug with which she hastened to go back under the lilacs to give it to her young. The depth of the hole from the surface to the bottom of the box which the larvæ occupied was about two inches (5 centimeters).

Unless we are to attribute to this bird's eyesight a sensibility to Roentgen rays, we must admit that it discovered the subterranean presence of the larvæ by smell.

I shall close these observations by recalling how the turtledove abandons its eggs at every stage of incubation as soon as the hand of man has touched them, although having been absent at the time it could not discover the fact otherwise than by scent, which enables it to perceive the infinitesimal odoriferous smear left by the finger on the eggshell.

Birds are, then, endowed with a sense of smell to a degree at least equal to that of the dog, to cite but one universally known example, and it is a great error of scientific literature to represent these animals, though provided with an exceedingly complete olfactory apparatus, as unable to discover their food otherwise than by sight.

Fortunately observations on man and the other mammals for anatomic and physiologic purposes have at least taught us that there is a fixed point of departure, that definite questions may be addressed to the object under observation. In the first place, it is known that certain portions of the brain are missing in certain animals and appear in certain others, usually of a higher order, and that their presence implies enlarged capacity in certain directions. In fact, science is to-day in a position to prove that given ganglia and sets of fibers are fitted to serve as the physical basis of certain psychic activities. For instance, in all animals a set of fibers, the optical nerve, issuing from the complicated apparatus of the eye, which receives impressions from the outer world, enters the brain. Uniformly it ends in a part of the brain alike in all. Experience teaches that whether the eye, or the optical nerve, or the portion of the brain in which it ends is destroyed, the power of sight goes with it. To this primary visual apparatus, as it may be called, a second is joined in man and the other mammals. Heavy sets of fibers run from a great lobe of the cerebrum to the terminal spots of the nerves of vision. The ends of this strand of fibers become closely interwoven with the ends of the nerves that receive the impression of light. A second visual mechanism thus connects itself with the first. Its significance has been discovered. If, by way of experiment, the continuity of the second set of fibers is broken, the animals so treated lose, not the ability to see, but in a measure the ability to recognize what they see. A dog in this condition, even if he suffers hunger, fails to snatch meat held before his eyes though he obviously sees it, and he does snatch it if he can recognize it with his nose, or if one of his eyes has been left intact.

Not only in the case of vision, but in the case of a number of other functions, the science of the last few decades has developed the fact that at least two central apparatus exist—a primary apparatus indispensable for a given function, and a secondary apparatus joined to the former. Concerning the secondary apparatus we know its importance in the execution of acquired movements and in the recognition of objects perceived on a previous occasion. Anatomically it originates in the cortical substance of the brain. In man and the higher animals various parts of the cortex are connected with one another in such manifold ways that it was natural for the assumption to gain currency that the cortex of the cerebrum is the anatomical foundation from which proceed the most varied perceptions, associations, and coordinations, that it is the seat of the higher psychic functions, above all, of the faculty of memory. The theory was greatly strengthened when experiment demonstrated that each and every part of the cortex does not appear in animals of all orders. The amphibious animals and the reptiles possess only cortical tracts, connected with the primary terminal places of the nerve system of smell. Only when birds are reached do we

happen upon the mighty nerve path leading from the primary termini of the nerve of vision to the cortex. The latter fact at once explains why the sight of birds is psychically far better developed than that of other animals. The bird of prey hovers hundreds of meters above the surface of the earth, yet when he darts down he is never mistaken—he has recognized a little mouse as one of his tidbits known from of old. Birds can not be lured with bait. Of all animals birds alone can be frightened off permanently by means appealing to the eye; only for them scarecrows, made to appear as like human beings as possible, are set up in the fields.

At best we have not advanced beyond the initial steps in this branch of science, but the road open to travel is becoming visible. Of one thing, however, we are wholly ignorant—of the rôle played in psychic processes by the ganglia, the centers in which the nerves of sense end, the primary termini, in other words. Do they, too, retain impressions? Do paths issue also from them rendering previous sensations available for later actions? Is the function of memory confined to the cortex, or does it appertain equally to the interior parts of the brain? If the latter is true, another question at once arises: What gain accrues to the psychic life from the activity of the cortex?

An approach to the solution of these questions has been made, it is hoped, by an inquiry conducted by me during the year 1897. It was important that I should have an abundance of observations at my disposal. The following appeal was therefore sent to a number of journals devoted to fishery and aquarium interests and to some scientific publications at home and abroad, and much to my satisfaction it was copied in the daily press.

"HAVE FISHES MEMORY?

"*A request for information.*

"It has been generally assumed that to a certain degree fishes possess memory, that they know persons, that they are able to find or avoid spots in which their experiences have been pleasant or the reverse, and, having once escaped the hook, they thereafter recognize it, etc.

"The advance of psychology makes it desirable that pertinent experiences should be collected for the following reason: Up to the present time the opinion has prevailed that the function of memory is mainly dependent upon the presence of the cortex of the brain. Of the part played in this particular by the inner portions of the brain we know nothing. Scientists have succeeded in demonstrating that the brain of fishes lacks the slightest trace of cortical substance. If, now, it can be proved beyond the shadow of a doubt that these animals gather experiences and apply them to subsequent situations—that, in other words, they possess memory—then the accepted doctrine, that only the cortical substance of the brain confers the power of memory, falls to the ground, and entirely new problems face the scientist.

"Therefore it is extremely important that all pertinent observations should be gathered together again and elaborated anew.

"The undersigned requests all in a position to observe fish, especially anglers and fishbreeders, to be good enough to send him data bearing upon the subject. In particular, he would beg that facts long accepted as such, if their correctness has been tested by recent observations, be communicated to him."

This appeal was attended by most satisfactory results. Within a few months I received an abundant crop of communications from all parts—from Germany, England, France, North America, Siam, India. It was surprising to note the interest of breeders, anglers, students of nature, and dilettanti. Equally surprising and delightful was the circumstance that comparatively few of the communications proved wholly worthless. The majority of my correspondents knew how to observe and report with unassailable accuracy. Two-thirds of the two hundred letters available were written in German, one-third in English. Only one French correspondent addressed me. The appeal was composed with a view to eliciting as much material as possible relative to the "intelligence," etc., of fishes. The motives for the investigation were not laid bare in their entirety, and the wording was made as simple as possible. The happy result was that numerous letters, many of them among the best that came, were received from persons like fishermen, attendants in aquariums, etc., whose occupations bring them close to fish. A gendarme, for instance, was moved to institute an investigation of his own among a number of fishermen, etc., and he sent me his results detailed with clearness and precision. I take this opportunity to express my thanks to all my correspondents.

The brain of osseous fishes is pretty accurately known. The nerves of sense, like those in the higher animals, lose themselves in their primary termini, but not the faintest path can be traced from these to anything resembling cortical substance. There is absolutely no cortex. These animals, then, must confine their activity to the primary termini. Accordingly, the ganglia are very much more developed than the corresponding ones in the mammalia. They do not, to be sure, differ essentially in composition from the ganglia of other animals, but what there is of them is developed more robustly. The fibers that appear only sparingly in the mammals, the ganglia which in the higher animals are only rudimentary, are strongly in evidence in fishes, and lend themselves readily to examination.

If, now, we wish to find out the functions of this apparatus, we must begin by determining the sense impressions fishes can receive. Do they see, or hear, or feel? Are they, perhaps, in possession of sense qualities lacking in other animals?

The reply to these preliminary questions must be our first task.

Whether a stimulus takes effect or not can be inferred only from the movements following upon its application. In the first steps of

the investigation we need not consider to what extent the stimulus is not only felt and responded to, but perceived. As is well known, even man, equipped as he is with a very acute capacity of perceiving, often responds to stimuli which he does not perceive, if his attention is not particularly directed to them. In fact, he can not recognize all the stimuli that excite him. In the course of the following investigation, perceptions will be spoken of only in cases when their presence is capable of being distinctly proved.

Another reservation must be made before we examine the sensations attained by way of the system of nerves. In the world of animals, as well as in the world of plants, there is a series of phenomena obviously independent of the intervention of a nerve apparatus. Not only plants, but also animals show the effects of heliotropism (phototropism)—that is, the tendency to turn toward or away from the light. These effects appear even in animals in which not a vestige of a nervous system can be demonstrated to exist. Similar phenomena, common to the lower animals and plants, are known in connection with heat, with chemical agents of irritation, with the tendency of bodies to seek a state of equilibrium. We have not yet discovered the mechanism or the disposition of the plasma at the root of these phenomena which can be evoked and which disappear with the same conformity to law as, let us say, the motion of iron filings toward a magnet. These various "tropisms" are of widespread occurrence, and their influence upon the general condition of organic beings has been carefully studied. The upper limit of their occurrence, in the ascending scale of animal orders, has not been determined. But we have no reason for supposing that the "sporting of the merry little fish in the sunlight" involves other processes than the mounting of the larvæ of lower marine animals to the sunny surface of the water, or the conduct of certain bacteria that constantly seek the part of their habitation exposed to the light. The relation of these primary biologic forms to light is as characteristic and as much subject to law as that of iron to the magnet.

The conduct of the youngest brood of fish, still attached to the yoke-sac as they swim about, is doubtless regulated by law with regard to light, the heat of the surrounding medium, and probably many other conditions of the outer world; it is born with them—rooted in their organism. Most probably the phenomenon usually called "flight" should be classed among "tropisms." It is present at a time at which a developed nervous system is out of the question. Another accomplishment existing at birth is the coordination of many movements. They depend as much upon the structure of the muscles as upon that of the nervous system. Schaper destroyed the nervous system of frog eggs in the early stage of development. When they were older, he saw them swim about, although subsequent investigation showed

that in the main they lacked nerves. Such a thing as learning the swimming motion is not a probable conjecture, in spite of the apparent necessity among higher animals to make an effort to acquire the motion of walking, flying, etc. A great part of this effort is found to be nothing more than a strengthening of the immature muscular system. Microscopic examination of the anatomy of the spinal cord reveals that all the fibers and cells necessary to put into motion the organs of walking exist long before a human being learns how to use his limbs. At all events, the most exhaustive study of the spinal cord fails to indicate that any essential change sets in after the first year of life.

Inquiries into the sense impressions of fishes are not numerous, and as most of them have been conducted by laymen, they, as a rule, take no account of published results. In fact, the literature on the subject has nowhere been collected in proper form. Doubtless some works have, therefore, escaped my attention. The best I am acquainted with is that by Bateson, on the conduct of fishes with reference to sense irritations that act upon them under the normal conditions of their life. Bateson[1] studied very many species of fishes, especially in their relation to food and breeding, at the marine aquarium at Plymouth.

As for the response to chemical irritations by the two senses of smell and taste, which in aquatic animals can not be distinguished from each other, it appears that the discriminating faculty is slight. The conger-eel ate meat smeared with spirits of iodoform, trimethylamine, spirits of camphor, and extract of anchovies. It refused, however, to touch cooked meat or meat treated with acids, due perhaps to the sense of touch residing in the organs of the mouth. Other fishes that have been observed act similarly. Smells did not disturb them; they were oblivious of stones smeared with the above substances; they paid no attention even to putrefying roe—it was the spawn of another family of fishes. A number of fishes, however, of which the flat-fishes (flounders, etc.), are a type, find their food chiefly by means of chemical sense impressions. They may be lying quiet when the food or the juice of food is introduced into the aquarium. When the odor issuing from it spreads, they grow restive and seek until they find the food, or, in case only the juice was dropped in, until they finally grow weary. *Motella tricirrata* fails to see worms moving quite close to it, but as soon as it scents them it moves to and fro restlessly until it happens across them. If the organ of smell was removed from this fish, it could not find its food, although its barbels and its eyes were perfect.

In no instance has an animal of this type, though equipped with sight, recognized food through vision.

[1] W. Bateson, The Sense Organs and Perceptions of Fishes, with Remarks on the Supply of Bait. (Journal of the Marine Biological Association of the United Kingdom. Vol. 1, 1889-90, p. 225.)

Anglers, as is well known, still lay great stress upon a sort of bait whose chemical properties operate at a distance. The belief is that fish can be lured to a given spot by certain substances. My correspondence goes to prove that this supposition is open to many exceptions.

Doubtless the reception of mechanical or tactile impressions, in other words, the sense of touch, occasionally has a part to play in the feeding of fishes and in their life as a whole. Bateson reports that the barbels are brought into requisition only after the food has been reached. *Protopterus*, *Motella*, and others distinctly make use of their barbels to investigate the food whose presence has announced itself through the sense of smell. Probably only the region about the mouth responds to touch irritations. An observer can readily convince himself that, if he avoids being seen or himself moving the fish, any part of its body as it lies in the aquarium may be touched, and the fish will not change its position. Additional experiments on this point would be highly desirable.

That fishes are open to light impressions appears from the fact that some, soles for instance, can change their colors according to the hue of the subsoil above which they happen to swim. Dunn recently reported his observations upon soles. (Contemporary Review, 1899.) Not only did he see light colored individuals turn black over dark soil, but when he carried them home in a pail with a dark bottom he found them all darkened. Dunn's essay, which, it should be said, ignores the results of all past investigations, also contains statements about the existence of electric and magnetic sensations, but they have been reached by way of pure speculation.

In general the behavior of fishes with regard to light proves that they receive light impressions. Some feed only in the dark, some rise to the bait only on days of a certain degree of cloudiness and at other times keep themselves far below the surface of the water, and it is known that a shadow suffices to drive away a shoal of fish.

It is universally agreed that fishes see, that is, are aware of images produced optically. Their vision apparently gives rise to a comparatively acute faculty of discrimination, for, as every angler knows, artificial bait is valuable only if quite up to the mark in certain respects, and the price lists of dealers in sporting articles show, by the multiplicity and variety of the artificial insects, fish, etc., offered for bait, how high an opinion fishermen entertain of the optical discrimination of their intended victims.

Again, fishermen carefully conceal the hook in the living bait, believing that fish will notice the slightest edge protruding. Doubtless fishes receive the impressions of form and color, and distinguish optically between rest and motion. Many are guided by optical impressions in taking their food. Bateson could not decide whether fishes that respond

most readily to chemical irritations see ordinarily. On account of the conditions in the aquarium, he could come to no definite conclusion, and he supposed that the darkness in the depths of the water might bring about different conduct. More than a hundred of my letters report that fish in ponds and aquariums swim close up to the attendant as soon as they catch sight of him, and in a number it is explicitly stated that the color of his clothing or an habitual movement of his at once attracts the shoal.

Do fishes hear? Of late years it has been determined that two different sense organs lie close to each other in that part of the body usually designated as the auditory apparatus. The labyrinth, beyond a doubt, regulates the action of the muscles for the orientation of the body. The cochlea, on the other hand, seems to serve only for the reception of sound sensation. The former apparatus, or an apparatus of equal functional value, occurs alike in vertebrates and in invertebrates, as, for instance, in crabs. The real organ of hearing, however, the cochlea, is not met with lower down than the amphibians; fishes lack it entirely. It is, of course, possible that the latter may be able to receive sound impressions through some part of the labyrinth. In fact, it would seem to be the case in view of the numerous reports about fish summoned to be fed by the ringing of a bell. Despite his many experiments in the aquarium, Bateson could never satisfy himself that his fish actually heard. Only very loud noises, such as the firing of a shot or an explosion on the street, disquieted them. A large number of my correspondents mention that fish, especially of the carp-like forms, came to be fed at the sound of a bell or whistle. One of them relates that trout were attracted by the barking of a dog in the habit of accompanying the attendant. But, in all these cases, the possibility of an optical impression is not excluded absolutely. Herr W. von Derschin, a breeder of wide experience, writes me that in his judgment the bell plays no part at all.

Kreidl's experiences, gathered at a pond where the fishes were summoned by a bell, made him suspect that they were attracted by the appearance of the attendant or by the percussion caused by his steps. He instituted a careful investigation, though it did not extend beyond goldfish in an aquarium. Sound waves originating in bells and whistles were conducted through the air to the fish, or reached the fish through the water by means of suspended springs operated by electricity. The fish were observed from a distance with the aid of a mirror. He did not notice anything to indicate that the sounds had been heard by the fish. He then made them more sensitive to impressions of all sorts by poisoning them to a slight degree with strychnine. Even then their movements betrayed no consciousness of the sounds. On the other hand, they were made restless by a clapping of the hands, the report of a revolver, or a stamping of the foot. The same restlessness was pro-

duced in fish whose labyrinth had been removed. Many of the accounts sent to me mention the fact that fishes in a lake are disturbed whenever a cannon shot is fired on shore. In his book on animal life in the Austro-Hungarian plain, Aug. Wojsisovics von Wojsvar tells, as I see in the "*Prometheus*", that the Servians thrust the *bucskalo*, a wooden instrument, into the water with an abrupt motion, in order to attract the sheath-fish [*Silurus glanis*]. But possibly the strong undulation gathering force in the water, rather than the sound, informs the fish of the moving of a heavy body at a distance.

From the data at our disposal we must infer that fishes are aware of violent percussions, such, too, as are caused by sound waves, but it is most doubtful whether they receive impressions of sound as we ordinarily use the word.

In the skin covering the head of fishes are situated numerous delicate sense organs, and similar ones are arranged in a line extending from the head to the tail on the side of every fish. This lateral line can always be recognized in the scaly animal. Issuing from the brain close to the nerve of equilibrium a vigorous nerve supplies the entire apparatus. Manifold observations in the past seemed to indicate that this apparatus was sensitive to variations of pressure exerted by the water, and that therefore it is well fitted to enable the animal to adjust itself to its fluid medium. Moreover, the same lateral line occurs in amphibians living in the water, and disappears in such, like frogs and salamanders, as remain on land in a second period of their life. Fish whose sight has been removed continue to avoid obstacles fairly well, and they are aware of the unobtrusive glass covering of the aquarium walls unless some special impulse sends them darting wildly hither and thither. Stahr tells of the male of a Chinese pet fish, that, when courting, arrayed in all the glory of his wedding finery, he is in the habit of rushing toward the female with great vehemence, and then suddenly subsiding, his breast fins spread out wide, without so much as touching his mate. The behavior of the female shows that somehow she is aware of these repeated concussions communicated to her only by the water. In agreement with older authors, Stahr supposes that the knowledge reaches her by means of the apparatuses situated in the lateral line. In fact, on their removal (Richard), the injured animal completely loses its balance, and Bonnier has proved conclusively that fish so treated lack the ability to maintain their equipoise against the various disturbances of the water. From fish whose lateral line had been destroyed with a hot platinum wire, he also took other sense organs, the eyes, the ears, in various combinations. It appeared that the labyrinth and the lateral line alike serve to receive shocks and differences of pressure, but that the special function of the lateral line is to receive the impression of the direction of a shock. Fuchs succeeded in proving in individuals with unimpaired sense organs that the

lateral line and certain similar apparatuses in the head of sharks are brought into action by no irritations other than those produced by variations in pressure. The ingenious series of experiments by which he arrived at his result deserves special mention. Through every nerve runs a so-called current of action, demonstrable by means of electric appliances. At the moment when a nerve comes into action the intensity of this current is somewhat diminished. Not all irritations produce this result; only such as actually force the nerve into action. For instance, the current of action in the eye is weakened when light falls upon it, not if the eye is excited in some other way. Fuchs measured the current of action in the nerves of the lateral line. He noted a diminution in intensity only when the uninjured fish was exposed to variations in the pressure exerted by the surrounding water. These oscillations, then, are the irritations to which the apparatus in the lateral line responds.

Let us sum up briefly what conclusions may be derived from our data concerning the sense impressions of fishes. They respond to chemical irritations (sense of taste and smell); they receive light sensations, gain optical images through their eyes, and can see. It is questionable whether, properly speaking, they can hear, but they are aware of violent commotions in the water, even such as are produced by sound waves. Finally, it appears that they possess additional sense organs in the canals of the head and along the lateral line which permit sensations corresponding to variations of pressure exerted by the surrounding medium. With regard to this sense apparatus, we know not only the end organs at the surface of the body, but also the nerves and their termini in the brain. We know, furthermore, that not a single one of these nerves extends beyond the first ganglion it reaches, but there are fiber strands which join all these end ganglia to one another in definite, constantly recurring ways. The question now is, Is the above apparatus fitted to retain impressions brought to it? Is there any lingering effect from past irritations?

As we have absolutely no knowledge of the psychic processes that fish may undergo—in other words, of states of consciousness accompanying the various movements visible to us—our descriptions must make use of a nomenclature which, as far as practicable, eschews the terms used for similar processes in man, for in order to keep strictly within the bounds of science we must refuse to entertain any supposition not forced upon us by given phenomena. We shall regard the animal as a machine, but by no manner of means is this attitude to be taken as a prejudgment of psychologic data of which we are still ignorant. A machine always responds to the touch of a given key or valve with the same motion; the relation between irritation and movement is absolutely simple and strictly regulated by law. Such simple relations are not unknown in the animal kingdom. They are charac-

teristic of the phenomena described above as "tropisms." Plants and the lower animals never vary their behavior toward light, heat, etc. Likewise a number of so-called simple reflexes follow, with approximately the same regularity, instantaneously upon the irritations producing them. The question is whether so low an order as fishes can acquire or whether they possess reflexes that can be modified, inhibited, or hastened by new sensations. Can impressions new to the animal exercise influence upon its conduct? Above all, can they maintain this influence for any length of time?

A peculiarity present in even the youngest brood of fish is a recoil from sudden optical or other light impressions.

This "flight reflex," as we shall call it for the sake of brevity, is retained by all fishes beyond the stage of maturity. It can be heightened—"the fish are timid;" it may be lessened—"the fish are getting tame." That fishes grow tame is reported in more than a hundred letters. In most cases the fish that had been observed were gold-fish, which during confinement in aquariums had learned not to flee before their accustomed attendants. The same, however, is reported concerning trout and other varieties of fish, even selachians. In many instances fish became so tame that they allowed themselves to be seized by persons they knew, taken out of the water, and replaced in the aquarium. By an observer in the Laboratoire de Zoologie et de Physiologie Maritime du Collège de France, at Concarneau, I am informed that a dog-shark (*Scyllium catulus*) not only swims up to the attendant who brings food and permits the latter to stroke him, but occasionally, on catching sight of the attendant, he works himself up with his tail and his fins in an angle of the glass panes of the aquarium, so that his head protrudes above the surface of the water. The same was noticed in a conger eel in the Laboratoire. Von Mushage, in Sablon-Montigny, reports that a mud-fish (*Cobitis fossilis*) which he had repeatedly caught when cleaning the aquarium now slips into his hand of its own accord and lies there curled up. Walther tells of a trout which, on first being put into the aquarium, was in the habit of jumping up out of the water whenever the door was opened; later it ceased to do so. From Takkamen, Siam, S. S. Flower writes that a shoal of *Trichogaster*, extremely shy when first confined in the aquarium, later on came readily to be fed, and certain individuals submitted quietly to being taken out of the water. For years a gold-fish was in the habit of coming up to a setter dog and playing with his tongue, and even tried to do it through the glass panes of the aquarium (H. Mullert, New York).

This tameness usually loses itself if conditions undergo an essential change—usually, not always. For instance, a trout that had been removed from a pond to a little fish globe, and had long been fed there, on being returned to the pond continued to feed from the hand. As a rule, however, fish resume their timid ways with a change in the conditions

under which they lost the "flight reflex." Numerous examples were reported. Herr Wallau, of Mayence, had tamed a rainbow trout to such a degree that it took food from his hand. If he caught it by the tail and raised it out of the water it would not approach him for three days. Similarly a macropod, which Herr Schott, of Ludwigsburg, had repeatedly teased with a little board, for some time kept away from him at feeding hours. Many observers noticed tame gold-fish resume their timorous habits after being chased by cats or blackbirds. In general, fish, even such as have not been tamed, grow particularly shy by being chased and disturbed. At a water gate near Raunheim numerous varieties of fishes, as Herr Buxbaum determined by marking individuals, take up their position for days together in the eddy richly impregnated with oxygen. They rise quite to the surface, and can easily be removed from the water. This state of affairs continues only a short time. Soon fish-eating birds collect about the sluice, and at first their booty is plentiful. Before long, however, the whole congregation of fishes sink as deep down as possible into the water. I am informed that a pike which had taken up a permanent stand left it when it had been shot at; two others acted similarly when they had come into contact, respectively, with a pike hook and a net. Von Link, in Stuttgart, tells me that after he had fired several shots on the same day into a group of rather large dace they disappeared whenever he came in his uniform. Landois reports that in the zoological gardens a little grebe (*Colymbus minor*), which had taken rich booty in an aquarium stocked with four or five hundred fish, soon was avoided by them. They hid in a corner of the aquarium, behind the water-supply pipe, where they cowered in a ball as thick as one's fist. Before they had been gaily scattered in all parts of the reservoir. I do not agree with Landois that this incident proves a comparatively high development of the psychic life of even little fishes, such as the common stickleback, the bitterling, the red-eye, etc. To explain the fact that after satisfying their "curiosity" they soon recognized their "enemy" and sought a well-situated place of refuge, the assumption suffices that pursuit intensified, or rather, after taming, renewed the "flight reflex." At all events, we are not absolutely forced to attribute to the fish conscious action serving a preconceived purpose.

Fishermen are well aware that places in which a great deal of fishing has been done are for some time avoided by fish. Various experiences seem to point to the fact that fishes in some way recognize, or at least avoid, objects believed to be noxious to them. Thus fishermen hold that the hook must be carefully buried in the bait or the fish will not bite. Many reports maintain that pike once made acquainted with wire nets thereafter avoided them. Herr Bueröb, master of the fisheries in Weimar, writes that, though a large pike did not recoil from persons passing his permanent place, even when tickled with a whip,

it disappeared instantly at sight of a pike hook. Later it was found that it bore the marks of having been pricked on a former occasion. If a net is dragged through a swarm of carp, kept and fed for breeding purposes, various observers report that for a week or two they remain away at feeding time, and nothing can induce them to appear. Mr. Mullert, of New York, whose numerous observations I have had to quote frequently, mentions that the fish bred by him in large quantities, which as a rule swim close up to him, flee if he holds a little net in his hand when he opens the aquarium. The same is reported by Von Quasowski, in Constance.

According to various observations, swarms of fish absent themselves if one of their number has been caught with the hook. Semon, for instance, reports this concerning a shoal of *Echeneis*, observed by him in the Torres Strait. The same is told me of the bream (*Abramis brama*) and the dace (*Idus melanotus*).

The above experiences in connection with the taming of fish may suffice to prove that impressions once received can be retained. The same conclusion is forced upon us with still greater clearness by the letters, in the neighborhood of 150, that concern themselves with the behavior of fish at feeding times, whether in ponds or streams or aquariums. The statements in all are so absolutely identical that the reported facts may well be accepted as the result of the aggregate experiences of all observers of fish.

Goldfish, long accustomed to be fed, grow so tame that they always come close to the spot at which the keeper appears to feed them. An interruption of months in the practice of feeding does not cause loss of the habit. Herr M. Schmidt-Metzler, in Frankfort on the Main, reports that his goldfish, which pass the winter in a hothouse, swim up to him the very day they are put back into the pond in the spring. Mr. H. Mullert (Brooklyn) writes that various sorts of goldfish and tench, kept in different aquariums, take food from his hand, and some allow him to caress them. For four months every year they are released in a pond. While there they are timid, and do not come up to the owner or attendant, but as soon as they are back in the aquarium they feel at home, and without fear swim up to the attendant that feeds them as though sixteen weeks had not intervened. The same is told of the perch, the *Scaphirhynchus* [shovelnose sturgeon], minnows, bitterlings, tench, sheath-fish, trout, and various species of carp. In many cases fish follow the keeper for some distance as he walks along the side of the pond.

Certain marks of an optical character seem to bind the fish to the attendant, although the opinion advanced by one observer, W. von Derschau, that they do not know the attendant as an individual, must be indorsed. Ives (Crookston) tells that in his trout hatchery the attendant wore a scarlet coat; anyone that put on this coat could lure the fish

close to him. Herr Jaffe, in Santfort, a breeder of great experience, also noticed that the clothing of the keeper went far in determining the confidence of his trout, and that they seemed loath to approach him when he changed it. Otto Zacharias (Plön), likewise an authority on the habits of fishes, reports that his fish approach him to be fed as soon as they see him; and the fish breeder, P. A. Wallau, of Mayence, observes great restlessness, jumping in the air, etc., among the trout in his ponds whenever the attendant throws up his arm as though to cast food into the water. Henri de Parville relates that in the gardens of the Luxembourg the fish, which are fed by a keeper in uniform, always swam up to the sides of the pond at the approach of two cadets from the military academy, who wore similar uniforms. Mrs. Johnston (Bethlehem, Pa.) maintains that her fish take food from her and from the physician attending her, but from no other visitor. Herr A. O. Bernhardt (Warsaw) observed that carp took food from the hands of certain ones of the attendants, but dived out of sight when others offered it. Hutt (Folkestone) fed *Blennius pholis* from a long slender stick; later he saw it jump up at the stick when no food was attached to it.

Optical impressions, however, do not seem to be the only ones retained. Benedict regularly fed his fish at the moment when a freight train passed, and was astonished to find that the fish came to him when the train passed, though no food was offered. But that may have something to do with the time of day; the regularly fed animals grew hungry at the accustomed hour. It is known that carp habitually rise to the surface of the water with a smacking noise at the time of the evening meal. That percussions attract tame fish is reported also by C. Fallon (Philadelphia, Pa.); he could summon his goldfish by stamping his foot on the side of the pond. Many of my correspondents believe that the behavior of fish toward the hook argues the possession of some sort of memory.

The matter is not quite so simple as appears on the surface. The process of taking in food involves many circumstances that as a rule are not sufficiently taken into consideration.

If food is placed on the tentacles concentrically arranged about the

new animal is formed in miniature, lacking the mouth cavity. If food be laid upon the tentacles so produced, they contract again and again to the point of fatigue, but they can not press it into the body. The act, then, is accomplished, in these low organisms, by an apparatus present from the first, and put into efficient motion by the proper stimuli. High up in the ranks of animal life the act of mere swallowing is executed by means of a bit of mechanism operating with unvarying regularity. Human beings may take or refuse to take food, but as soon as a bite passes the arch of the palate and enters the domain of the swallowing apparatus, properly speaking, it is beyond their control. It is then caught up and pushed along by a series of pharyngeal movements that may occur in individuals bereft of all consciousness. The sea-anemone, an illustration that might easily be multiplied, proves that an irritation is needed to induce the act of eating. The sort of movements brought about by means of closely connected, long-plowed paths of sensation and motion, and always capable of being called forth by the same irritation in the identical way; such movements are called combined reflexes. To cause them, the first condition is an irritation of the proper kind, but the irritation must furthermore be of a certain degree of strength, which in turn can be attained by the short application of a forcible stimulus or the long-enduring application of a weak stimulus. We saw above that in the higher animals the act of eating divides itself into two parts—deglutition is automatic, but for the seizing of food other stimuli, apparently dependent upon the will, must be called into action.

A striking example is available to show that the first division of the act of eating also occurs only in response to a stimulus of a certain degree of intensity. During the day a frog is a rather inert animal. He spends most daylight hours quietly, in a sort of doze. He is peculiarly fitted to serve the purposes of this demonstration, because often long intervals elapse during the day between his times of taking food. We have numerous descriptions of how the frog eats—how the "sly robber" eyes his booty, how he sits quietly for a long time before he seizes the "enemy lulled into security by his immobility," etc., how he executes many deeds of crafty cunning. Simple observation teaches otherwise. An earthworm crawls in front of a frog. Unless he is very hungry this simple optical irritation does not suffice to induce him to spring upon his prey and seize it. Now, the worm moves on, the optical stimulus gathers force, and finally it calls forth the first reflex act—the frog turns his head toward the worm. He is not yet ready to spring, but the longer the impression of the crawling worm acts upon him the more restless grows the frog, and at last he jumps forward with a bound, often enough missing his prey. What he has done so far must be classified wholly under well-known laws of reflex [illegible]n. There is nothing to suggest greed of booty or cunning; all

we notice can be adequately explained by our physiological knowledge. But the act does not always take so slow a course. It appears that the frog sometimes responds more readily to irritations; that he sometimes advances upon the worm more alertly than in the above description; that he turns his head at the first movement made by the worm. Hunger, the prevailing temperature, above all the frog's condition after he has taken his first bite of food, increase his sensitiveness. Then he eats greedily as much as he can lay hold of. On what this state of heightened excitability depends has not been determined. In this more sensitive condition a new element is observable. The frog needs no constant stimulus. Having once responded to the irritation, he goes on executing a series of acts calculated to serve a definite purpose. He begins to crawl after the escaping worm; he chases his prey. Obviously a new element has supervened. The earlier sensation continues in operation, and calls forth action adapted to bring about an intended result.

Numerous experiments made by physiologists have proved that besides a certain intensity of the irritation, a variety of other conditions must be fulfilled in order to produce reflexes. Equally these experiments have demonstrated that in certain circumstances reflexes may be inhibited by the central system of nerves in animals equipped with a brain. We are not dealing with uncertainties, nor are we setting up a hazardous hypothesis, when we assume that the taking of food may be inhibited when other irritations—of an optical nature, for instance—are stronger than those exerted by the food.

Though in the present state of science we are far from taking account of all the circumstances that induce the higher animals to feed, still it is known that the component elements can be analyzed, and that the main concern is the intensity of the optical, chemical, etc., stimulus issuing from the food, the disposition of the body when the stimulus is put into operation, and the inhibitive influences at work at the time.

Fishes approach their food only if other sense impressions of great vividness are excluded. If they are disposed to eat it, hunger, the quality, and perhaps the electricity of the air and the water play a distinct part, and, above all, if the general behavior of the food is such as to exert a stimulus of the proper character and of sufficient intensity to call forth the reflex act of eating. If these conditions are not fulfilled—if, for instance, the artificial bait in some important particular fails to resemble natural bait, or if the movements of a badly impaled worm are abnormal, or if the effluvia from the hand of the fisherman has imparted an unusual scent to the bait—then the irritation is unfit to produce the expected reflex. Again, the response depends upon the characteristics of different species of fishes. There are some that approach their food slowly and cautiously; others throw themselves upon it headlong. The inert carp-like fi[illegible]e vivacious sal-

monids typify the two extremes well. The degree of hunger felt impedes or facilitates, as the case may be, the production of the series of reflexes. It can be imagined, too, that certain acute irritations, such as from wounds, etc., exert a restraining influence with regard to the taking of food. Furthermore, every angler knows the effect of changes in temperature and weather upon fishes in their relation to food. About thirty correspondents tell me of fish of prey which still carried the hook they had torn from the line in their mouth, and permitted themselves to be caught with another immediately upon their escape or after an interval. Such cases do not prove, as my informants think, that these fish have no memory. They are as little able to judge by the appearance of the second bait as they had been by that of the first that a hook was concealed in it. The same trick may deceive even human beings several times. Moreover, we do not know whether fish feel pain when the sides of their mouth cavity are pierced. Indeed, a number of facts make it seem doubtful whether what human beings designate as pain extends very far down into the lower order of animals. Fish of prey, endowed with a keen desire for food at all times, suffer enormous injuries without suffering corresponding impairment of appetite. Dunn (Contemporary Review, 1899) reports that a shark, which was caught with the hook, and was opened in order to remove the liver for medical purposes, and was then returned to the water half dead, soon after was caught again with bait. He also saw a Motella caught with the hook whose stomach was pulled out so far that it hung from under the gill bars. He supposed that the fish had suffered this serious injury from the hook of an earlier angler from whom it had escaped. In the supplement to the Allgemeine Zeitung, 1897, No. 213, E. St., in an article describing his journeys in the South Pacific, relates that sharks which have slipped from a fisherman's hook, and in doing so have torn their upper jaw, almost always rise to and swallow the same bait. In the stomach of one shark he found, besides the claw of a crab deeply embedded, a piece of corroded iron, which, after piercing the walls of the esophagus with its pointed end, had inflicted an injury upon the pericardium, the scar of which still showed. On closer examination the iron turned out to be an old shark hook of very nearly the same kind as the one with which the fish was eventually caught.

The same disposition has been noticed in *Esox*. Fifteen or twenty minutes after it has been grappled it allows itself to be caught with the same hook.

The avidity with which hungry fish rise to bait varies in closely related species. *Salmo salvelinus* [the charr] and *Salmo iridea* [rainbow trout] occasionally snap at a moving finger, *Salmo fario* [the European trout] never.

Many fishermen insist that fish "know" the hook. To me it seems

doubtful. However, I will quote from my correspondence a few particularly confident statements. The more cautious fish are said fairly to study the bait before they touch it, and they never allow themselves to be deceived a second time, fish of prey being the only ones inclined to swallow the same hook twice. Even of the latter it is reported that having once been caught they grow more "careful." Several correspondents report that trout which have nibbled at bait and have escaped swim up to the hook, but dart away from it at once, and that pike whose companions have been caught with a fork or a net avoid the net for months. It is questionable whether an observation made by Roland Müller, Mochenwangen, bears upon this point. He is in the habit of fishing for trout with the yellow-gleaming minnow Devon bait. At first very many of them nibble at the bait; soon the number diminishes, and he asserts that he has noticed that such as take up their stand at a definite place can not be induced to bite a second time in a given year. Von Tschusi, of Schmitthofen, likewise reports that a trout whose permanent station was well known had once been grappled with a hook, and it refused, for a whole year, to come near a hook baited in the same way. When it was finally landed with another sort of bait, the thread was still in its mouth. Possibly in some of these cases the explanation is that, on account of their hurt, the fish refuse food altogether, and hence are not lured by the bait. In view of the variety of processes that go to make up the act of food taking, the conclusion is inevitable that their behavior with regard to the hook indicates neither the presence nor the lack of the function of memory in fishes.

Several of my correspondents dwell upon a famous pike experiment, first tried, it would seem, by Möbius. Their opinion is that the result can be explained only on the hypothesis that memory exists in fish. A pike in an aquarium is separated by a pane of glass from little fish which he is in the habit of eating. At first, the reports say, he throws himself against the glass and hurts his snout. After a time, even though the pane is removed, he makes no attempt to reach the small fry.

This experiment is not conclusive. In the first place, I venture to doubt that the pike which, guided by the organs in the lateral line, avoids glass partitions with extraordinary skill, loses his cunning precisely in the case of a partition separating him from his food and throws himself against it with such force as to bruise himself. Again, a number of persons have assured me that in aquariums exposed to light pike rarely attack other fish for feeding purposes. A large dealer in fish here has for years been keeping pike with other fishes in the aquariums in his show window without ever losing any of the latter. When he feeds his pike, he must carry them down into a dark cellar. If, then, in the first place, it is improbable that the experience of the pike engaged in attacking his little neighbors was unpleasant, it can

scarcely be held that pike exhibited with little fish restrain their appetite only because their experiences in attempting to devour little fish had apparently been forbidding.

This exhausts the material offered by my correspondence. The aggregate of the observations made by several hundred persons seems extremely insignificant, but what there is is secured by the fact that almost every item was observed at various times, by different persons, in widely separated places, and often with regard to species of fish not related to one another.

The very limited capacity of fish appears when we realize that only the following few facts could be deduced from the great amount of material furnished me by observers: 1. The innate impulse toward flight can be lessened in fish by accustoming them to impressions as a rule alarming; but tameness so acquired is lost when new stimuli supervene. The impulse toward flight may appear even in consequence of stimuli never present before. The fish get shy. 2. The optical or chemical irritation usually calling forth the act of taking food may by long custom be replaced by some other, as, for instance, the optical image of the attendant charged with the duty of feeding the fish.

In all ascertainable cases it is simply a question of a change of front toward a definite irritation. Fish which as a rule swim up to their food when not disturbed by irrelevant impressions learn to subordinate them to such an extent that they approach food even in their presence, or at least do not flee. They also learn to approach food when irritations other than those proceeding directly from the food acquaint them with its presence. They not only swim toward crumbs, but the sight of the person who usually scatters the crumbs attracts them even when no food is in sight.

Nothing prevents us from grouping these facts under the concept of memory. We are, then, in position to say that vertebrates so low as fish possess a sort of memory, widely differing, by many gradations, from the memory of mammals, the only sort hitherto studied. Compared with the latter, it is a very much simpler process, the peculiarity of which lies in the close connection existing between the irritation and the response. Not a single fact forces upon us the assumption that these simple processes are accompanied or dominated by the mental process of associating ideas. An observation of Bateson's offers a pregnant illustration of the difference in this respect between fishes and other animals. Motella, as was told above, finds food only by means of the sense of taste or smell. It does not become aware of its presence through any other channel, even if the food enters its field of vision. One of this genus was kept in a shallow reservoir in the Brighton Aquarium, where it was often fed by people bending over the sides. This individual learned to rise to the surface when it was

hungry and to snap at anything that came its way, even at a finger held out to it. In other words, it learned to eat in response to an optical stimulus. Even then, however, it did not seize an earthworm crawling through the water. To produce the act of eating it was necessary that the optical irritation should be unvarying, and should proceed from the surface of the water. The obvious inference that the worm already in the water might be edible was not made. However, to be conclusive, the experiment ought to be repeated and more carefully studied, for it is possible that the vision of motella is defective under water. In general, my investigation has led me to believe that in using fish in aquariums for experimental work many problems present themselves whose solution would not be excessively difficult if the questions were put with precision and if the observer took good care not to read more than they warrant into the results of his inquiry.

In the above presentation psychologic problems in the narrow sense of the term have been avoided. To make headway on this field of inquiry we must for the present confine ourselves strictly to observation. Above all, we must take heed not to read into our observations the probabilities that might be sanctioned by reasoning from analogy. Therefore no attempt was made to formulate an attitude with regard to the question: Do fishes know anything of the processes described? Have they any sort of consciousness? At present such problems are impossible of solution. I found no observation making inevitable the opinion that fishes not merely are open to impressions but are actually aware of them, and that they were in a single instance influenced by them to change their conduct in a way possible only when an impression has been observed, has been meditated on, and is applied in a subsequent emergency. All the phenomena were capable of a simpler explanation. For a stimulus to evoke a secondary effect it must not necessarily be observed, and its later application does not absolutely demand conscious memory. The science of to-day is not aware of phenomena necessarily involving the recognition and use of stimuli as such until the higher animals are reached. It is probable that the seat of this highest function is to be sought in the cortex of the brain. Moreover, the cortex alone is plowed through with the paths of association sufficing for the manifold coordinations that are wholly lacking in fishes.

SCIENTIFIC THOUGHT IN THE NINETEENTH CENTURY.[1]

By William North Rice.

It is an interesting fact that the life of our association is almost coextensive with that nineteenth century of Christian civilization which is now drawing to a close. In intellectual, as in physical phenomena, we are tempted to overestimate the magnitude of near objects and to underestimate that of distant ones; but science and art tend to advance with accelerated velocity, and we are undoubtedly right in ranking the achievements of our age in science and its applications as far greater than those of any previous century.

When our predecessors assembled a hundred years ago to organize this Academy, they could avail themselves of no other means of transportation than those which were in use before the time of Homer. If the distances over land were too great for convenient walking, they were carried or drawn by horses. If they had occasion to cross bodies of water, they used oars or sails. We have been brought to our destination to-day by the forces of steam and electricity.

The harnessing of these mighty forces for man's use has transformed not only the modes of transportation, but the processes of production of all kinds of commodities. It has wrought a revolution in the whole industrial system. The day of the small workshop is gone. The day of the great factory is come. Every phase of human life is affected by those arts which have arisen from the applications of science. Comforts and luxuries which a hundred years ago were beyond the reach of the most wealthy are now available for the use of even the poor. Aniline dyes give to fabrics used for clothing or decoration colors besides which those of the rainbow are pale neutral tints. Sanitary science arrests the massacre of the innocents, and increases the average duration of human life. Anæsthetics and antiseptics take away from surgery its pain and its peril.

But though our asssociation is an academy of arts and sciences it has, at least in its later life, devoted itself chiefly to the cultivation of pure science, leaving to other organizations the development of the

[1] Address at the Centennial Celebration of the Connecticut Academy of Arts and Sciences, October 11, 1899. Printed in Science, December 29, 1899.

applications of science. Fitly, then, our thoughts to-day dwell, not upon the vast progress of the useful arts, but upon the progress of pure science. Not the economic and the industrial, but the intellectual history of our century claims our attention.

I do not propose, in the few moments allotted to me this afternoon, to give an inventory of the important scientific discoveries of the nineteenth century. The time would not suffice therefor, even were my knowledge of the various sciences sufficiently encyclopedic to justify me in the attempt. I wish rather to call your attention to a single broad, general aspect of the intellectual history of our age. I wish to remind you in how large a degree those general ideas which make the distinction between the unscientific and the scientific view of nature have been the work of the nineteenth century.

The first of these ideas is the extension of the universe in space. The unscientific mind looks upon the celestial bodies as mere appendages to the earth, relatively of small size, and at no very great distance. The scientific mind beholds the stellar universe stretching away beyond measured distances whose numerical expression transcends all power of imagination, into immeasurable immensities.

The second of these ideas is the extension of the universe in time. To the unscientific mind the universe has no history. Since it began to exist it has existed substantially in its present condition. Among Christian peoples, until the belief was corrected by science, the Hebrew tradition of a creative week six thousand years ago was generally accepted as a historic fact. If, on the other hand, unscientific minds not possessed of any supposed revelation in regard to the date of the world's origin, thought of the universe as eternal, that eternity was still conceived as an eternity of unhistoric monotony. The scientific mind sees in the present condition of the universe the monuments of a long history of progress.

The third of these ideas is the unity of the universe. To the unscientific mind the universe is a chaos. To the scientific mind it becomes a cosmos. To the unscientific mind the processes of nature seem to be the result of forces mutually independent and often discordant. Polytheism in religion is the natural counterpart of the unscientific view of the universe. To the scientific mind the boundless complexity of the universe is dominated by a supreme unity. One system of law, intelligible, formulable, pervades the universe, through all its measureless extension in space and time. The student of science may be theist or pantheist, atheist or agnostic; polytheist he can never be.

What then, let us ask ourselves, has been the contribution of our century to the development of these three ideas which characterize the scientific view of nature; the spatial extension of the universe, the historic extension of the universe, and the unity of the universe.

The development of the idea of the extension of the universe in

space belongs mainly to earlier times than ours. The Greek geometers acquired approximately correct notions of the size of the earth and the distance of the moon. The Copernican astronomy in the sixteenth century shifted the center of the solar system from the earth to the sun, and placed in truer perspective our view of the celestial spheres. But, though astronomy, the oldest of the sisterhood of the sciences, attained a somewhat mature development centuries ago, it has in our own century thrown new light upon the subject of the vastness of the universe. The discovery of Neptune has greatly increased the area of the solar system; the measurement of the parallax of a few of the brightest and presumably the nearest of the stars has rendered far more definite our knowledge of the magnitude of the stellar universe; and telescopes of higher magnifying power than had been used before have resolved many clusters of small and distant stars.

If the development of the idea of the spatial extension of the universe belongs mainly to an earlier period, the idea of its historic extension belongs mainly to our century. It is true, indeed, that Pythagoras and others of the ancient philosophers did not fail to recognize indications of change in the surface of the earth. And, in the beginning of the Renaissance, we find Leonardo da Vinci and others insisting that the fossils discovered in excavations in the stratified rocks were proof of the former existence of a sea teeming with marine life where cultivated lands and populous cities had taken its place. Hutton's Theory of the Earth, which in an important sense marks the beginning of modern geological theorizing, appeared in the Edinburgh Philosophical Transactions in 1788, but was not published as a separate work till seven years later. Not till 1815 was published William Smith's geological map of England, the first example of systematic stratigraphic work extended over any large area of country. To the beginning of our century belong also the classical and epoch-making researches of Cuvier upon the fossil fauna of the Paris basin. By far the larger part, therefore, of the development of geologic science, with its far-reaching revelations of continental emergence and submergence, mountain growth and decay, and evolution and extinction of successive faunas and floras, belongs to the nineteenth century. Far on into our century extended the conflict with theological conservatism, in which the elder Silliman, James L. Kingsley, and others of the early members of our academy bore an honorable part, and which ended in the recognition, by the general public as well as by the select circle of scientific students, of an antiquity of the earth far transcending the limits allowed by venerable tradition.

To our century also belongs chiefly the development in astronomy of the idea of the history of the solar system. It is, indeed, true that in the conception of the nebular hypothesis Laplace, whose Théorie de la Monde was published in 1796, was preceded by Kant and Sweden-

borg; yet the credit of the discovery belongs not so much to the first conception of the idea as to its development into a thoroughly scientific theory. Our century, moreover, has added to those evidences of the nebular theory which Laplace derived from the analogies of movement in the solar system, the evidence furnished by the spectroscope, which finds in the nebulæ matter in some such condition as that from which the solar system is supposed to have been evolved.

But by far the most important contribution of this century to the intellectual life of man is the share which it has had in developing the idea of the unity of nature. The greatest step prior to this century in the development of that idea (and probably the most important single discovery in the whole history of science) was Newton's discovery of universal gravitation two hundred years ago; but the investigations of our century have revealed with a fullness not dreamed of before a threefold unity in nature—a unity of substance, a unity of force, and a unity of process.

Spectrum analysis has taught us somewhat of the chemical constitution, not only of the sun, but also of the distant stars and nebulæ; and has thus revealed a substantial identity of chemical constitution throughout the universe. Profoundly interesting from this point of view is the recent discovery in uraninite and some other minerals of the element helium, previously known only by its line in the spectrum of the sun. Profoundly interesting will be, if confirmed by further researches, the still more recent discovery of terrestrial coronium.

The doctrine of the conservation of energy formulates a unity of force in all physical processes. In this case, as in others, prophetic glimpses of the truth came to gifted minds in earlier times. Lord Bacon declared heat to be a species of motion. And Huyghens, in the seventeenth century, distinctly formulated the theory of light as an undulation, though the mighty influence of Newton maintained the emission theory in general acceptance for a century and a half.

When Lavoisier exploded the phlogiston theory and laid the foundation of modern chemical philosophy, it was seen that in every chemical change there is a complete equation of matter. But there was in the phlogiston theory a distorted representation of a truth which the chemical theory of Lavoisier and his successors ignored. They could give no account of the light and heat and electricity so generally associated with chemical transformations. These "imponderable agents," as they were called, believed to be material, yet so tenuous as to be destitute of weight, haunted like ghosts the workshop of the artisan and the laboratory of the scientist, wonderfully important in their effects, but utterly unintelligible in their nature. It was almost exactly at the beginning of our century that the researches of Rumford discovered the first words of the spell by which these ghosts were destined to be laid. When Rumford declared, in his interpreta-

tion of his experiments, "Anything which any insulated body or system of bodies can continue to furnish without limitation can not possibly be a material substance," the fate of the supposed imponderable fluid heat was sealed; but it was not till near the middle of our century that Joule completed the work of Rumford by the determination of the mechanical equivalent of heat. About the same time Foucault's measurement of the velocity of light in air and in water afforded conclusive proof of the undulatory theory of light. In these great discoveries was laid the strong foundation for the magnificent generalization of the conservation of energy—a generalization which the sagacious intuition of Mayer and Carpenter and Le Conte at once extended beyond the realm of inorganic nature to the more subtile processes of vegetable and animal life. In this connection I may be permitted to refer to the work of some of my colleagues with the Atwater-Rosa calorimeter, which has given more complete experimental proof than had previously been given of the conservation of energy in the human body.

But by far the greatest of the intellectual achievements of our age has been the development of the idea of the unity of process pervading the whole history of nature. The word which sums up in itself the expression of the most characteristic and fruitful intellectual life of our age is the word "evolution." The latter half of our century has been so dominated by that idea in all its thinking that it may well be named the Age of Evolution. We may give as the date of the beginning of the new epoch the year 1858; and the Wittenberg theses of the intellectual reformation of our time were the twin papers of Darwin and Wallace, wherein was promulgated the theory of natural selection.

And yet, of course, the idea of evolution was not new when these papers were presented to the Linnæan Society. Consciously or unconsciously, the aim of science at all times must have been to bring events that seemed isolated into a continuous development. To exclude the idea of evolution from any class of phenomena is to exclude that class of phenomena from the realm of science. In the former half of our century evolutionary conceptions of the history of inorganic nature had become pretty well established. The nebular hypothesis was obviously a theory of planetary evolution. The Lyellian geology, which took the place of the catastrophism of the last century, was the conception of evolution applied to the physical history of the earth.

Nor had there been wanting anticipations of evolution within the realm of biology. The author of that sublime Hebrew psalm of creation, preserved to us as the first chapter of Genesis, was in his way a good deal of an evolutionist. "Let the earth bring forth," "let the waters bring forth," are words that point to a process of growth rather than to a process of manufacture in the origination of living beings. In crude and vague forms the idea of evolution was held by

some of the Greek philosophers. Just at the beginning of our century Lamarck developed the idea of evolution into something like a scientific theory. Yet it is no less true that the epoch of evolution in human thought began with Darwin. Manifold suggestions there were of genetic relationships between different organisms, whether organic forms were studied by the systematist or the embryologist, the geographer or the paleontologist; but each and all found the path to any credible theory of organic evolution blocked by the stubborn fact that variations in species appeared everywhere to be limited in degree and to oscillate about a central average type instead of becoming cumulative from generation to generation. In the Darwinian principle of natural selection for the first time was suggested a force whose existence in nature could not be doubted, and whose tendency, conservative in stable environment, progressive in changing environment, would account at once for the permanence of species through long ages and for epochs of relatively rapid change. However Darwin's work may be discredited by the exaggerations of Weismannism, however it may be minified by Neo-Lamarckians, it is the theory of natural selection which has so nearly removed the barrier in the path of evolution, impassable before, as to lead, first, the scientific world, and later the world of thought in general, to a substantially unanimous belief in the derivative origin of species. Certain it is that no discovery since Newton's discovery of universal gravitation has produced so profound an effect upon the intellectual life of mankind. The tombs of Newton and Darwin lie close together in England's Valhalla, and together their names must stand as the two great epoch-making names in the history of science.

Darwin's discovery relates primarily to the origin of species by descent with modification from preexisting species. It throws no direct light upon the question of the origin of life. But analogy is a guide that we may reasonably follow in our thinking, provided only we bear in mind that she is a treacherous guide and sometimes leads astray. Conclusions that rest only on analogy must be held tentatively and not dogmatically. Yet it would be an unreasonable excess of caution that would refuse to recognize the direction in which analogy points. When we trace a continuous evolution from the nebula to the dawn of life, and again a continuous evolution from the dawn of life to the varied flora and fauna of to-day, crowned, as it is, with glory in the appearance of man himself, we can hardly fail to accept the suggestion that the transition from the lifeless to the living was itself a process of evolution. Though the supposed instances of spontaneous generation all resolve themselves into errors of experimentation, though the power of chemical synthesis, in spite of the vast progress it has made, stops far short of the complexity of protoplasm, though we must confess ourselves unable to imagine any hypothesis

for the origin of that complex apparatus which the microscope is revealing to us in the infinitesimal laboratory of the cell, are we not compelled to believe that the law of continuity has not been broken and that a process of natural transition from the lifeless to the living may yet be within reach of human discovery?

Still further: Are we content to believe that evolution began with the nebula? Are we satisfied to assume our chemical atoms as an ultimate and inexplicable fact? Herschel and Maxwell, indeed, have reasoned, from the supposed absolute likeness of atoms of any particular element, that they bear "the stamp of a manufactured article," and must therefore be supposed to have been specially created at some definite epoch of beginning. But, when we are speaking of things of which we know as little as we know of atoms, there is logically a boundless difference between saying that we know no difference between the atoms of hydrogen and saying that we know there is no difference. Is it not legitimate for us to recognize here again the direction in which analogy points, and to ask whether those fundamental units of physical nature, the atoms themselves, may not be products of evolution? Thus analogy suggests to us the question, whether there is any beginning of the series of evolutionary changes which we see stretching backward into the remote past; whether the nebulæ from which systems have been evolved were not themselves evolved; whether existing forms of matter were not evolved from other forms that we know not; whether creative Power and creative Intelligence have not been eternally immanent in an eternal universe. I can not help thinking that theology may fitly welcome such a suggestion, as relieving it from the incongruous notion of a benevolent Deity spending an eternity in solitude and idleness. The contemplation of his own attributes might seem a fitting employment for a Hindoo Brahm. It hardly fits the character of the Heavenly Father, of whom we are told that he "worketh hitherto."

In the last suggestion I have ventured outside the realm of science. But most men are not so constituted that they can carry their scientific and their philosophical and religious beliefs in compartments separated by thought-proof bulkheads. Scientific and philosophic and religious thought, in the individual and in the race, must act and react upon each other. It was, therefore, inevitable that our century of scientific progress should disturb the religious beliefs of men. When conceptions of the cosmos with which religious beliefs had been associated were rudely shattered, it was inevitable that those religious beliefs themselves should seem to be imperiled. And so, in the early years of the century, it was said, If the world is more than six thousand years old the Bible is a fraud and the Christian religion a dream. And later it was said, If physical and vital forces are correlated with each other there is no soul, no distinction of right and wrong, and no

immortality. And again it was said, If species originate by evolution, and not by special creation, there is no God. So it had been said centuries before, If the earth revolves around the sun, Christian faith must be abandoned as a superstition. But in the nineteenth century, as in the sixteenth, the scientific conclusions won their way to universal acceptance, and Christian faith survived. It showed a plasticity which enabled it to adapt itself to the changing environment. The magically inerrant Bible may be abandoned, and leave intact the faith of the church in a divine revelation. The correlation of forces acting in the human cerebrum with those of inorganic nature may be freely admitted; and yet we may hold that there are other forms of causation in the universe than physical energy, and that the inexpugnable belief of moral responsibility is more valid than the strongest induction. The "carpenter God" of the older natural theology may vanish from a universe, which we have come to regard as a growth and not a building; but there remains the immanent Intelligence

> "Whose dwelling is the light of setting suns,
> And the round ocean, and the living air,
> And the blue sky, and in the mind of man;"

the God in whom "we live and move and have our being."

The church has learned wisdom. The persecution of Galileo is not likely to be repeated, nor even the milder forms of persecution which assailed the geologists at the beginning, and the evolutionists in the middle, of our century. And science, too, has learned something. In all its wealth of discovery it recognizes more clearly than ever before the fathomless abysses of the unknown and unknowable. It stands with unsandaled feet in the presence of mysteries that transcend human thought. Religion never so tolerant. Science never so reverent. Nearer than ever before seems the time when all souls that are loyal to truth and goodness shall find fellowship in freedom of faith and in service of love.

THE GARDEN AND ITS DEVELOPMENT.[1]

By Dr. Paul Falkenberg.

[An address delivered at the Festival of February 28, 1899, by Dr. Paul Falkenberg, present rector of the University of Rostock.]

Most Worthy Assembly: When our university, in pious remembrance of the birthday of the ever-blessed Archduke Friedrich Franz II, established the 28th of February as an annual festival, the only one upon which the entire school is assembled in this place, it was done from a feeling of heartfelt thankfulness for what our well-beloved prince and chancellor had done for the university of his native State during his long and yet all too brief reign. Doubts as to the vitality of the institution, which were freely expressed, he resolutely overcame and took the most active personal interest in its welfare. Thanks are due to our illustrious reorganizer that new life now streams through every part of the corporeal frame of our alma mater, notwithstanding that she has nearly completed half a millenium of activity. Especially is this the case in the departments of medicine and of natural science, though they are not among those that can boast of the earliest origin, it being only during the present century that they obtained an independent footing. In 1810 those departments numbered together but five instructors; to-day they have twenty-three. Not contented with this, the Archduke Friedrich Franz II also established or considerably enlarged the Institute for Scientific Work for the benefit of all those minor branches of natural science that have in the course of development become independent, as well as for the branches of medicine that are ever tending toward greater specialization.

The students in the department of botany have not, however, had the advantage of the establishment of a botanical garden, or rather of the restoration of one—for at least twice during the life of our university the botanical garden has succumbed, a victim to adverse circumstances—and our school has been the only one which during the entire century up to the year 1885 has been compelled to do without a botanical establishment of its own.

[1] Translated from Der Garten und seine Entwicklung. Rostock, 1899.

This is so much the more striking because for a long time the botanical garden has been considered as evidence that botany is properly pursued in a university. The public in general esteems a science according to its practical value, and to this botany is no exception. Certainly no branch is more frequently asserted to be an object of great public interest. The botanist hears such assertions with some skepticism, for he knows that the interest of the public consists almost wholly in the pleasure felt at the sight of beautiful flowers and their use in the home and garden. Interest in botanical problems and the complicated biological phenomena with which the science busies itself is usually summed up by the public in the question, "Why do not my plants do well?" This is not astonishing, for in fact a comprehension of botany now demands more chemical and physical knowledge and insight than has hitherto been regarded as sufficient for general education. For this reason I shall, with your permission, not attempt in the short time now and here at my disposal a theme by which I might lay before you the fundamental principles of this science were I to occupy the greater portion of the day. To-day I would rather discourse to you of the generally understood, practical side of my department and relate in brief the development of the garden with reference to style and architecture. This subject is, indeed, of special interest in the history of culture, inasmuch as the ideal of the garden has varied much in different times and countries according to the artistic requirements of mankind. For the sake of convenience I here ignore the kitchen garden, on which necessity has in every age impressed the same utilitarian character.

Like all art, the horticultural art is the product of advanced culture. Even among the Greeks the appreciation of the ornamental garden was a late attainment, only reached in the time of Alexander the Great through contact with the East. Whenever Homer describes gardens, as at the court of the Phæacian king Alcinous, he praises only their fruitfulness. His age knew only the useful garden. What Sophocles later praises in the grove of Colonos is rather its romantic wildness than its artistic qualities. It is, however, especially misleading to base statements concerning the condition of gardening in early times upon the descriptions of the poets, as one can never know where truth stops and where the imagination of the poet may lead.

We reach the solid ground of direct observation at quite an early period, as the Egyptians have left us in their wall decorations many pictures of gardens. It is, however, at the beginning of our era and upon Italian ground that we first find an uninterrupted, connected development of the garden. The Romans, practical, but wanting in creative power, simultaneously imported from Greece both garden flowers and garden art, and from the time of the end of the Republic they followed the example of Lucullus and bedecked the hills on both

sides of the Tiber with the luxurious gardens and country villas of the rich. Pliny in his letters mentions many quite quaint peculiarities—for example, the clipping of trees so as to form figures of animals was already practiced—but we do not get from him a picture of the state of horticultural art as a whole. This makes all the more valuable the representation of a garden found not far from Rome, at Prima Porta, in the villa of Livia, the wife of the Emperor Augustus. The painting covers continuously all four walls of the room and places us, after the style of the modern panoramas, in the midst of the groves of a garden. The room itself is conceived as an open quadrangle surrounded by a garden scheme excellently portrayed in perspective upon the wall. The quadrangle is first surrounded by a strip of grass plot about 3 meters wide, separated from the spectator only by a golden lattice work about a foot high. At the outer edge the limit of the grass plot is marked by an open marble balustrade a meter in height. Immediately behind this rises all around a thick grove, which excludes any glimpse from without into the inclosed marble quadrangle. This grove is made up of laurels, quinces, pomegranates, cypresses, and date palms, whose green crowns are depicted against the blue sky with extraordinary truth to nature. In their shade grows a thicket of roses, poppies, and other flowers, which lean over the marble balustrade. Besides this, single low-growing plants are seen at regular distances in the grass plot: ferns, flower-de-luce, and conifers. This garden spot, entirely shut off from the world, breathes a noble simplicity such as would hardly have been expected from Pliny's description.

Judging from the absence of fantastic elements and theatrical effect, this fresco doubtless gives us an actual representation of a scene in a large park. From similar paintings we obtain from Pompeii information, belonging to the same or a somewhat earlier time, concerning what the well-to-do middle classes could effect in the way of gardens in the interior of their houses. Among the Romans the living rooms were grouped around two courts placed one behind the other, of which the anterior one, the atrium, is a Roman invention, while the second one, the peristylium, is a peculiarity of the house plan which the Romans borrowed from the Greeks. It is the peristylium that interests us in this connection, because, on account of the existence of a second court, it could be regularly transformed into a garden. This quadrate garden closely adapted itself to the plan of the house and formed its termination. Its characteristic appearance was due to the fact that it was surrounded by a colonnade. All traces of the plants grown in the Pompeiian house garden have, of course, disappeared, but we can still determine its arrangement from the walks covered with mosaic plaster. It was regularly divided by two intersecting walks into four quadrants of equal size. Upon these grew excellent rose and myrtle bushes, lilies as well as crocuses, violets and the other

flowers which fashion made indispensable for the elegant luxury of the Romans. Potted plants were also placed upon the balustrade between the pillars. To these were added many slender hermæ and small statuettes distributed between the plants. Water was usually brought in as an ornamental feature; especially popular were niches about a meter in height ornamented with stone or shell mosaic, into which the water from the Pompeiian aqueduct plashed over a few steps and was then led into a little basin in the middle of the garden. Within a space of a few paces square the little garden was able to satisfy all demands.

While the great park-like gardens of imperial Rome and their imitations early disappeared under the tread of the northern invaders, leaving no trace behind, the Roman house garden, under the protection of the church, was destined to survive all catastrophes. As the early Christian architects adopted for the ground plan of their churches the arrangement of rooms in which they held their first sacred service—namely, that of the Roman atrium and its annexes—so they at the same time adopted the peristyle garden; for the garden, surrounded by a pillared portico, which as a "paradise" almost invariably accompanied the Roman basilica where space permitted, was nothing else than the Roman house garden transferred from the narrow dimensions of a private house to the monumental style of the victorious church. Indeed it here obtained quite another function, for it was used to afford an honorable burial to those among the laity who were most faithful to the church, as, for example, even the Emperor Otto II, who in 983 was interred at Rome in the garden of the old basilica of St. Peter. In the course of time the original garden plot had to yield to a burial plot, and the church garden became a churchyard in our modern sense.

We find that the basilica garden suffered less change in the monasteries. There the crosswise form of the pathway was preserved, and only one-fourth being set aside for interments, the remainder was used as a garden for the living. Therefore, as in the old Roman house gardens, we find in these the plot divided into four rectangles by two paths whose cross-like form now had a symbolic significance, and in the middle, at the intersection of the paths, there is again found a basin of water or more frequently a fountain. Among the flowers along the cross walks there seldom failed to grow balm, basil, and all the aromatic herbs that were the materials of the so-called pharmacy of the monastery, which has spread the fame of the Benedictines and the Carthusians even to-day throughout Protestant countries.

The Roman cross-walk garden was soon brought to Germany and was systematically established by the Benedictines at Charlemagne's court, as a testimony to which we still possess an important document. A clerical person of the court sent to the [illegible] -bert, who wished

in the year 822 to rebuild the monastery of St. Gall, a large, carefully executed plan which, covering several skins of parchment, is still found at St. Gall. For all the gardens within the walls of the monastery the Roman cross-walk arrangement has been preserved, and was only discarded for the kitchen garden. With regard to this latter, the plan clearly states what plants shall be cultivated in the different beds, but this was probably done at the instance of Charlemagne, who influenced the contents of the German garden much more than the style of its arrangement. Germany has to thank him for the introduction of the most common species of fruit trees, together with walnuts, quinces, and numerous pot herbs. Of course he was not able to naturalize on this side of the Alps all the plants with which he had become acquainted in Italian gardens, but a long list of introduced plants became fully acclimated, and it speaks much for the constancy of the German peasant that these plants to-day constitute the solid foundation of his garden—the rose, the white lily, the wallflower, the poppy, rue, sage—whereby not only have plants become adapted to the German climate, but idioms have been imported into the language, as *lactuca* changed to "lattich" (lettuce), or *levisticum* to "liebestöckel" (lovage).

The succeeding age, that of the Holy Roman Empire of the German nation, was not a time for peaceful garden art, and there was wanting the necessary space for its cultivation. The townsmen crowded themselves together behind the city walls, the nobles dwelt in their isolated castles where the castle yard with its linden tree frequently represented the entire garden. Where a small garden was provided it had to be restricted to the most essential things, for even in the comparatively large castles there was but little room for horticulture. Yet even here it was sometimes possible to gratify one's private fancies. In the Höllenthal, at the foot of the lofty Meissner, for example, there is a steep rocky cone, the Bielstein, upon which there was formerly a castle. At the present time nothing can be seen of it but a few ruins, among which grow two species of plants found nowhere in Germany but on this rock and not met with again until we reach the boundary of Hungary and Moravia. Such a striking, completely localized occurrence of Hungarian plants in a distant mountain valley of Hesse can not be ascribed to chance; they must formerly have been transplanted there by the hand of man and be the last remnant, now run wild, of a castle garden long ago destroyed. Such cases of special interest in plants other than those made popular by fashion is seldom found in those rude times. In general the German love for nature had to content itself for long centuries with artless tree and grass gardens, such as the miniatures and wood cuts of the sixteenth century depict as existing under the walls of the city or at the foot of the crags on which some castle was built. As soon as the castles were

demolished the citizens ventured forth without dread from the narrow city walls, and then there was developed a real care for the unpretentious front garden.

In the meantime a new epoch of culture had begun on the farther side of the Alps, and with it a new garden style grew up, starting from the same State which in the fifteenth century gave such a remarkable impetus to the history of culture, from Florence, where Machiavelli first introduced military service for all citizens, where Niccolo da Uzzano first established the principle of the modern income tax.

Among the artists of the fourteenth century who wished to excite an interest in the art of antiquity and thereby create a new art, Leon Battista Alberti, among others, represented the theatrical side. An architect by profession, he also created the show garden which now, in more peaceful times, was attached to the palace. From the description of gardens given by Pliny, he adopted the splendid Corinthian pillar as a supporter for vines, colonnades, and artificial grottoes. But he also gave special importance to the plan of the garden, and in a way that was characteristic at once of an architect and a Southerner. Its field, which was surrounded by a thick shorn hedge, must be rectangular, round, or semicircular, or at least have such a regular contour as would constitute a good architectural plan. Then the architect created with compass and rule a perfectly symmetrical division of beds and that exact symmetry of the garden which is a necessity of life to the Italian, but which is to our taste an unspeakable weariness, especially when developed on a large scale. According to this geometrical principle there was, for example, laid out later at Rome the garden of the Quirinal palace, which is divided by straight paths crossing at right angles into some 80 equal quadrants, all surrounded by a hedge the height of a man. The uniformity of this chessboard system is slightly modified by planting, in about a dozen of these quadrants, trees which necessarily rise above the level of the hedge, but they are so kept under by the shears that they do not conceal the imposing regularity of the plan. In view of this monumental monotony it is hardly at all noticed that in certain of the quadrants miniature gardens of proportional size are planted. Here, by means of low hedges of box, were artistic scrolls of arabesques and symmetrically arranged beds in which the elegance of the design and the diversity of invention of Italian taste completely compensated for what was otherwise wanting.

In order to correctly judge this style of garden, there should be taken into account the needs of the Italians and the conditions under which it arose.

The lack of shade characteristic of this kind of garden does not generally seem a fault to an Italian, for he remains indoors until after sunset during the season when the sun is oppressive. The high hedge serves as a green decoration without interfering with the circulation

of air, as thickets and trees would do. At the same time it shields the inclosed ground from direct view and thus permits the concealment behind it of the mechanical features of gardening. As the space so inclosed could be used as a vegetable garden, it became customary not to separate the useful garden from the ornamental, and in the larger gardens the space behind the hedges was even leased out for useful purposes.

Although the German taste will sadly miss in this garden green leaves and summer flowers, this is the consequence of the rainless summer climate, not of the indifference of the Italians. In order to obtain in the smaller beds a particolored appearance which could not be effected with deciduous flowers, direct means were used by completely filling up the small box-bordered compartments with broken stones or glass slag of definite colors. In this way the effect of a modern tapestry garden was produced long before the Northern gardeners invented a similar arrangement with living plants.

While decorative summer flowers and green grassplots were completely wanting in the Italian renaissance garden, it, however, possessed instead a number of plants adapted by their form to clearly accentuate the geometrical lines of this style of garden. Myrtle and laurel afford the best imaginable material for the clipped hedges, to imitate which the yew is generally employed in the North. Slender cypresses were especially preferred to mark the corners of the regular plots, or they were used to form independent straight alleys which had the effect of colonnades. When here and there the low, flat, spreading crowns of the holm-oak were used to form overarching shady pathways, Alberti protested against it as contrary to the style of the garden, but practical use has, in the course of time, overcome all theoretical considerations. What lent to all these components of an Italian garden a special value was the fact that the plants were all evergreen, and throughout both summer and winter the geometrical outline of the garden was clearly expressed.

The garden of the Italian renaissance contained more than plants. It was at the same time a museum in which were placed for exhibition the remains of antique sculpture which the increased interest in the ancient world gradually recovered from the Italian soil. No example of the early garden of the renaissance has been preserved unchanged up to our times, but the garden of the Albani villa, at Rome, although established almost three hundred years later, gives us in its strictly horticultural part, as well as in its use of sculpture and architecture, a good idea of an early renaissance garden.

Because of the predilection for placing the garden upon a hillside, the problem was presented of building for it and its associated ornamental structures a series of stairways, often very complicated, which united the various terraces of the garden. From such a configuration

of the ground arose the idea of enlivening the garden by the use of water devices.

Not everywhere, indeed, could such an abundance of water be obtained as at Tivoli, where an arm, led off from the Teverone, rushed through the terrace-formed and supported villa of the Cardinal of Este. Where, upon such mountain declivities, springs were available, their water was so directed that behind the house it fell over a series of steps. As the precipitous character of the site made it necessary to level off against the mountain a larger area, the so-called "teatro," in order that the declivity might not confiningly press upon the house, the cascades were naturally led so as to form the middle point and termination of this area. These water courses, which were for the most part quite scanty, as we find them in the Albanian Mountains at the villas of Frascati, were the prototypes of a whole series of arrangements of cascades in the most widely scattered castle gardens. Never, however, have they produced a more imposing effect than behind the castle of Wilhelmshöhe, where, of colossal magnitude, they close in a teatro equalled by none now extant.

By the elevation of the daughters of the Medici to the royal throne of France the Italian garden obtained a ready reception and imitation in that country. This was also the case in the Netherlands, where, indeed, the appearance of the flat-laid-out garden became quite changed. Everything that could cast a shade had to be avoided under the cloudy sky of Holland. The stone balustrade was, from want of materials, replaced by thin boards without special architectural treatment. The abundance of standing water led to the laying out of long, canal-like stagnant basins, and upon the rectilinear box-bordered beds low-growing flowers were cultivated. Therefore the Dutch gardens appeared parti colored, indeed, but flat and barren, and had, as a whole, an insipid, commonplace character. With its pronounced predilection for floriculture, this style was for centuries the pattern for the stiff, ordinary suburban garden of Germany, with its straight central path and its flower borders. This form of garden, yet well known to us all, has only just disappeared because of the rapid growth of the city, which has absorbed the old gardens about the towns and changed them into suburbs.

This Dutch style became first possible at a time when there was at the command of the florist a considerable variety of plants, as the style of the garden depends in the greatest degree upon the plant material available. Until the year 1600 this was much more scanty than is generally supposed.

Very slowly did the scholastic prejudices against the study of nature disappear, and it was the renaissance that first effected in this a complete revolution. Interest in the diversity of plant forms was awak-

ened; in some the zeal for collection was excited, until such individual fancies, as often happens, came to be fashionable.

The Medici here also led the way. Though the contemporary writers of the fifteenth century boasted concerning the garden of the Careggi, near Florence—a pleasure villa still in existence—that it contained nearly all the known species of plants, we should probably not accept too implicitly the diversity of its contents. It was not until 1560 that there was collected in Europe the plant material that constitutes to-day the ordinary basis of our most modest gardens. This immigration occurred at several different periods and from several different countries.

The first and perhaps the most remarkable importation of flowering plants into European gardens occurred from 1560 to 1620, through the efforts of the Austrian monarchy. From Asia Minor and the Balkan peninsula there now came for the first time—together with lilacs, horse-chestnuts, and jasmines—carnations, and oriental bulbs, crown imperials, hyacinths, lilies, tulips, and narcissus, which, through the Hapsburgs, were also carried to the Netherlands, where a regular tulip mania ensued, and this gave the first impulse to Dutch painting, especially to that of flowers. In France, under Henry IV and Louis XIII, the fashion for the new splendid flowers became potent, so that they were used for embroidery and fabrics of silk damask. The Jardin du Roi in Paris, now the Jardin des Plantes, founded in 1626, gave its samples of these to the provincial gardens founded especially for the artistic handicrafts.

At the same time there were imported into Europe through the Spaniards the first plants of the New World, especially from Peru. Together with tobacco, the potato, and the sunflower were brought in the fig cactus and the *Agave americana*, commonly called the aloe, both of which becoming completely wild in Spain spread from thence throughout all the Mediterranean countries, and have there finally become the characteristic plants of the landscape. Somewhat later the North American plants invaded Europe *en masse*. Those that came from Canada were brought over by the French, among them the wild grape (1636). England, on her side, imported the plants of Virginia, including the magnolias of older times. As possessors of lands at the Cape of Good Hope the Dutch brought into Europe, from 1680 to 1700, pelargoniums and succulent plants; later, also, heaths—all those forms which to-day are cultivated in special houses—cape houses. The introduction of so many plants from lands having usually a much warmer climate compelled European gardens to create conditions of life suitable to the newcomers. The botanical garden at Leyden appears to have been the first to erect, in 1599, a glass house for plants as a necessary protection against the winter's cold. This, however, did

not sufficiently meet the want, and arrangements were made to warm these houses artificially. Of the Nuremberg University of Altorf it was in 1656 especially mentioned that it possessed the first heated greenhouse in Germany, and this was also, as late as 1795, described as one of the most complete in that country.

While this introduction of foreign plants, in which all seafaring nations of that time took part, completely changed the appearance of the Italian garden in its Dutch form, there developed from it in France a new specifically French style of gardening which took no notice at all of this great increase in plant material. This style, created by Lenôtre, and excellently characteristic of the whole age of Louis XIV, is an eminently architectural one, principally employing green tree masses and thus dispensing with the use of a diversity of materials. Its method was essentially the vast enlargement of the always small and decorative features of the Italian garden. The controlling motive in this change was the desire to create about the buildings that constituted an ideal center for the entire plan, a large, free, open space. To this was added the wish to have the indispensable perspective views through the garden much more extensive. Being at a greater distance from the house, a 6-foot inclosing hedge was no longer sufficient. It was made higher and higher, and, as bushes were no longer large enough for it, trees were taken, which were usually allowed to grow to their natural height. As the tops of a single file of trees seen against the sky would look threadlike, it became necessary to plant also the surrounding space with trees. The garden thus became a wood through which narrow vistas were cut where desired. Its peculiar characteristic was smoothly shorn walls, formed by clipped trees, forming long corridors or spacious compartments. By this method of treatment single plants were naturally completely subordinated, they being entirely absorbed in the work as a whole. The precise geometric pattern of the garden also disappeared from view between these high, well-shorn pieces of forest. For this reason the rectangular regularity of plan began to be disregarded, and through the garden were cut oblique vistas, which, running between high tree walls, were completely unexpected and therefore agreeable. To all these vistas a significant termination was afforded by introducing the richest sculpture, architecture, and water features, all being done, of course, with the violent extravagance of the age of the "*roi soleil*." The ornamental spring-like fountains of the Italian renaissance were now transformed into walled-in, lake-like basins from which correspondingly increased water jets were thrown aloft. In the basins and before the green walls of foliage there reveled an entire Olympus in marble, whose dimensions and impassioned bearing had to be increased to colossal proportions in order to produce effect from a distance. The green walls of foliage, at first shorn quite flat, were gradually

treated like the walls of a house, shallow niches being cut in them, or pillars in relief being carved out. At last complete windows were cut in the green wall, many even with two frames, one over the other. Trellis walls and arbors, made from artistically united laths and rods, and the rectangular ostentatious arrangement of an orangery with the plants in exactly similar tubs and with similarly clipped crowns complete the stiff magnificence of the French garden. As Louis XIV was imitated at all the courts of Europe, so everywhere were gardens planted according to the style of his pattern garden at Versailles. By the Spanish Bourbons, at Palermo; by the Hapsburgs, at Schönbrunn; by spiritual and secular princes in Germany, in Salzburg and Mainz; in Schleissheim and Herrenhausen; at the state castle at Potsdam; even up as far as Drottningholm, on the Malar Lake, everywhere arose greater or less imitations, many of which, left to themselves, have now from want of care resumed a form more in accordance with modern taste.

In spite of all the astonishment excited by the general magnificence of the French garden with its imposing perspectives, we are yet obliged to admit that it carried mannerism and artificiality to a degree never before equalled in horticultural art, so that even the very plant material revolted against it. However, mankind became gradually tired of full-bottomed wigs and longed for the idyllic peace of a primitive human state. The splendor of Louis XIV paled before the influence of Jean Jacques Rousseau—the stilted court etiquette had to yield before the so-called naturalness of shepherd poesy. This revolution of views was again most accurately reflected by the garden—the century-long rule of the French style gave way to an equally long domination of the English method.

The principle of this style, created by the landscape artist and architect, William Kent, who died in 1748, was to imitate nature—yes, indeed! the garden must be merely a bit of nature. Clipped walls of trees, straight paths, and marble-inclosed basins, terraces, and balustrades were unmercifully swept aside. Only motives from the English landscape could find place. Woods and fields, hills, ponds, and lakes constituted the entire materials of the first English garden. Vistas showing villages and castles were valued as constituents, and in order to complete the impression that the garden was a piece of wild nature it was sought to make the boundaries between it and the neighboring landscape as inconspicuous as possible. This was done by sinking ditches with steep walls and without railings, invisible a short distance away, allowing the unsuspecting glance to sweep the landscape without hindrance.

But merely to create in the garden something that was elsewhere possessed already should certainly not be the chief aim of the gardener's art, and Kent himself felt the monotony of this style and

endeavored to enliven it by placing here and there little buildings, such as small temples or hermitages.

A happy event occurred to assist these endeavors. About the middle of the last century there became known to the European world of culture the Chinese garden, which in the most bizarre manner endeavored to create as great a diversity as possible within a small space and purposely attempted to produce sharp contrasts. This Chinese fashion was suddenly adopted, and Chinese temples and pagodas (as they may be seen at Doberan upon the Camp) were introduced into the English garden, and even the entire principle of the Chinese style was accepted in order to afford to the stroller views of the utmost possible contrast. The windings of the paths and bushes, apparently growing by chance, prevented any comprehensive view, and there could thus be shown to the astonished rambler, in motley succession, wild fields, dark woods, artificial murmuring brooks, hills, and sheets of water, sometimes with melancholy shade, sometimes in full sunlight. In order to increase the effect of the landscape upon those already sentimentally inclined, structures were erected on every hand according to the fancy of the time. To the bark houses and rustic bridges of the shepherd period followed, at the time of the taste for the antique, round Grecian temples dedicated to sorrow or friendship, and broken pillars or funeral urns posted in suitable situations never failed of their touching effect upon sensitive souls. Then followed the romantic period with Gothic ruins and fallen towers, between which were placed, according to the inclination of the owner, obelisks, pyramids, and mosques. In this way the English-Chinese garden became loaded with the most contradictory decorative features until it could no longer be endured by the finer taste and historical sense of the modern age.

Since 1840 these strange structures have gradually disappeared, and yet there is no reason to dread the primitive uniformity of the English garden. The diversity which it lacked has been effectively supplied from the botanical side. As in the English garden, the specific character of trees is preserved, unnatural pruning not being used to check freedom of growth, it was thought possible to introduce into Europe the varied forms of American forest trees. The climate being similar, the prospect of success seemed especially favorable. They naturally were first adopted in England, and from there the possessors of large estates on the Continent received them. When we to-day everywhere see in parks and gardens the abundance of North American conifers, oaks, and maples, which by their foliage and autumn tints control the character of the landscape, we have to thank for this enrichment the park of Schwöbber, at Hameln, in which Otto von Münchausen, in 1750, made large sowings of the fruits of North American forest trees. Somewhat later there followed the Velheim

Park, at Harbcke, near Brunswick, and the landgrave's park of Weissenstein Castle, now known as Wilhelmshöhe. The introduction of these American forest trees was very important, as it essentially influenced the entire appearance of the garden, the introduction of so many characteristic tree forms sufficing to free modern landscape gardening from the reproach of monotony.

The modern garden, with which the name of Prince Pückler Muskau is chiefly connected, adopts many peculiarities from other styles, but rests chiefly upon an entirely new principle. The characteristic beauties and surprising combinations that nature sometimes attains with her plant materials is sought to be turned to account and artistically regulated. Forest and meadow are so distributed that they afford the most beautiful effects of light and shade. Trees are sometimes employed alone, sometimes in masses, or by skillful combinations of contrasted forms effects are produced such as nature herself presents in plant growth. This idealized landscape is not carried immediately up to the house, as was done in the original English garden, but between the house and the landscape there intervenes, according to the Italian-French principle, a piece of garden more or less regularly laid out, a so-called pleasure ground, whose size and form is regulated by the size of the dwelling. Upon this piece, according to the conditions of the case, there may be employed as a means of ornament even terraces, sculpture, and water, and here especially can all lovers of modern floriculture be gratified, here is a suitable place for arabesques of tapestry beds, and here may beautiful single plants be cultivated.

From the abundance of plant materials at hand the modern garden chooses only what is good and suitable for the place. Among the diversity of materials the standpoint of the collector of rarities whose pride is to grow as many varieties as possible is here completely disregarded. This task is assigned to the botanical garden, wherein not the beauty of the specimen, but rather its scientific interest, is of first importance.

Originally the botanical garden was intended for physicians, who could there find collected only those plants to which could rightly (or more often wrongly) be ascribed healing virtues. Therefore, the Italians, who already in the eleventh century possessed at Salerno the first medical school, founded the first botanical garden. It was also a Florentine, Angelo, a contemporary of Petrarch, who was called to Prague by the Emperor Charles IV to establish for the university, founded in the year 1348, a botanical garden, the first of its kind in Germany. Since then the aim and style of the botanical garden have changed very much, for it has become an institution in which are collected from all zones plants of scientific interest. It derives its characteristic form from the circumstance that in the course of time there

has been assigned to it the culture of plants requiring protection, this including for Europe all tropical forms without exception. Such plants could not for a long time join the immigration that flooded the Continent. The first specimens, having to endure the hardships of a long sea voyage, reached Europe only by chance. The Emperor of Austria was the first to systematically favor the introduction of tropical plants; and when in 1754 a first expedition, sent out for this purpose by the Emperor Francis I brought home its treasures, it established at one stroke the world-wide fame of Schönbrunn. The number of tropical plants in European gardens gradually increased until in 1830 there were about 1,000 species. Then the establishment of steam navigation and regular transmarine service created quite new and favorable commercial conditions, since when the number has been augmented about sixfold.

As a great part of the collection is withdrawn into greenhouses, there has arisen a considerable difficulty in suitably arranging the plants. Only exceptionally, when large areas of ground and great endowments are available, is it possible to make it at the same time an exhibition garden for the general public. It remains chiefly an institution for instruction, and for that reason can not be arranged from a strictly æsthetic point of view. Two different principles for its arrangement have been suggested; it is a pity that neither of them can be consistently and logically carried out. According to the older plan, it was sought to instruct the spectator as to the different families of plants by arranging them according to a natural system. But herbs and trees that belong to the same family can not be cultivated in the same bed; one plant will stand in moist shade while its nearest relative will have the sun and drought, not to mention the fact that one may endure the climate, while the other must be placed in the greenhouse. A systematic survey of the vegetable kingdom, which would be desirable in the garden, must therefore always be incomplete. The necessary gaps are painfully apparent to any observant, systematic student, and to complete these certain directors of gardens have had recourse to the most grotesque devices.

In the deepest shade of the hot and moist primitive forest of Sumatra there lives upon the roots of trees a parasitic plant entirely destitute of leaves, but which possesses instead the largest flower in the world, for its diameter reaches a whole meter. Since 1860 this giant child of the primitive tropical forest shades has been seen all summer long in the botanical garden at Breslau, where it stands in the open air among its relatives, and in spite of the coolness of our sun gleams from afar. It blooms even all summer long, for it is made of tin. Out of regard for the public no attempt has been made to imitate the powerful odor of this plant, which is that of rotting flesh, perceptible

for 20 meters around, but without that it is surely the strangest flower that scientific pedantry has ever reared.

Besides the systematic arrangement it has been recently attempted to arrange the plants of the botanic garden according to their geographic distribution, so that the various kinds that make up the flora of a particular region may be presented as a complete whole. If it were possible to carry out this principle, there could certainly be nothing more instructive than to thus bring together in a small space views of the vegetation of the most diverse regions as a living object lesson; but, alas! this can not be done. Differences in vegetation depend essentially upon differences in climate; that is to say, upon temperature, exposure, the movement of the air, and the special way in which these factors are distributed throughout the year. As, however, any garden is associated with only one climate, it is a hopeless undertaking to attempt to reproduce together the floras of very diverse regions. This succeeds well with single species, but if we include more, we can in Germany, for example, succeed only tolerably even with floras of climates quite similar to ours, such as those of North America and eastern Asia. But we fail when we try to grow in botanical gardens the flora of the Mediterranean countries. German gardens have indeed tried to do this, but since the official director of one of them practically admits that the Italian flora is represented only by specimens of the olive tree, the fig tree, the myrtle, and the fig cactus (that naturally can only be kept out during the summer), and by a few species of tulips and narcissuses, which at this time of the year have disappeared from view and are represented only by labels, it may indeed be said that he is self-deceived who supposes that such an inadequate representation can be of any value for purposes of instruction or æsthetics. Even when under the most favorable circumstances—that is to say, in a heated greenhouse—the public thinks that it sees a representation of tropical vegetation, the effect is very incomplete and one-sided, as the necessities of space compel the crowding of the plants into a confined, narrow, and proportionally small building, as is expressed indeed in the name "palm house." Now, in the Tropics the dicotyledonous trees constitute a much greater portion of the total vegetation than in more temperate climes, and all these characteristic forms must be rejected because they would take up too much room and keep too much light from the other plants. The European botanists therefore look with envy, and for many reasons not without justice, upon the botanical gardens of tropical countries, of which, indeed, only those at Calcutta and Buitenzorg, in Java, are scientific institutions.

As regards, however, an absolutely complete representation of the plant world, they are still worse off than we are in Europe. We can

at least, in warm houses, create for a few specimens of the tropical flora the necessary conditions of existence. In a tropical climate, however, artificially cooled houses, and consequently all plants of a cool climate, must naturally be completely out of the question, and one can rely only upon annexes placed for this purpose in the mountains. As some of those in Java have to be placed at a height of 7,000 feet in order to suffice for the needs of European plants, and can only be reached from the main garden at Buitenzorg by a journey of several days, this is a disadvantage that essentially injures their utility, so that our gardens appear in a certain sense to be more complete.

Since with us the botanical garden has to contain a great quantity of plant material that must be treated by quite diverse methods, it has hitherto been found impossible to give to its arrangement any form that would meet the demands of an artistic style. A view of any such institution will show that it is the result of a compromise between what is desirable and what is possible—a compromise between the requirements of science and the aspirations of æsthetics. The visitor must not be surprised, therefore, if many of the most beautiful ornamental plants are omitted in the botanical garden as unimportant, while inconspicuous ones are cultivated because, perhaps, some important scientific question depends upon them. Though such a collection can not rival the satisfactory and harmonious impression produced by æsthetic styles, yet we must remember that the botanical garden is in a high sense intended for use—it has to serve science in that it concerns itself with the investigation of living material, in that it gives for education the necessary materials for demonstration, and brings before students not indeed all plants, but at least selected specimens of the most important types of the vegetable kingdom. Not only for elementary instruction, but for the high school as well, it has been found that education by observation is the best education.

REVIEW OF THE EVIDENCE RELATING TO AURIFEROUS GRAVEL MAN IN CALIFORNIA.[1]

By William H. Holmes.

First Paper.

INTRODUCTORY.

During recent years much has been said and written regarding the antiquity of man in America, and as opportunity has presented I have engaged in the discussion of the subject, endeavoring to determine the exact value of the evidence brought forward by the various observers. By far the strongest body of data tending to establish the existence of a man of great antiquity is that emanating from the gold belt of California and first brought together by Prof. James D. Whitney, State geologist of California, and published in his notable work on the auriferous gravels.[2] There is considerable literature embodying original observations outside of this volume, the most important contribution being a paper by Dr. George F. Becker, published in the Bulletin of the Geological Society of America for 1891.[3]

For a long time I have entertained the idea of visiting the Pacific slope for the purpose of becoming personally acquainted with the region furnishing the evidence and with the people, so far as the hand of time has spared them, familiar with the golden era of California. I hoped at least to see enough to enable me to make up my

[1] Reprinted from American Anthropologist, January and October, 1899. The writer intended again to visit California and continue the investigation begun in 1899, but as the opportunity has not arisen, the preliminary paper is here republished with careful revision and with such new matter as happens to be at hand. An attitude apparently antagonistic to the evidence as it stood was taken, for the reason, first, that the negative side had never been systematically presented, and, second, because the possible nucleus of reliable data was evidently obscured by a mass of worthless testimony that required careful sifting. If renewed interest in the investigation of this important subject has been awakened, the main object of the original writing is accomplished.

[2] J. D. Whitney, The Auriferous Gravels of the Sierra Nevada of California, Cambridge, 1879, Vol. VI, No. 1 (1st part).

[3] Other articles are Skertchley, S. B. J., On the Occurrence of Stone Mortars in the ancient (Pliocene?) River gravels of Butte County, California. Journal Anth. Inst., May, 1888. Blake, W. P., The Pliocene Skull of California and the stone implements of Table Mountain. Journal of Geology, October and November, 1899, p. 631.

own mind as to the value of the evidence, and it seemed within the range of possibility that something decisive in the way of new evidence, or of side lights on the old, might develop—something that would open the way to a final settlement of the great questions at issue.

In September, 1898, I received instructions from the Secretary of the Smithsonian Institution to visit California for the purpose of making collections and of prosecuting anthropological investigations along such lines as might promise to be of value to the National Museum. It was arranged that the work should be conducted under the auspices of the Director of the Bureau of American Ethnology. A short time before setting out I learned that Prof. W J McGee was contemplating a trip to the Southwest a little later in the season, and I succeeded in inducing him to join me for a short time in the auriferous gravel region; I thus had the advantage of conjoint work with him in a section of superlative interest geologically, archæologically, and scenically, and one that has been made classic in science by Whitney and in song by Bret Harte.

HISTORY OF DISCOVERIES.

The auriferous, or gold-bearing, gravels, with which we are especially concerned, are scattered over a vast area in central California, extending from the high sierra on the east down the far-reaching ridges and canyons to the lowlands of the coastal belt, and from the Yuba on the north to the Merced on the south, an area equal in extent, perhaps, to that of the State of Connecticut.

The great gold discoveries began with the influx of miners in 1849 and during the two or three succeeding decades the gravel deposits were dug over to an extent without parallel in the history of mining operations. They were first attacked by pick and pan, then sluicing was introduced, and later hydraulic operations were conducted on a grand scale. Tunnel mining was also extensively carried on, and the mountains were pierced by countless shafts, sometimes so close together and so profound that it seemed almost that the mountains might collapse. This work had not continued long when reports began to be circulated, gradually reaching the ears of the outer world, that relics of man were found in these gravels, and controversies arose in which the religious press took an active part, combating the idea that traces of man could be found in formations that antedated the days of Adam, as these gravels evidently did. Mr. C. D. Voy, of Oakland, Dr. Perez Snell, of Sonora, and others collected various relics reported to have come from the gravels and secured some data relating to their origin; but the matter was never brought to a focus until Professor Whitney became interested in the discoveries and in the early sixties began with his assistants to visit the district and to collect and collate the scattered but remarkable observations.

WHITNEY'S RESEARCHES AND CONCLUSIONS.

Professor Whitney found that the gold-bearing gravel deposits were, in the main, very old; that their formation began at least in Middle Tertiary time and continued down to the end of the Pliocene period, and, in fact, in varying degree down to the present time. Examining the evidence with the utmost care, he found it impossible to avoid the conclusion that many of the relics of man and his arts came from those portions of the gravels that could with reasonable certainty be assigned to the Pliocene; that these finds were associated with the remains of extinct species of animals and plants; that they represented a race of ordinary physical characters, though having a culture of the lowest range compatible with the human status. He pointed out that a prominent feature of the evidence was its coherency; coming from a multitude of independent sources and from widely distributed localities, it all pointed in one direction. There was no suggestion of the manufacture of evidence and no apparent motive for deception. The observations were all those of miners, but a "long chain of circumstantial evidence is frequently more convincing than a single statement of an [expert] eyewitness."[1] Since Whitney's time the evidence has been strengthened by Becker, and especially by his statement that Mr. Clarence King, director of the Survey of the Fortieth Parallel, found part of a stone pestle in the firmly compacted tufaceous deposits under the lava cap of Tuolumne table mountain and removed it from the matrix with his own hands.

It is impossible not to be deeply impressed by the amount and consistent nature of the evidence presented; yet such is the magnitude of the proposition to be sustained that even this testimony seems inadequate, and we seek by reexamination and renewed research to determine its exact strength and true significance.

AGE OF THE AURIFEROUS GRAVELS.

The substantial correctness of the geologic determinations of Whitney has recently been made fully apparent by researches of the able geologists of the United States Geological Survey. It was expected by many students of the subject that the relic-bearing gravels would in time prove to be younger than Whitney believed; that they would be found to correspond in age with the Glacial period—possibly with the closing episodes of that period as determined in the Eastern States—and others were confident that they would prove to be even post-Glacial; but instead of this, Becker, Lindgrin, Turner, and Diller have extended the gravel-forming epoch to cover the Miocene and probably the greater part of the Eocene, thus making comparisons with the close of the Glacial period hardly more reasonable than the attempt to include the whole group of phenomena within the period of Biblical record.

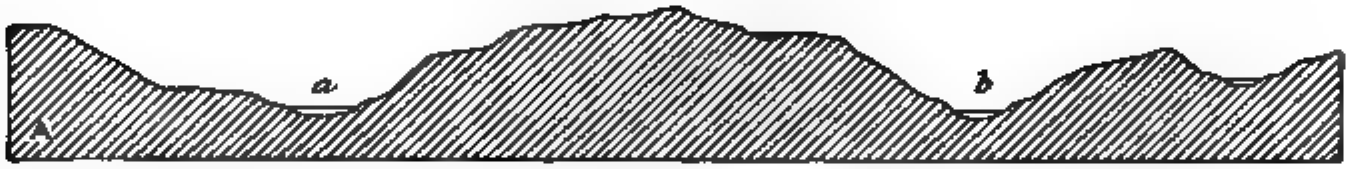
[1] Auriferous Gravels, p. 260.

To say that they were ten times or a hundred times older than the Glacial period, as represented by the greatest extension of the ice in Ohio and Delaware valleys, would probably not be doing justice to a lapse of time that can be expressed only in several geologic periods.

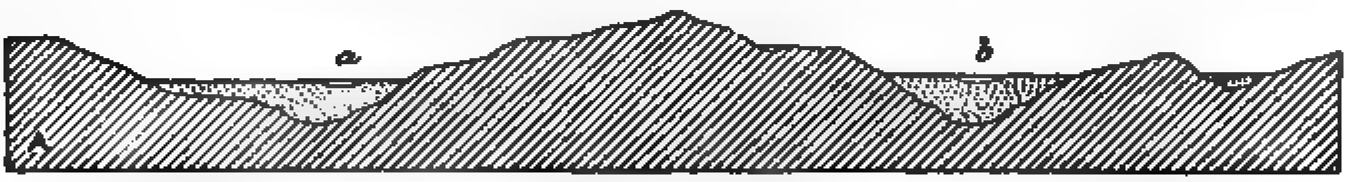
As many readers may not be familiar with the geologic relations of the auriferous gravels, and hence find themselves unable to form definite notions of the great lapse of time and the vast transformations of nature with which we have to deal, it may be well to present briefly the main features of the later geologic history of the region. The accompanying sections, with appended data, will serve to tell the story so fully that a few words only will be necessary to make it understood. In early Tertiary times the prototypes of the modern rivers ran out from the sierra and down through the foothills to the sea pretty much as they do to-day. The valleys were not so deep as now, as indicated in 1 and 4, Plate I, but the streams had strong currents and rapidly scored down the gold-bearing formations which they traversed, filling their channels with coarse, waterworn débris to the depth of hundreds of feet and depositing the freed gold along their beds. This second phase of progress is indicated in 2, Plate I. It is from these gravels that some of the finds of human relics are reported, and it is therefore affirmed that along the banks of these ancient rivers the first human beings of which science has a trace lived and pursued their varied avocations.

But there came over this region a momentous change. A period of great volcanic activity set in, and streams of lava and rivers of mud descended from the sierra, filling up the valleys; new channels were eroded, to be filled in their turn, one system of drainage succeeding another for a prolonged period, at the close of which the deepest valleys were filled to the brim with the deposits, as shown in 3, Plate I; and when the flows of basalt—the final products of vulcanism—ceased, the waters of the high sierra began the work of laying out the drainage system that has come down to the present time.

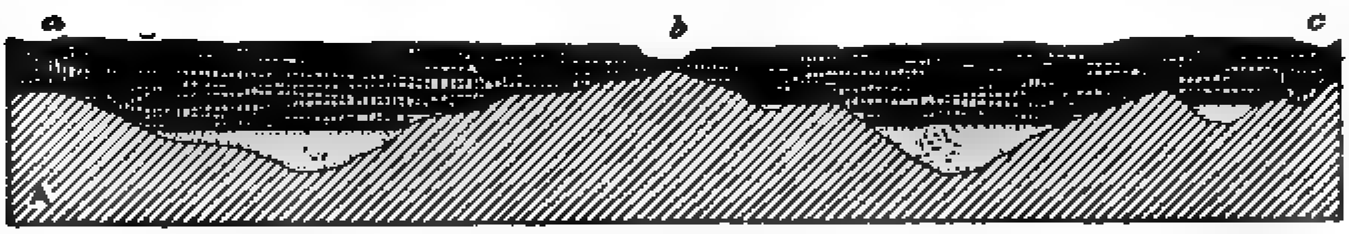
Since that remote day the region has been elevated to greater heights; the Merced, the Stanislaus, the Tuolumne, the American, the Yuba, and other streams have cut their channels by the slow processes of erosion down to profound depths and now run their courses in valleys 2,000 feet deep and many miles in width—gorges so profound, precipitous, and vast that it is a day's journey to cross them even where the hand of the enterprising gold hunter has ventured to blaze the tedious way. The striking character of the present profile is shown in 4, Plate I, by reference to which it may be seen that the cutting of the present valleys to such great depths has left the old stream beds with their deposits of gravel, their treasures of gold, and (it is alleged) their relics of humanity high up toward the mountain summits (*e*). In these elevated districts the miners seek and find the gravel outcrops and follow them far into and even through the ridges, the meanderings



1.—Section showing conditions in early Tertiary times. *A*, Auriferous slates; *a*, *b*, stream beds.



2.—Conditions at close of great gravel-forming period and before volcanic activity began. *A*, Auriferous slates; *a*, *b*, river beds clogged with auriferous gravels.



3.—Conditions at close of volcanic period; valleys filled up and mountains buried with eruptive formations. *A*, Auriferous slates; *a*, *b*, *c*, river channels.

4.—Conditions to-day. Rivers in channels 2,000 feet deep. Remnants of volcanic deposits and old river gravels capping mountain summits, *c*, *c*. Old profiles shown by dotted lines.

TRANSVERSE SECTIONS OF RIVER CHANNELS, AURIFEROUS GRAVEL REGION, REPRESENTING THE PROFILES AS THEY ARE SUPPOSED TO HAVE APPEARED AT FOUR WIDELY SEPARATED PERIODS.

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se who get little idea of the lapse of time not expressed in years 'ail to comprehend what vast ages are suggested to the geologist terms Eocene, Miocene, Pliocene, Pleistocene, and Recent, but agnitude of the events involved—the entire obliteration of the opography and the carving out of a new California, including gorges as the Yosemite and the still more sublime Hetch-Hetchy— readily be appreciated, and must make a deep impression on every l and lead to hesitation in accepting the propositions that man ured before these events were initiated and that he has witnessed survived their consummation. (See Plate XVI.)

CATEGORIES OF GRAVEL FINDS.

Iaving reached satisfactory and apparently final conclusions respect- g the age of the auriferous gravels themselves, it is in order to amine the various groups of associated phenomena with which cheologists must concern themselves. There are four categories data to be considered.

A. The animal remains (lower orders).
B. The plant remains.
C. The remains of man.
D. The remains of human handiwork.

A. The animal remains found in the gravels in fossil state represent a large number of species, chiefly mammals, identified by Dr. Joseph Leidy. Whitney enumerates the following forms: Mastodon, elephant, rhinoceros, horse, camel, tapir, ox, llama, deer, wolf, and dog. These are all of extinct species, and although some may have existed down to Post-Pliocene time, as indicated by Dr. Becker,[1] they fall as a group naturally within the Neocene (Miocene-Pliocene) age.

B. The fossil plants of the gravels secured in Whitney's time were studied by Dr. Leo Lesquereux, and by this eminent authority were called Pliocene, although he found many forms that could with equal justice be assigned to the Miocene. Extensive collections obtained in more recent years have been identified by Ward and Knowlton, and it is agreed that on the whole they represent early rather than late Neocene forms; that they are clearly of Middle Tertiary age. According to Professor Knowlton, there is not one species which can undoubtedly be identified with living forms.[2]

C. Human remains reported from the gravels are not plentiful, and all that appear to have been preserved are an imperfect human cranium,

[1] George F. Becker, Antiquities from under Tuolumne Table Mountain in California. Bull. Geol. Soc. of America, Vol. II, p. 189.

[2] Lindgren and Knowlton, Age of the auriferous gravels. Journal of Geology, Vol. IV, No. 8, p. 905.

known as the "Calaveras skull," and a few unimportant fragments of another skull. Fragments of skulls and various bones of the body have been reported from the old gravels in a number of localities. These remains, and especially the Calaveras skull, indicate a man not differing materially from the California Indian of to-day, although said by Whitney to present some characteristics of the Eskimo.

D. The remains of human handiwork to be considered are, on the other hand, quite numerous. Many hundreds of specimens have been reported from the gravels, and are believed, in a general way, to belong to the Neocene deposits. According to the finders, many of them were intimately associated with the remains of fossil animals and plants, and some appear to be from gravels that antedate the volcanic era.

INCONGRUITIES IN THE EVIDENCE.

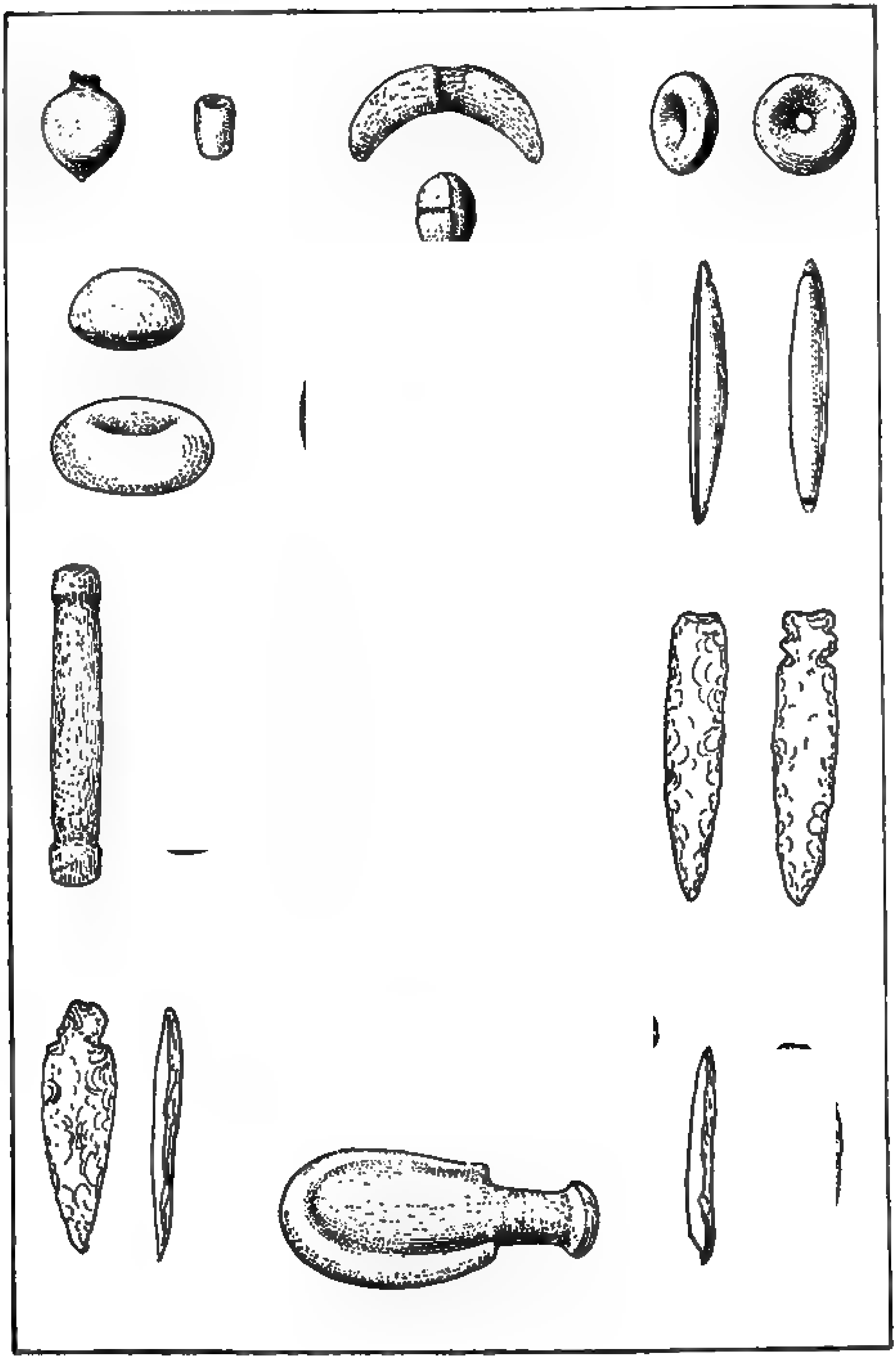
In comparing these four groups of remains we observe that the fossil animals belong without exception to extinct species, that the plants are likewise extinct, and that all of both groups take their place naturally within the limits of the Neocene. When, however, we examine the human remains, we are met by the striking fact that they do not represent an extinct form, or even a well-marked variety of *Homo sapiens*, but a people practically identical with ourselves; and it therefore takes a great stretch of the imagination to conceive that this man could have formed part of a fauna every other mammalian member of which has succumbed during the uncounted centuries of succeeding geologic periods.

On examining the art remains it is found that they also seem out of place in Tertiary times, that they present a decidedly modern aspect. Of the fifteen or twenty varieties reported from the gravels by Whitney and others, all appear to be of recent types. They are practically identical with the stone implements used by the native tribes of California to-day or in the recent past. If these forms are really of Tertiary origin, we have here one of the greatest marvels yet encountered by science; and perhaps if Professor Whitney had fully appreciated the story of human evolution as it is understood to-day, he would have hesitated to announce the conclusions formulated, notwithstanding the imposing array of testimony with which he was confronted. To suppose that man could have remained unchanged physically; to suppose that he could have remained unchanged mentally, socially, industrially, and esthetically for a million years, roughly speaking (and all of this is implied by the evidence furnished), seems, in the present state of our knowledge, hardly less than admitting a miracle.

Professor Whitney believed the implements found were just such as might be expected of a Tertiary man, and observes:

"It has been always the same kind of implements which have been exhibited to us, namely, the coarsest and least finished which one would suppose could be made and still be implements."[1]

[1] Auriferous gravels, p. 279.



GROUP OF IMPLEMENTS AND UTENSILS SAID TO HAVE BEEN DERIVED FROM THE AURIFEROUS GRAVELS.

The figures included in the plate are copied from hasty sketches and do not assume to be exact. They serve, however, to indicate the character and range of the finds. Photographic reproductions of some of the objects are given in succeeding plates.

But on examination we find that they are not of advanced types, and comprise the following varieties: Mortars, pestles, dishes, platters, ladles, metates, mullers, rubbing stones, hammer stones, mauls, perforated stones, rings of stone, pitted stones, spindle-shaped stones, grooved pebbles, hammer heads, sinkers, crescent stones, spearheads, arrowheads, knives, and bone implements, to which may be added shell beads and wampum.

The series of specimens presented in Pl. II will serve to indicate the wide range of form covered by these objects, and the photographic illustrations given in Pls. III to XI will convey a better idea of the character of some of them. The substantial identity of these implements with the familiar relics of our Californian tribes is thus made apparent. The assumption that man shaped and used this group of art works in Tertiary times and continued to use them without change, without improvement or retrogression, down through the ages, through complete transformations of land and sea and the extinction of all known living things, should be supported by proof more conclusive than anything yet adduced.

Again, the suggestion that the ancient people disappeared as a result of nature's mutations, leaving their bones and handiwork in the stream beds of the Neocene period, and that another people, springing up or appearing on the same spot in recent years, has duplicated each and every character, activity, and art form, is hardly to be entertained.

Another consideration is interesting in this connection. Should we feel compelled to concede the existence of a race of advanced stone-age culture, such as that suggested by the group of artifacts preserved, it would necessitate the further concession that the origin of the race is to be looked for in a still earlier period, for the best experience of anthropologists goes to show that early steps in culture are hesitating and slow, that the various stages which, in the normal order of cultured progress, precede the era of polished stone, must have been of very great length: and should we adopt the conclusion of Whitney that no considerable advance in culture took place in California between Tertiary times and the present, and take this as a reasonable index of the rate of progress, we should have to look for the cradle of the race somewhere in the remote ages of the Mesozoic.

It may further be noted that the biologist, accustomed to regard animate nature from the point of view of the theory of evolution, will find it difficult to accept conclusions that would place the perfected man, the highest type of the highest class of animal life, the mammalia, too near the beginning of a series that ought in the natural order of things to show definite indications of progressive change.

EXAMINATION OF THE IMPLEMENTS PRESERVED.

Turning now to the objects of art described by Whitney and others and preserved in the museum of the University of California and else-

where (Pls. III to XI), we inquire more fully into their character and appearance. Whitney has said that the gravels were deposited by streams having violent currents, that the bones of animals were torn asunder and scattered, and that all objects were necessarily more or less worn; but it is observed that not one of the art objects attributed to the gravels shows the least sign of rough usage or wear; the marks they display of the tools employed in their manufacture or of the implements associated with them in use are as fresh as in the implements and utensils found on modern Indian sites. This fact is so significant that it can not be passed over without reasonable consideration.

Glancing again at the numerous implements, utensils, and ornaments attributed to the auriferous gravels, we may inquire, What materials are represented? There are several varieties of stone, including granite, andesite, rhyolite, slate, obsidian, etc. Andesite, however, prevails, and at least one-half of the objects are of this material. As most of these rocks in their original distribution are confined to somewhat limited portions of the geological column, some early and others late, it is proposed to inquire whether any of the specimens are of materials later in origin than the strata in which they are said to have been found. Full data are not yet at hand for a satisfactory discussion of this point; but it may be mentioned that andesite specimens are reported from horizons extending all the way from the earliest to the most recent gravels, yet so far as our geologists have gone this rock is not found in the formations of the particular region until toward the latter half of the Neocene. The objects being generally large, it is not to be supposed for a moment that they could have been brought from a distance. Again, obsidian is known only as a late product, having its origin in the most recent flows of the Sierra, yet we have obsidian implements reported from the gravels of various districts, and in one case, at least, from deposits that must belong very near the initial stages of eruptive activity. This interesting line of research remains to be followed up until definite results are reached; this, however, can not be profitably done until the geology of the region is more exhaustively studied.

The various objects attributed to the gravels by Voy and others and now preserved in the museum of the University of California were examined at my request by Mr. F. L. Ransome, of the United States Geological Survey, for the purpose of determining the material. The result is, of course, only tentative, but Mr. Ransome is thoroughly familiar with the formations of the auriferous region, and his determinations are as satisfactory as can be made without cutting the specimens and making slides for microscopic examination. The list furnished is as follows:

1. Lobed mortar. Fine-grained mica hornblende diorite; probably dyke.
2. Mortar and pestle. Mortar, diorite. A common peripheral facies of granodiorite when occurring in intrusive masses in foothills of the Sierra. Pestle is not so clear, but is apparently a fine-grained dioritic dyke rock.

3. Mortar. Appears to be a fine-grained dioritic dyke rock, but is too dirty and stained to be satisfactorily examined.
4. Pestle. Fine-grained rock, apparently a crystalline schist.
4. Mortar. Gray hornblende andesite, a common facies in Neocene breccias.
5. Mortar. Fine-grained hornblende andesite.
6. Mortar. Apparently a hornblende andesite, though of somewhat unusual type. Porphyritic hornblende is not conspicuous. Apparently phenocrysts of plagioclase and hornblende in gray glassy base.
7. Cylindrical mortar. Hornblende andesite.
9. Mortar and pestle. Mortar is pinkish hornblende andesite. Pestle is amphibolite schist. Shows fibrous structure and apparently remnants of augite.
10. Mortar. Diorite porphyry. Probably dyke rock.
12. Mortar. Pinkish hornblende andesite.
13. Dish or mealing stone. Gray hornblende andesite.
14. Dish or mealing stone. Appears to be fine-grained syenite, but possibly a diorite. Almost certainly a dyke rock.
15. Dish or mealing stone. A fine-grained pinkish rock containing talc (or some equally soft mineral) and a silvery mica.
16. Mortar and pestle. Mortar, hornblende andesite. Pestle, a porphyritic rock, species not recognizable. Not an andesite.
16. Boat-shaped stone. Compact banded rock, apparently from metamorphic Calaveras formation.
16a. Boat-shaped stone. Amphibolite schist.
16b. Boat-shaped stone. Compact rock of doubtful nature.
17. Large bead. Rusted hornblende andesite (?).
18. Mortar. Hornblende andesite (?). Badly weathered.
19. Crescent-shaped stone. Very fine grained. Apparently an altered dyke rock.
20 Cylindrical mortar, 9 inches high. Rather soft talcose rock; probably altered dyke rock.
20a. Small mortar. Diorite with segregation patch. Dyke, or periphery of granodiorite mass in foothills.
21. Mortar. Diorite porphyry. May come from periphery of granodiorite mass or from a dyke. Contains a dark segregation patch.
21a. Grooved pebble. Fine-grained gray hornblende andesite.
21b. Grooved pebble. Fine-grained gray hornblende diorite. Dyke rock.
21c. Grooved pebble. Fine-grained diorite. Dyke rock.
23. Mortar. Gray hornblende andesite.
—. Round stone (found in 1863 at Gold Springs Gulch, Tuolumne County, in auriferous gravel of Pliocene age). Fine-grained diorite. Dyke rock.

Pls. III to XI are devoted to the illustration of a number of these objects. The photographs used, with one exception, were lent for the purpose by Dr. R. E. C. Stearns, of Los Angeles, Cal., the original Voy collection numbers being given, as also in the preceding list. The objects referred to in the above list form but a small fraction of the multitude of relics reported from the gravels.

In presenting these objects the original statements that they were found in definite relations with Tertiary strata and Tertiary mammalian remains are allowed to stand, but it should be understood that the view of the problems involved taken in this paper requires that such statements should as yet be followed by an interrogation. This interrogation does not raise a question as to the veracity of the finders, but serves to express the fear that in some way errors of observation or record have been made.

EXPLANATION OF PLATE III.

Dish or mealing plate found in 1862, associated with other stone relics and with fossil bones of the mastodon and other extinct mammals, in the auriferous gravels of Gold Springs Gulch, Tuolumne County, Cal. It is said to have been buried beneath 20 feet of calcareous tufa, but its exact relation with the associated gravels is not recorded. Longitudinal diameter, 18½ inches; transverse diameter, 13 inches; full depth, 3½ inches; depth of basin, about 2 inches. Inner surface well polished from use, and margins and under side worked and worn moderately smooth. Material, gray hornblende andesite. Referred to by Whitney in Auriferous Gravels, page 263. Two similar specimens are reported from gravel deposits near Georgetown, Placer County. This specimen is identical in every way with the mealing platters of the California tribes of to-day. No. 13, Voy collection.

MEALING PLATE FROM GRAVELS IN GOLD SPRING GULCH (ONE-THIRD ACTUAL SIZE).

EXPLANATION OF PLATE IV.

Globular mortar, with cylindrical pestle, found in 1861, with other stone relics and the bones of fossil mammals in auriferous gravel, about 16 feet beneath the surface at Kincaid Flat, Tuolumne County, Cal. The shape of the mortar is symmetrical, and the surface well smoothed by pecking and use. Diameter, 10 inches; height, 7½ inches; depth of basin, 5½ inches. Material, pinkish hornblende andesite. Referred to by Whitney, p. 263. No. 9, Voy collection.

The pestle has enlargements at both ends and is not quite symmetrical in shape, and the surface shows the marks of the shaping tool quite distinctly, save at the ends, which are worn by use. Length, 11 inches; diameter of middle portion, 1½ inches; of larger end, 2 inches.

GLOBULAR MORTAR AND CYLINDRICAL PESTLE FROM GRAVELS AT KINCAID FLAT
(ABOUT ONE-THIRD ACTUAL SIZE).

EXPLANATION OF PLATE V.

Globular mortar, ornamented with incised markings, forming a rude reticulated design on the exterior surface. Found in 1863, with other stone relics and associated with mammalian remains in auriferous gravel, about 16 feet below the surface, in Gold Spring Gulch, Tuolumne County, Cal. Diameter, 12 inches; height, 9½ inches; depth of basin, 7 inches. Material, pinkish hornblende andesite. Referred to by Whitney, p. 263. This type of mortar is in use to-day and the incised reticulate decoration is occasionally seen. The pestle is of the simple cylindrical form usual in California. No. 12, Voy collection.

GLOBULAR MORTAR AND PESTLE FROM GRAVELS IN GOLD SPRING GULCH (ONE-HALF ACTUAL SIZE).

EXPLANATION OF PLATE VI.

Cylindrical mortar, found in 1861, with other relics of stone, embedded in auriferous gravel about 10 feet beneath the surface, 3 miles northeast of Shingle Springs, Eldorado County, Cal. Height, 9 inches; diameter near top, 7 inches; at base, about 5½ inches. Conical pit 4½ inches deep. The material is a rather soft talcose rock, probably altered dyke material. The outlines are somewhat irregular, but the surfaces are all artificial. Referred to by Whitney, p. 265. This type of mortar is not unusual in central California, but is apparently not often found in use by the present tribes. No. 20. Voy collection.

CYLINDRICAL MORTAR FROM GRAVELS AT SHINGLE SPRINGS (ONE-HALF ACTUAL SIZE)

EXPLANATION OF PLATE VII.

Mortar of oblong shape and with deep circular pit. Said to have been found in 1862 in auriferous gravel beneath 14 (or 140) feet of basalt and 200 feet in from the surface of the slope near the Boston Tunnel Company's mine, Table Mountain, Tuolumne County, Cal. Circumference, 25 inches. Material, hornblende andesite of somewhat unusual type. Shape only partially artificial and not peculiar to any region. No. 6, Voy collection.

OBLONG MORTAR ATTRIBUTED TO GRAVELS UNDER LAVA CAP OF TABLE MOUNTAIN (ONE-HALF (?) ACTUAL SIZE).

EXPLANATION OF PLATE VIII.

Scoop-shaped utensil of gray diorite, found in 1864, in auriferous gravel, 16 feet below the surface, near Oregon Bar, North Fork American River, Placer County, Cal. The shape is unusual, although many specimens of somewhat similar character have been found in California. Length, 11¼ inches; width, 6¼ inches. This specimen is now in the Museum of Science and Art, Philadelphia, and was kindly forwarded for examination to the National Museum by Dr. Stewart Culin.

SCOOP-SHAPED UTENSIL FROM GRAVELS AT OREGON BAR (ONE-HALF ACTUAL SIZE).

EXPLANATION OF PLATE IX.

Grooved pebbles, probably used as hammers or as club heads. Found with other stone relics, in auriferous gravel, 10 feet beneath the surface, on Indian Gulch, near Spanish Flat, Eldorado County, Cal. Objects of this general character have a wide range geographically and served various purposes in the economy of savage and barbarian life. The upper specimen is of hornblende andesite and the lower of fine-grained diorite. Referred to by Whitney, p. 276. No. 21, a, b, Voy collection.

GROOVED PEBBLES FROM GRAVELS AT INDIAN GULCH (ACTUAL SIZE)

EXPLANATION OF PLATE X.

Three objects, usually referred to as charm stones, found in 1864, with other stone relics, in auriferous gravel, 10 feet below the surface, in Indian Gulch, Eldorado County, Cal. They are described as "boat-shaped," and have notches or grooves at the ends for convenience in attaching cords. The longest one is symmetrical in shape and well finished and is made of greenish-gray slate. The short specimen is similar in character and material, while the remaining piece is much ruder and is made of amphibolite schist. Referred to by Whitney, p. 276. No. 16, 16a, 16b, Voy collection.

These forms, as well as numerous variants, are found in large numbers in California.[1]

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[1] Yates, Lorenzo G. Charm stones. Bulletin No. 2, Santa Barbara Society of Natural History, pp. 13–28. Five plates.

BOAT-SHAPED OBJECTS FROM GRAVELS IN INDIAN GULCH (ACTUAL SIZE

EXPLANATION OF PLATE XI.

Obsidian blades, one specialized as a knife or lance head, and the other in the rough. The first was found in 1869, with other relics and with mastodon remains, in auriferous gravel, 10 feet below the surface, at Horse Shoe Bend, Merced River, Mariposa County, Cal. The second was obtained in 1863, with other relics, from auriferous gravel, 10 feet below the surface, near Princeton, Mariposa County, Cal. Referred to by Whitney, p. 261. A similar specimen came from the gravels in Fresno County, Cal. Nos. 23 and 24, Voy collection.

These specimens present no features at variance with the ordinary obsidian implements of California. Engraved actual size.

OBSIDIAN BLADES FROM GRAVELS AT HORSE SHOE BEND, MERCED RIVER (ACTUAL SIZE).

were repeated at nearly every mine visited in Nevada, Placer, Eldorado, and Calaveras counties. At Forest Hill, Placer County, the Dardanelles mine, extensively worked in the early days by Richard Clark and others, has undermined and obliterated a half or more of a terraced spur or "flat," as such features are called in that country, formerly occupied by an Indian village. (See plate XII.) According to Mr. Clark, who still resides in Forest Hill, this site has not been occupied by the natives since work began in the mine in 1852, but an hour's search brought to light a dozen mortars and grinding stones, twenty or thirty rubbing stones and pestles, together with several varieties of smaller tools. As the ground of the site sloped toward the mine, most of the larger and especially the rounder objects must long since have rolled into the great pit (fig. 1), the gravel walls of which are on the one side upward

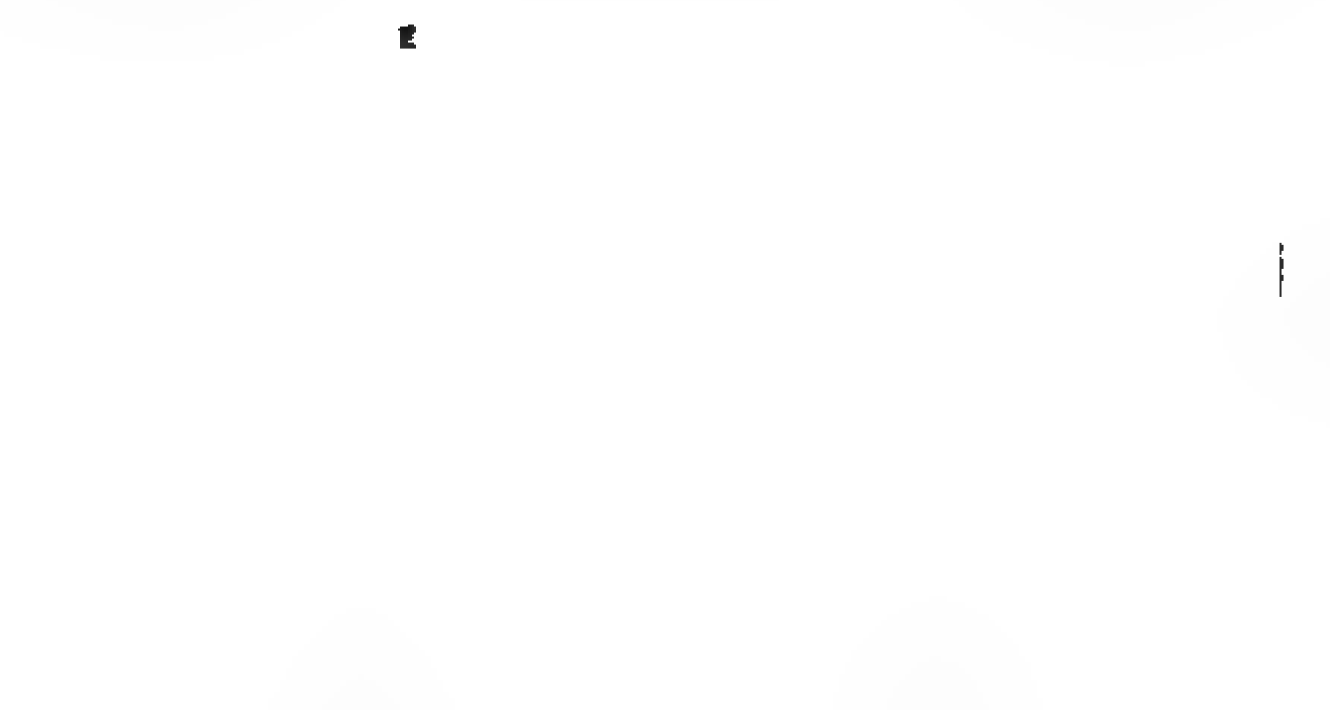


FIG. 1.—Section showing relations of ancient village site to gravel mine.

A, Auriferous slates—bed rock. *B*, auriferous gravels, 250 feet thick; *C*, great excavation made in gravels by hydraulic mining. *D*, crumbled gravels, result of caving in; *E*, ancient village site; *F*, portion of village site destroyed by mine. The dark triangular figures in the talus show the distribution of artifacts resulting from mining operations.

of 200 feet in height. Many of the objects obtained by me were already in the gullies leading down to the mine, and in the preceding half century large numbers must have gone over to become intermingled with the gravels, where they would remain for good, unless some observant miner happened to bring them to light. Specimens thus found, falling into the hands of such collectors as C. D. Voy, would naturally be added to the growing list of Tertiary gravel relics. The flat dish or platter found by Voy in this or a neighboring mine[1] is identical in type with several of the specimens from the village site on the brink of the mine. A rough, roundish mortar and a small handstone were found by Professor McGee on a ledge 30 feet below

[1] Auriferous gravels, p. 277.

1.—Weathered gravel wall of mine 200 feet in height, with ancient village site above.

2.—Margin of mine, showing ancient village site on the hill beyond.

VIEWS IN DARDANELLES MINE SHOWING POSITION OF ANCIENT VILLAGE SITE.

the brink of this mine, where they had fallen from above, and at Todds Valley, a few miles farther southward, a roundish bowlder some 3 feet in diameter, having a neatly shaped mortar in one side of it, was found resting on the bed rock of a deep mine. This specimen also had undoubtedly fallen in from above. An Indian dwelling was situated on the rim of the mine near by, and about it were scattered mortars of all kinds. A brush shelter in which the women grind acorns, a little higher up than the dwelling, contained a fixed mortar with numerous pits and at least a dozen pestles, both flattish and cylindrical in shape.

These significant relationships of Indian village sites and gravel diggings were repeated everywhere, and although Whitney observed the presence of the "Diggers," he made the mistake of supposing they used only fixed mortars, that is, those worked in the surface of large masses or outcrops of rock. The fact is that portable mortars and grinding stones of diversified forms are and have been used by Indians in all parts of California. It is not to be supposed that miners would pay much attention to the origin of relics found by them in the mines, since they attached no particular significance to them; so that between the unwary geologist, the unthinking miner, and the professional collector cultivating a prolific field, it is to be expected that many mistakes would be made.

No one can venture to say just what percentage of the finds reported by Whitney and accepted by him as evidence of antiquity are of the class here described, but certainly a large proportion may be assumed; and the observations made above cast a shadow of doubt over all specimens corresponding to known Indian forms reported from open mines, from such shafts and tunnels as do not extend beneath undisturbed formations, or from positions where any kind of post-Tertiary disturbance could have taken place.

In a second paper I hope to review the evidence further, and especially to present some data relating to the Calaveras skull.

Second Paper.

INTRODUCTION.

The main features of the problem of auriferous gravel man in California stand out in bold relief. On the one hand the evidence is interpreted as establishing the existence of a Tertiary man of high type physically and mentally, equal or superior to the Indian tribes of the region to-day, and occupying a culture plane corresponding to the polished-stone age of Europe. It is assumed that this remotely ancient man continued to live and thrive without perceptible advance or retrogression while nature passed through a thousand centuries of revolution; or that, as an alternative proposition, if the Tertiary race did not

persist, but disappeared along with the other mammalian fauna of the time, a new race sprang up, duplicating the physical characters and culture of a former geologic period. There are those high in the councils of anthropologic and geologic science who profess to see no reason for rejecting these bold and extraordinary propositions. On the other hand, there are those who hold that the facts adduced do not warrant either of these conclusions, who see in the whole body of observations and assumptions only a mass of errors and misinterpretations. Thus for a number of years the opposing views have stood without apparent change, the proofs, though strong, not being sufficiently decisive to carry full conviction with regard to a proposition of such exceptional magnitude. It is probable that without positive reenforcement the evidence would gradually lose its hold and disappear; but science can not afford to await this tedious process of selection, and some attempt to hasten a decision is demanded. If new evidence can not be found, renewed discussion will at least develop the full strength or weakness of the old, and it is especially desirable to take this matter up while some of the pioneers of the Sierra Nevada are still with us.

It has been shown in the preceding pages that much of the testimony furnished by Whitney is not well considered, and that there is excellent reason for questioning or rejecting most of the observations placed on record regarding the deep finds. The mines of the more northern counties, already referred to in some detail, seem to have furnished nothing that can be relied upon to prove anything more than the presence of the Digger tribes or their immediate predecessors in the region, and it remains now to look critically into the evidence furnished by the vast diggings of the south, and especially in the great valleys of the Tuolumne and the Stanislaus.

TABLE MOUNTAIN REGION.

The region of Table Mountain, in Tuolumne and Calaveras counties, has yielded a large part of the testimony most relied on to support the theory of an auriferous gravel man. Here finds have been reported in bewildering numbers, the objects coming from many sources, often apparently wholly independent of one another. During my visit to this region I sought to get back as near as possible to original sources of information, to see the people having personal knowledge of the finds, and to acquire a correct notion of the aboriginal occupancy before, during, and since the great period of mining activity.

Indian implements in mines.—Accompanied by Prof. W J McGee, I journeyed from Jamestown, the railway terminus, situated under the eastern escarpment of Table Mountain, to Sonora, Sawmill Flat, Yankee Hill, Columbia, Springfield, and Shaws Flat. I crossed over and passed around Table Mountain, visiting Rawhide and Tuttletown,

and, traversing the great gorge of the Stanislaus, spent several days in the vicinity of Murphys, Altaville, and Angels Camp. These places were all centers of great activity in the early days of gold mining, as amply attested by vast excavations covering many square miles of territory, and I was told by those who had seen it that the Indians flocked in from the surrounding mountains to such an extent that it was not unusual to see the lodges of a thousand Diggers gathered about a single camp; and the hills and valleys still bear ample evidence of their presence. Numberless pits and trenches were then gaping to receive the scattered utensils of these people, whose village sites, one after another, were undermined and destroyed, and collectors reaped a goodly harvest of supposed ancient relics from the mines. The Snell collection, referred to by Whitney and culled from by Voy, was gathered from this locality and consisted of the usual stone implements and utensils of the Indian tribes, as well as of several forms not in common use to-day and thought by some to especially represent the ancient time. A remnant of this collection is now owned by Mr. J. W. Pownall, of Columbia, and will probably pass eventually into the keeping of the University of California. Through the generosity of Mr. Pownall three specimens were obtained for the National Museum.

As indicated in the preceding paragraph, a thorough knowledge of the aboriginal occupancy is of vital importance in this discussion, but Whitney knew little of the native culture, as his remarks amply show, and he could not have separated objects that had fallen in or had been introduced by other means into the mines from like objects originally belonging in the gravel, if such there were. Neither Whitney nor Voy, so far as I can learn, had any idea of the need and vital importance of such discrimination. Their lists of finds from the mines are hardly more than lists of Indian implements.

Implements from deep tunnels.—But what is to be said of the finds reported from the deep shafts and tunnels that penetrate obliquely or horizontally beneath the lava-capped summits of Table Mountain? (See fig. 2.) Relics of the swarming Diggers could not fall in horizontally, and if these relics do not belong with the fossil animals and plants in the gravels of the ancient river channels, we are left to determine how they could have been introduced, or how deception was so successfully and generally practiced.

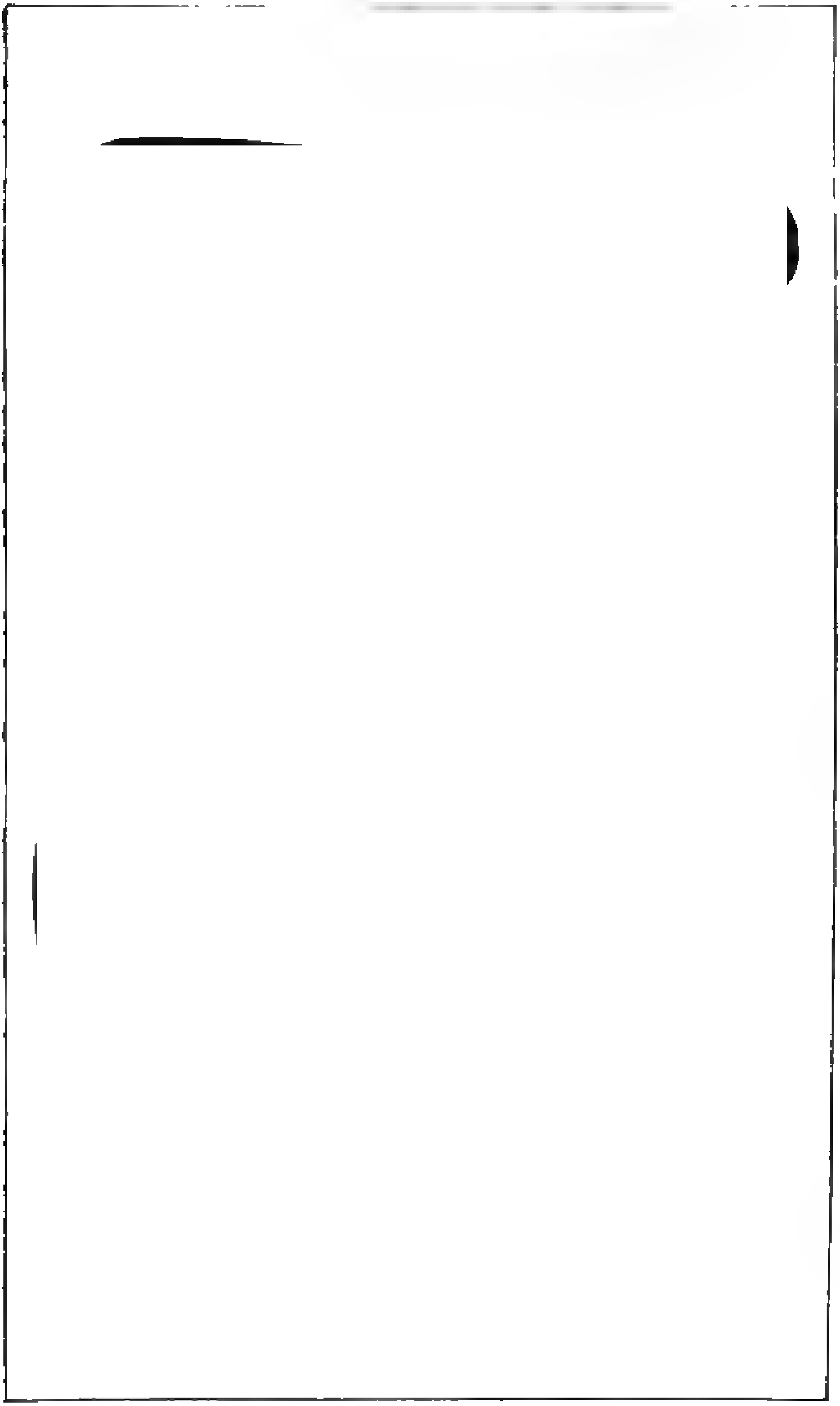
The fact that the implements recovered from the deep horizontal diggings are, so far as I have encountered them, all identical in type with the prevailing recent forms emphasizes the need of inquiring with the utmost care as to whether or not these implements could have been introduced while the mines were in operation. As already shown, the mountain Indians were in those days very numerous about the mining camps. The men were employed to a considerable extent in

the mines, and it is entirely reasonable to suppose that their implements and utensils would at times be carried into the mines, perhaps to prepare or contain food, or perhaps merely as a natural proceeding with half-nomadic peoples habitually carrying their property about with them from want of a house in which to lock it up. That any kind of native implement should be carried into the tunnels, there to be lost or forgotten and covered up as the handling and rehandling of gravels went on, is not unnatural. That such should be afterwards dug up with the reopening of passageways and the shifting of the tailings is to be expected, for the search for gold under these old lava beds was not a straight-away boring of the mountains, but a driving and redriving of tunnels in any direction that promised renewed finds of pay material. As a matter of course little attention was paid to the comings and goings of the humble helpers, and if miners came upon stray implements buried in the gravels it is quite natural that they should report them to the foremen or superintendents without seriously considering the question as to recent or ancient origin. Naturally little value was attached to such specimens, as the real significance of their occurrence in the old gravel was at most but dimly understood.

FIG. 2.—Section of Table Mountain showing mines penetrating to old river channels. The tunnels are not literally rendered, but are sketched in merely to show the methods of reaching the gold gravels. The position of the King find beneath the lava cap is shown. See illustration of specimen, Pl. XIV.

Again, let us not forget, it is quite within the bounds of probability that some fun-loving miner should have sought amusement by reporting objects found about the camp, to the superintendent or others, pretending that they came from beneath the mountain. There can be no doubt that practical joking of this character was prevalent in those days, and that implements of the classes involved in this discussion were known by the miners to excite unusual interest in religious as well as scientific quarters. There are thus two ways in which errors might have crept into the evidence—two ways, either of which would lead to that repetition of like finds which is considered so significant by advocates of antiquity.

The Neale finds.—The case cited in detail by Dr. Becker may well illustrate what I have been saying, and this case, it should be noted, is a typical one, and constitutes one of the strongest bits of testimony of



MORTAR AND PESTLE SAID TO HAVE BEEN FOUND IN MONTEZUMA MINE BENEATH THE LAVA CAP OF TABLE MOUNTAIN (ONE-HALF ACTUAL SIZE).

its class on record.[1] Mr. J. H. Neale was superintendent of the Montezuma mine, situated on the western slope of Table Mountain, 4 or 5 miles southwest of the village of Jamestown. The gold-bearing gravels of the old river bed beneath the mountain, covered by the claim, became exhausted, and the mine was closed several years ago. Mr. Neale now resides in the town of Sonora, 5 miles north of Jamestown. In 1877, according to Dr. Becker's account, Mr. Neale discovered some mortars, pestles, and obsidian implements in the deepest part of the mine, beneath Table Mountain and close to the bed rock. These objects soon passed out of his hands, and one of the mortars with the accompanying pestle (see Pl. XIII) was given to Dr. R. I. Bromley, of Sonora. Ten years after the finding these specimens came to the notice of Dr. Becker, who, desiring to learn more of their origin, sought out Mr. Neale, and obtained the statement to which affidavit was made, the circumstances being given in detail in Dr. Becker's paper. The essential paragraphs of the document are as follows:

> At a distance of between 1,400 and 1,500 feet from the mouth of the tunnel, or of between 200 and 300 feet beyond the edge of the solid lava, Mr. Neale saw several spearheads of some dark rock and nearly one foot in length. On exploring further, he himself found a small mortar three or four inches in diameter and of irregular shape. This was discovered within a foot or two of the spearheads. He then found a large, well-formed pestle, now the property of Dr. R. I. Bromley, and near by a large and very regular mortar, also at present the property of Dr. Bromley.
>
> All of these relics were found the same afternoon, and were within a few feet of one another and close to the bed rock, perhaps within 1 foot of it. (P. 192.)

I took the trouble to visit the mine, which was found closed and caved in about the mouth, and with a newly opened tunnel alongside. The site is on a steep slope, falling away to the west from the base of the towering escarpment of the mountain (and apparently much more than 1,500 feet from it), and is surrounded by limited areas upon which houses could be built or lodges pitched. All about I found traces of native occupancy, and a dozen mortars, pestles, and pounding stones were picked up. These did not differ in character or material from the corresponding varieties of utensils reported from the deep gravels. The Neale affidavit states that the mortars and other implements therein referred to were found in the tunnel, some 1,500 feet from the mouth of the mine and 200 or 300 feet in beyond the margin of the lava cap of the mountain, and hence beneath several hundred feet of the volcanic deposits that covered the country before the valleys of to-day began to be scored out (see fig. 2).

Is it not more reasonable to suppose that some of the typical implements of the Indians living at the mouth of Montezuma mine should

[1] Geo. F. Becker, Antiquities from under Tuolumne Table Mountain in California. Bull. Geol. Soc. of America, Vol. II, p. 189.

have been carried in for one purpose or another, embedded in the gravels, and afterwards dug up and carried out to the superintendent than that the implements of a Tertiary race should have been left in the bed of a Tertiary torrent to be brought out as good as new, after the lapse of vast periods of time, into the camp of a modern community using identical forms?

I took pains to have Mr. Neale tell me the story of the finds in all possible detail. The account as related in the work of Dr. Becker had evidently passed out of his mind in large degree, as it had also passed out of my own. His statements, written down in my notebook during and immediately following the interview, were to the following effect:

One of the miners coming out to lunch at noon brought with him to the superintendent's office a stone mortar and a broken pestle which he said had been dug up in the deepest part of the tunnel, some 1,500 feet from the mouth of the mine (see Pl. XIII). Mr. Neale advised him on returning to work to look out for other utensils in the same place, and agreeably to his expectations two others were secured, a small ovoid mortar, 5 or 6 inches in diameter, and a flattish mortar or

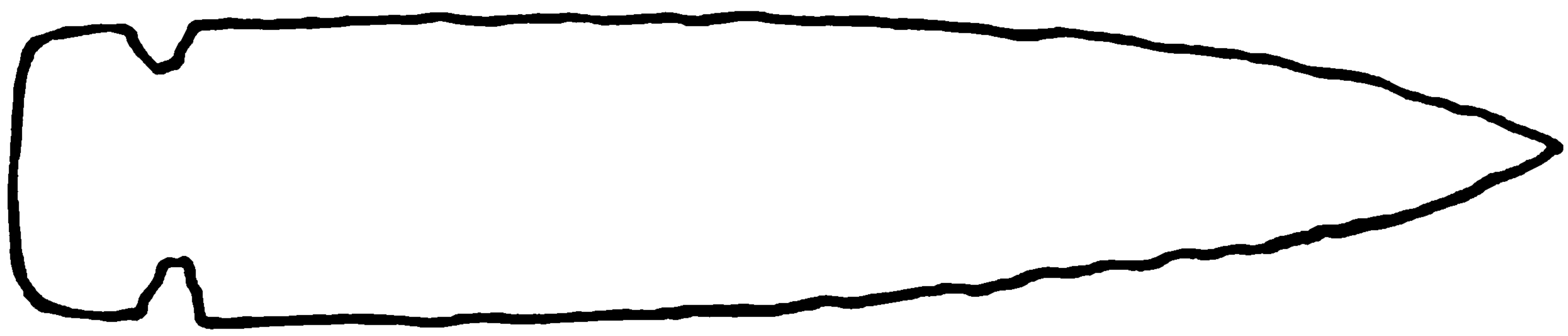


Fig. 3.—Outline of obsidian implement said to have been found in Montezuma mine, as sketched by Mr. Neale.

dish, 7 or 8 inches in diameter. These have since been lost to sight. On another occasion a lot of obsidian blades, or spearheads, eleven in number and averaging 10 inches in length, were brought to him by workmen from the mine. They had been found in what Mr. Neale called a "side channel;" that is, the bed of a branch of the main Tertiary stream, about a thousand feet in from the mouth of the tunnel, and 200 or 300 feet vertically from the surface of the mountain slope. These measurements were given as estimates only, but at the same time they were, he felt sure, not far wrong. Four or five of the specimens he gave to Mr. C. D. Voy, the collector. The others also had been given away, but all trace of them had been lost. Mr. Neale spoke enthusiastically of the size and perfection of these implements, and as he spoke drew outlines of long notched blades in the dust at our feet. Some had one notch (see fig. 3), some had two notches, and others were plain leaf-shape blades.

Desiring to find out more concerning these objects, he went on to say, he showed them to the Indians who chanced to be present, but, strangely enough, they expressed great fear of them, refusing to

touch them or even to speak about them; but finally, when asked whether they had any idea whence they came, said they had seen such implements far away in the mountains, but declined to speak of the place further or to undertake to procure others. This statement by Mr. Neale struck me at once as interesting and significant, and I was not surprised when a few days later it was learned that obsidian blades of identical pattern were now and then found with Digger Indian remains in the burial pits of the region. The inference to be drawn from these facts is that the implements brought to Mr. Neale had been obtained from some one of the burial places in the vicinity by the miners, who found no spot too sacred to be invaded in the eager search for gold. An additional inference is that the Indians were aware of the origin of the specimens and were afraid of them because of the mortal dread that every Indian feels of anything connected with the dead. How the eleven large spearheads got into the mine, or whether they ever came from the mine at all, are queries that I shall not assume to answer, but that they came from the bed of a Tertiary torrent seems highly improbable; for how could a cache of eleven slender, leaf-like implements remain unscattered under these conditions; how could fragile glass blades stand the crushing and grinding of a torrent bed; or how could so large a number of brittle blades remain unbroken under the pick of the miner working in a dark tunnel? For, as Dr. Becker states, "The auriferous gravel is hard picking; in large part it requires blasting."

That the affidavit of Mr. Neale does not materially strengthen the evidence favoring antiquity I am now fully convinced. In his conversation with me he did not claim to have been in the mine when the finds were made, and a sworn statement vouching for the truth of assertions made by other persons, and these other persons unnamed miners, can not be of value in establishing a proposition requiring proofs of the very highest order. That the other like finds of the Table Mountain region, recorded by Whitney, are equally open to criticism may reasonably be assumed.

The King Pestle.—The only bit of testimony that may not be challenged with impunity is the finding of a fragmentary pestle in the face of Table Mountain 2 or 3 miles north of the Montezuma mine by Mr. Clarence King and reported in detail and with an illustration in Dr. Becker's paper (p. 193), already referred to. Dr. Becker says:

"Another unpublished discovery has also been made in these gravels, which will be in so far more satisfactory to the members of this society, that the discoverer is well known personally to most of them and by reputation to every geologist. In the spring of 1869 Mr. Clarence King visited the portion of the Table Mountain which lies a couple of miles southeast of Tuttletown, and therefore near Rawhide camp, to search for fossils in the auriferous gravels. At one point, close to the high bluff of basalt capping, a recent wash had swept

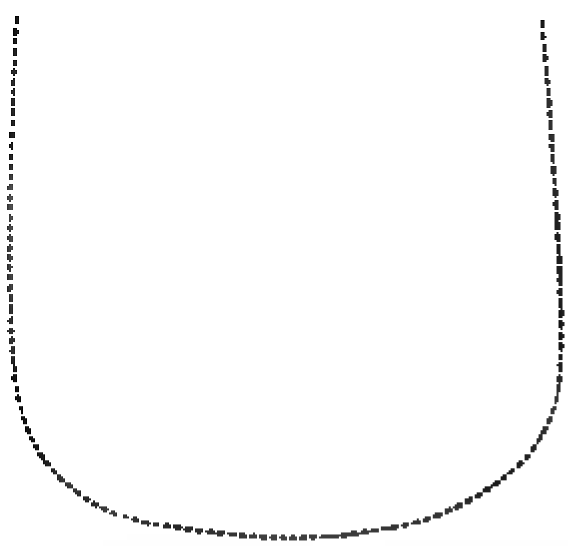
away all talus and exposed the underlying compact, hard, auriferous gravel beds, which were beyond all question in place. In examining this exposure for fossils he observed a fractured end of what appeared to be a cylindrical mass of stone. This mass he forced out of its place with considerable difficulty on account of the hardness of the gravel in which it was tightly wedged. It left behind a perfect cast of its shape in the matrix and proved to be a part of a polished stone implement, no doubt a pestle. It seems to be made of fine-grained diabase. * * * It is difficult to imagine more satisfactory evidence than this of the occurrence of implements in the auriferous, preglacial, subbasaltic gravels."

I sought the particular site from which the object was obtained, and passed up and down over every outcrop of rock on the slope, from the lava cap to the pasture fields below, in the hope of finding some trace of human handiwork, but beyond the usual Digger mealing stones scattered over the surface, nothing was found. I tried to learn whether it was possible that one of these objects could have become embedded in the exposed tufa deposits in recent or comparatively recent times, for such embedding sometimes results from a resetting or recementing of loosened materials, but no definite result was reached. This remarkable specimen is now in possession of the National Museum, and is shown in Pl. XIV, *a*, in connection with a typical pestle of the California tribes of modern times (*b*). It has been symmetrically shaped and the upper end is highly polished from long use in the hand.

The unfortunate part about this very noteworthy feature of the testimony is that Mr. King failed to publish it—that he failed to give to the world what could well claim to be the most important observation ever made by a geologist bearing upon the history of the human race, leaving it to come out through the agency of Dr. Becker, twenty-five years later.

THE CALAVERAS SKULL.

Notwithstanding the fact that the finds of stone implements in intimate relation with the auriferous gravels furnish the great body of testimony upon which a Tertiary man is predicated, they have attracted but slight attention from the public as compared with the reputed discovery of human remains, and more especially the discovery of the so-called Calaveras skull in a mine shaft at Altaville. The prominence of the latter find is due largely to the fact that it is the only specimen of its kind that has escaped oblivion. This relic has been the subject of much disputation, but I shall not stop here to cite or review the literature. It may be observed, however, that the general trend of sentiment and even of scientific opinion has been adverse to the specimen as proof of antiquity. At the same time there is a very important contingent of scientific men, especially those grouped about the original apostle of antiquity, Whitney, who cling tenaciously



a

b

STONE PESTLES.

a. Fragment of pestle obtained from Tertiary deposits of Table Mountain, by Mr. King. The lower end was probably longer than is indicated by the dotted line. (Three-fourths actual size.)

b. Modern Indian pestle of form common throughout California. Introduced for comparison. (Three-fourths actual size.)

to the idea that this and other finds of human bones are bona fide relics of Tertiary man. As long as this condition exists it is manifestly unwise to attempt to pass over the evidence of the Calaveras skull, as some are inclined to do, with the assertion that it is insufficient and hence unworthy of consideration.

In Plate XV, *a*, is given a view of the skull as it appeared when first brought to the attention of Whitney in 1866, and in *b*, we see it as it appeared after having been cleaned up by Dr. Wyman at Cambridge.[1] The former is from a photograph made by Alonzo Rhodes,

Fig. 4.—*Profile view of the Calaveras skull. From Plate I, Auriferous Gravels.*

at Murphys, Cal. Being faded, the photograph had to be redrawn for engraving, hence the cut has not the merits of a photograph directly reproduced. The latter is copied from a lithographic plate published by Whitney in his work on the Auriferous Gravels and is manifestly defective, quite a little of the character and natural ruggedness having been lost by the draftsman. The profile view, from the same work, is reproduced in fig. 4. The specimen is now preserved in the Peabody Museum at Cambridge, and comprises about three-fourths of the skull. Enough remains, however, to enable the craniologist to

[1] Auriferous gravels, Pl. I.

determine something of the physical characteristics and hence of the mental equipment of the person to whom it belonged. The account of the skull given by Whitney includes a careful description by Jeffries Wyman, one of the highest American authorities of the time. The whole subject is presented in such manner as to convey to the unprejudiced mind an impression that the skull is a genuine and well-authenticated relic of antiquity.

The skull is said to have been taken from the Mattison & Co. mine on the gentle slope of an oblong rounded hill, some 300 feet in height, situated in the suburbs of Altaville, a mile or more northwest from the important mining town of Angels Camp. This shaft is still open, a roomy rectangular well some 130 feet deep, cut in beds of compact, tenacious, volcanic rock and underlying strata of varying character, and has undergone little change in the thirty-three years that have passed since the reported finding of the skull. A road once passed the mine and continued around the hill, but it is now nearly obliterated, and all traces of buildings are gone from the slope, which is diversified only by occasional old mine dumps and a growth of scrubby trees. It was my intention to descend into the shaft and examine the formations, but there was no time to spare for erecting the necessary windlass. It is important that the formations at the depth from which the skull is said to have come should be examined for comparison with the material adhering to and partially filling the skull.

Whitney's account of the skull.—According to Whitney's account the skull was taken from the shaft of Mattison & Co's. mine in February, 1866. Mr. Mattison, with his own hands, took the skull from near the bottom of a bed of gravel 130 feet from the surface and within a few feet of the bed rock—the crystalline slates in which the Tertiary river had carved its channel. It was "lying on the side of the channel [of the Tertiary river] with a mass of driftwood, as if it had been deposited there by an eddy of the stream, and afterwards covered over in the deposit of gravel by which bed No. 8 was formed."

Figure 5 embodies the essential features of a section obtained by Mr. Edward Hughes, of Stockton, in connection with an unpublished paper on the Calaveras skull, written by Dr. A. S. Hudson. It seems to correspond in every essential feature with the section published by Whitney and with a section furnished me, together with photographs of implements and human and animal remains from the region, by Mr. R. E. C. Stearns, of Los Angeles.

According to Whitney, Mr. Mattison did not recognize the object as a skull when taken from the gravel, but "thought it to be a piece of the root of a tree." Mr. Scribner also stated that when the skull was brought to him "it was so embedded and incrusted with earthy and stony material that he did not recognize what it was." Mr. Mat-

tison, however, seems to have considered the curious gravel-covered lump of sufficient interest to note carefully the conditions under which it was found, "as if deposited in the eddy of a stream," and soon afterwards carried it in a bag to Angels, presenting it to Mr. Scribner, merchant, and agent of Wells-Fargo & Co. It was not until a clerk in Mr. Scribner's store, probably Mr. Matthews, cleaned off a portion of the incrusting material that anyone suspected that the object was a human skull. Soon after this the skull was sent to Dr. William Jones, at Murphys, 12 miles away. The Doctor was an enthusiastic collector of natural-history specimens, and, regarding the skull as having more than ordinary interest, wrote to the office of the State geological survey in San Francisco, describing the specimen. A few days later, on June 29, at the request of Mr. William M. Gabb, paleontologist of the survey, the Doctor forwarded it to San Francisco.

Professor Whitney soon afterwards visited Calaveras County and proceeded to make careful inquiries into the origin of the skull. He visited Mr. Mattison and others, obtaining the statements embodied in

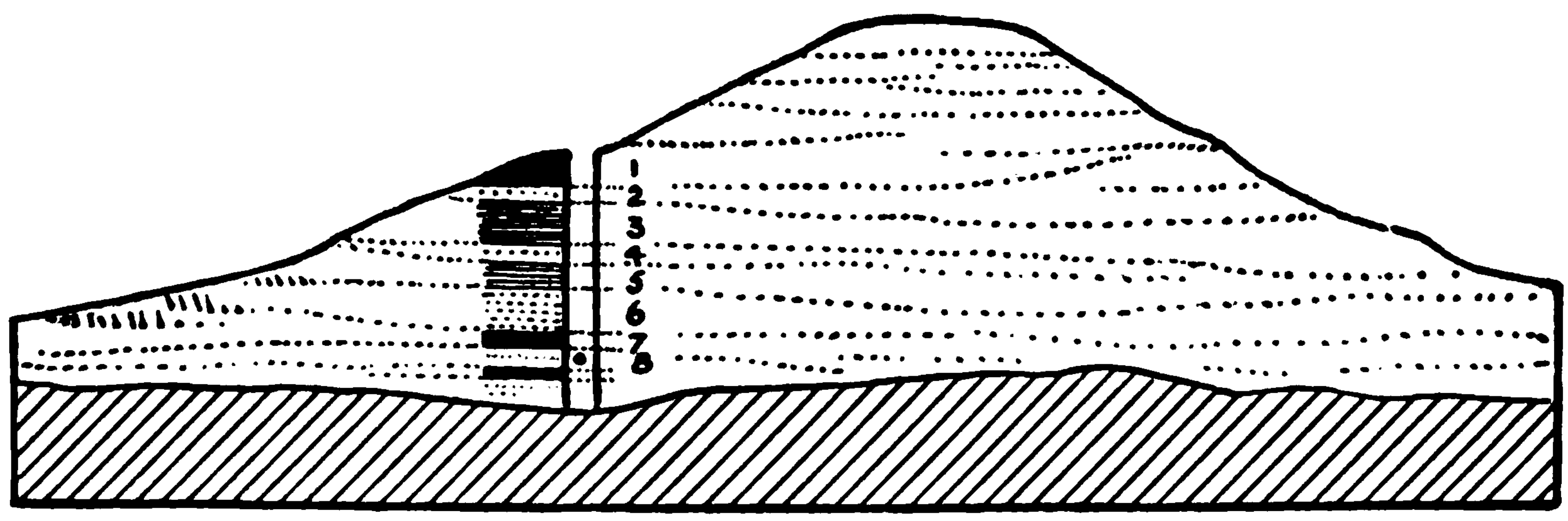


FIG. 5.—Section of the deposits exposed in Mattison mine, Bald Mountain. The skull is said to have been found in stratum No. 8.

his report, and became convinced that the skull had been found precisely as described by Mr. Mattison, and that its subsequent history was correctly given by Mr. Scribner and Dr. Jones.

When delivered to Professor Whitney the base of the skull was "embedded in a conglomerate mass of ferruginous earth, waterworn pebbles of much altered volcanic rock, calcareous tufa, and fragments of bones. This mixed material covered the whole base of the skull and filled the left temporal fossa, concealing the whole of the jaw. A thin calcareous incrustation appears to have covered the whole skull when found; portions of it had been scaled off, probably in cleaning away the other material attached to the base" (Pl. XV, *a*). Together the two eminent professors carefully chiseled away the foreign matter adhering to its base, so as to expose the natural surface of the skull, leaving it in its present state (Pl. XV, *b*). The skull was found to be that of a very old person, the teeth being gone and the alveoli nearly absorbed. The lower jaw is gone and the cranium is far from perfect; portions of the occiput are missing and the remaining por-

tions are badly fractured. Professor Whitney expresses his views as to how the specimen came to be thus rudely fractured, and as to subsequent events in its history, in the following words:

"The skull was unquestionably dug up somewhere, and had unquestionably been subjected to quite a series of peculiar conditions. In the first place, it had been broken, and broken in such a manner as to indicate great violence, as the fractures go through the thickest and heaviest parts of the skull; again, the evidence of violent and protracted motion, as seen in the manner in which the various bones are wedged into the hollow and internal parts of the skull, as, for instance, the bones of the foot under the malar bone. The appearance of the skull was something such as would be expected to result from its having been swept, with many other bones, from the place where it was originally deposited down the shallow but violent current of a stream, where it would be exposed to violent blows against the bowlders lying in its bed. During this passage it was smashed, and fragments of the bones occurring with it were thrust into all the cavities where they could lodge. It then came to rest somewhere, in a position where water charged with lime salts had access to it, and on a bed of auriferous gravel. While it lay there the mass on which it rested was cemented to it by the calcareous matter deposited around the skull, and thus the base of hard mixed tufa and pebbles which was attached to it when it was placed in the writer's hands was formed. At this time, too, the snail crept in under the malar bone, and there died. Subsequently to this the whole was enveloped by a deposit of gravel, which did not afterwards become thoroughly consolidated, and which, therefore, was easily removed by the gentlemen who first cleaned up the specimen in question, they only removing the looser gravel which surrounded it" (p. 272).

In cutting away the incrusting material several fragments of bones were found—some that might have belonged to the same individual to whom the skull pertained, while others evidently belonged to a smaller person. Besides these there were bones of some small mammal, a small snail shell of the species *Helix mormonum*, a small wampum or shell bead, and some bits of charcoal.

Chemical examinations by Mr. Sharpless developed the fact that nearly all the organic matter of the bone had disappeared and a large portion of the phosphate of lime had been replaced by the carbonate, indicating a fossilized condition; a trace only of organic matter remained.

From Dr. Wyman's report, published in Whitney's paper, we learn:

"First. That the skull presents no signs of having belonged to an inferior race. In its breadth it agrees with the other crania from California, except those of the Diggers, but surpasses them in the other particulars in which comparisons have been made. This is especially obvious in the greater prominence of the forehead and the capacity of its chamber. Second. In so far as it differs in dimensions from the other crania from California it approaches the Eskimo" (p. 273).

Portions of the above statements will be referred to in some detail on.

Information from local sources.—During my short visit to the district I found only a few men who could claim personal knowledge of the skull and of the people most directly concerned in its discovery and immediately subsequent history. Scribner and Jones are dead and others have removed from the district. At Big Trees, 18 miles above Murphys, I found Mr. J. L. Sperry, who kept the hotel at Murphys and was Whitney's host while the latter was visiting that section. He proved to be a good friend of the Professor and a believer in the correctness of his views regarding the skull. His hotel faced the office of Dr. Jones, to whom the skull was sent from Scribner's, and he told me that one day as he was standing in the door of his hotel Dr. Jones came out of his office opposite, and with characteristic imprecations threw a broken skull into the middle of the street. Called upon to explain, the Doctor said that the skull had been brought to him as a relic of great antiquity, but that he had just discovered cobwebs in it, and concluded that he had been made the subject of one of Scribner's practical jokes. Afterwards the Doctor picked up the specimen again and carried it into his office, saying that perhaps he had been too hasty and that he would give it further consideration. Shortly afterwards the skull was sent to San Francisco, and a little later Whitney returned to Murphys and proceeded to make inquiries as to its origin. Mr. Sperry drove him to Angels Camp to see Mattison and to obtain from him a statement regarding the discovery of the skull. The statement was obtained, and satisfied Whitney as to the genuineness of the find. The opposition to the evidence was, he said, mainly the result of religious prejudices and, he thought, had no solid foundation.

Others at Murphys were familiar with the story, often told and retold, but all were unbelievers and took great pleasure in telling of the practical jokes perpetrated by Scribner and his coterie upon their friends, and upon Dr. Jones in particular. In general the versions of the story of the skull were much alike, showing a common origin, but having individual variations characteristic of memory recitals. I talked with J. L. N. Shephard, C. A. Curtis, W. J. Mercer, E. H. Schaeffle, and others well informed on the events of the early days; and the statement by Mr. Joseph Shephard, a prominent local engineer, made in writing to Mr. H. W. Turner, of the United States Geological Survey, may serve to indicate the general trend of these accounts and the character of the persons connected with the story of the skull. His statement is as follows:

"When the skull was found in Mr. Madison's (Mathewson's) shaft, there lived in Angels three men, John Scribner (merchant), William Coddington (ditch owner), and Ross B. Coons (saloon keeper). In Murphy's there lived William Griffiths (ditch superintendent) and Dr. Jones, all good friends one with another, and all owners in the Union Water Company's ditch, except probably Coons. Griffiths delivered the skull to Dr. Jones, how long after Madison (Mathewson) found it

I know not, but when Dr. Jones found cobwebs in it he threw it out of his office, but decided to take it back again. From this on I suppose the history of the skull is well known. I recollect that when the public began to talk about it, the common belief was that Scribner, Coddington, and Coons, of Angels, and Griffiths, of Murphys, knew how the skull got into Madison's shaft, and used it simply to play a practical joke on their friend Dr. Jones; and, as has been said, they were capable of doing it. There is no doubt that Madison was sincere in his belief that the find was genuine."

As all authentic details relating to the history of the skull are valuable, the following extracts are made from a paper written several years ago by Dr. A. S. Hudson, of Stockton. The manuscript was obtained for me by Professor Edward Hughes, of Stockton, and being imperfectly finished and somewhat erratic in treatment, it is not considered advisable to publish it in full, but such parts as relate to the author's visit to the mining region are interesting and suggestive and may be given.

In 1883 Dr. Hudson corresponded with Dr. John Walker, of Sonora, who, in a letter, stated that he had taken a lively interest in the skull, opposing its claims to authenticity, and had endeavored to convince Whitney that he was doing a great injury to science by accepting the evidence. He induced a friend to convey to Whitney the information that "the specimen was found at Salt Spring Valley, near the surface, and not in a mine on Bald Mountain; but Whitney treated the information discourteously." Continuing, the letter stated that "about the time the discovery was made several caves were found and skulls of the same description taken from them. They were evidently the burial places of Digger Indians. No one about the diggings supposed otherwise."

Later Dr. Hudson visited Dr. Walker at Sonora, but made up his mind that the Doctor had little actual knowledge of the matter, and slight foundation for his assertion "that the whole affair was a fabrication and a joke on Whitney." Going on to Angels, he interviewed Scribner and Mattison. He was most favorably impressed with Mr. Scribner, who in a dignified and convincing manner assured him that Dr. Walker was wrong, and that no deception whatever had been practiced. Having gathered all the facts in the case that Scribner cared to impart, the Doctor visited Mr. Mattison, "the veritable miner and supposed discoverer of the head of our inquiry. Fortunately he and his wife were found at home, and without hesitation proceeded to relate the story, with the steps which brought the find to light. The man's wife had a better memory than he, and she seemed to be equally well informed about it. Thus I was furnished with two witnesses in one home. It was said: late in the year 1865 he (Matson) began to dig for gold. He sank his shaft in Bald Mountain, and not Table Mountain. * * * Reaching the depth of 128

feet, the industrious miner struck some old wood. Here in neighborly pose the remains of vegetable and animal [human] life were found. They were found embedded in gravel and a kind of cement, which he thought was wood also. Taking the round or globular, dirt-covered bundle home, he said nothing about it to his family, but kept it in his house a year or more. Here I showed Matson and his wife the figure or cut copied from Professor Whitney's book. * * * Mrs. Matson at once recognized the picture as representing the specimen in question.[1] It was said the cemented gravel so adhered to it as to fill out the back head and make it look a natural occipital portion."

Dr. Hudson left Calaveras County "perplexed and discouraged." The stories told him seemed "incomplete and incoherent." "But," he continues—

"Some two weeks later Mr. Scribner called at our office in Stockton with the welcome errand of a refreshed memory, and with additional facts fitting into the body of the narrative, making it more consistent. * * * It seems, as time went on, Mrs. Matson, an orderly housekeeper, began to take a dislike to that untidy thing—an unwashed dead head in her house—and made complaint. It was more in the way than of use or ornament, and she decided to get rid of it. Thereupon her husband, like a proper acquiescing partner in life, carried it to Mr. Scribner's store, where at the same time the Wells-Fargo Company had its business office. Mr. J. C. Scribner and his partner, Mr. Henry Matthews, now became the uninvited custodians of the topmost part of an aged and unknown man. * * * This man Matthews had a common failing among people—he was fond of liquor—and sometimes indulged his taste to excess. Some few days, or maybe weeks, prior to the advent of the skull at Scribner's, Matthews, not feeling well, paid a visit to Dr. Jones, a worthy physician at Murphys, consulted him in regard to his health, and obtained from the Doctor a prescription and medicine. The medicine proved rather strong; it depleted the patient rapidly and produced unlooked-for discomfort. As he grew weaker and impatient under the continued action of the purge, it made Matthews swear; he swore at the unholy medicine and at the d—d outcast of a doctor who gave it. The natural result was, he became cross toward Dr. Jones. Not to lose sight of the skull, we note that as soon as Mr. Scribner saw the dirty, rotted remains of old mortality before him, so soon he decided it was out of his line, and he did not want the offensive thing about. But Matthews took to it instinctively and at once. He thought that it, with some half-rotted and half-petrified pieces of wood and a few lumps of native ore might do to embellish Dr. Jones's cabinet of geological and natural history curiosities. Therefore they, the uneasy head and the rest, were immediately dumped into an empty potato sack and sent to Dr. Jones at Murphy's. On the same day it came, without note, comment, or message, and Esculapius opened the sack and took out its contents one by one. After a short inspection of the specimens of

[1] A comparison of the skull as it originally appeared and as seen by Mrs. Mattison, and the skull as cleaned up by Wyman and illustrated by Whitney will be instructive in this connection. See Pl. XV; also Auriferous gravels, p. 268.

ancient remains, he, with a pious imprecation on the head of the other fellow and his impudence, gave it a toss into the back yard. There the bony thing, which had long resisted the tooth of final destruction, was again exposed to a more quickening action of hurtful elements. There in the damp of rain and mildew it remained for many months unnoticed. There it is quite likely—indeed, probable—that the little *Helix mormonum*, which can be seen photographed at the left-hand base of the figure (Pl. XV, *a*), became attached.

"At length Mr. Matson, in one of his occasional visits to Murphys, saw, like a familiar ghost, his old acquaintance, the same old head. He inquired of Dr. Jones where he got it, not knowing what disposition Mr. Scribner and company had made of it. Learning for the first time that several months anterior thereto Matson had dug the head out of his own shaft 128 feet below the surface of the ground, the Doctor then suspected it might turn out something of interest. These unlooked-for facts at once invested the dirty topknot with new and even profound considerations. It was soon photographed by Mr. Alonzo Rhodes, of Murphys, and the negative was sent to Mr. Shew, at San Francisco, where pictures were printed. The attention of Professor Whitney was now called to the resurrected head. He, in company with Mr. Matson, the miner, visited the now old and abandoned miners' shaft. They found it partly filled with water and dirt, which was soon pumped dry. Mr. Matson pointed out on the wall of the bank the precise spot the interesting object lay in conjunction with fragments of wood. The wood he thought was a fragment from quite a large tree. From this spot Whitney told Mr. Scribner he gathered gravel and carefully compared it with that scraped from the skull.[1] They proved identical one with the other. It seemed the gravels in the different layers above were of other kinds. This fact precludes the possibility of designing person or persons securing the object from 'Salt Spring Valley' (as opposers have asserted), and dropping it down the shaft. I inquired of Mr. Matson how it came to be rumored that the skull was taken from 'mud spring in Salt Spring Valley' and thence conveyed to his mining shaft? He answered, 'Before I began mining at that place and several years back into the decade of 1850, a Mrs. Hoffman had gathered several skulls from Salt Spring Valley, a place some 12 miles distant from Angels, and had them on exhibition in a sort of cabinet collection.' One of these heads had been fractured and crushed on the left parietal bone, the line of fracture running to the temple. Some similitude or relationship between these and the Calaveras head was believed to exist. But how or in what manner nobody could tell, for none knew.

"It may be proper here to say that Mr. Matson is a plain, hard-working day laborer, a blacksmith by calling. He seems to be a very honest-appearing man. He evidences no disposition to magnify, falsify, or to depart from the correct line of truth. Here ends all there is or, as far as I can learn, ever was, about the so-called 'joke' over the Calaveras skull, except its occasional rehearsal and the more important fact that it was a joke by Matthews on Dr. Jones and not on Professor Whitney.

"As mentioned above, the animus of it was not to play upon the spirit of scientific inquiry nor to deride native anthropological study,

[1] Compare with "Auriferous gravels," p. 271.

a

b

THE CALAVERAS SKULL.

a. Copied from a photograph made by [illegible] Rhodes at [illegible] [illegible] daguerreotype plate

but it was a trick sprung on the spur of the moment, in a spirit of humorous hilarity, by Matthews, Scribner's partner in business. But the Doctor, being the victim, did not see the point."

This story is interesting as emanating from Mr. Scribner, who, according to many accounts, knew more than any other person regarding the origin and early movements of the skull.

At Angels Camp I visited Mr. Rasmussen, a former business partner of Mr. Scribner's, but he had given the matter little attention and did not know whether Scribner believed in the authenticity of the skull or not; but Mr. George Stickle, present postmaster of the village, showed a decided interest in the matter. He had been closely associated with the Scribner coterie in the early days, and knew all the principal people of Angels Camp almost from its foundation. It is his belief that the whole affair grew out of the "joshing" proclivities of his fellow-townsmen, and he laughed heartily as he recited the circumstances of the finding and subsequent misadventures of the so-called Calaveras skull. He went on to state that the skull had been in his store several weeks before it fell into the hands of his fun-loving associates. Together with a companion specimen, it had been brought to him from a burial place in Salt Spring Valley, 12 miles west of Angels, by Mr. J. I. Boone. I was extremely sorry not to be able to visit the supposed place of origin of so famous a specimen, for the stories seemed sufficiently circumstantial to warrant scientific attention.

Is it a changeling skull?—According to some of the current stories of the region, the skull was placed in the mine by one of Mattison's neighbors merely as a joke, while he was at home for dinner, and he is supposed to have found it where it was buried among the débris at the bottom of the shaft. This may or may not be true. At any rate, as no names are given, the statement can not be verified.

The remark made by Mr. Stickle and others that the skull obtained by Whitney did not come from the Mattison mine or through Mattison at all may also have little value as evidence; but it is suggestive, and gives rise to a legitimate inquiry as to the possibilities in the case. There were ancient skulls in plenty in this region in early times, and the valley and county received their name *Calaveras*—which in Spanish signifies *skulls*—from this circumstance.

The Indians of the high sierra do not bury their dead, but cast them into pits, caverns, holes in the rocks, and deep gorges. Generation after generation follows one another into these gaping Golgothas where, in a confused heap, along with rude personal belongings and sacrificial offerings, the bodies decay and are covered by accumulating débris and deposits from running or percolating waters. As mining operations went on these burial places were cleaned out and the bones became public property. Skulls were plentiful at Angels in those

days, as many persons testify. There is, therefore, a chance that the skull sent to Dr. Jones was not the one found by Mattison, but a cement-covered specimen derived from some other source, as Stickle states and Scribner suggested. Certainly there were several months during which little or no trace was kept of the lump of conglomerate carried home by Mattison. The usual answer to the suggestion that there might have been a changeling skull is that the Calaveras specimen is not a common skull, but a fossil, and must have come from gravel deposits identical with those in Bald Mountain, if not actually from the Mattison mine, and that its great age is thus sufficiently established. But who shall say that many of the skulls found about Angels Camp were not obtained from comparatively recent burials in surface exposures of auriferous gravels or in other gravels where the conditions were such as to permit of rapid ferruginous and calcareous cementation, giving rise to phenomena identical with those observed in the Calaveras skull?

Testimony of the skull itself.—Recognizing the fallibility of human testimony and the consequent difficulty of surely connecting the Calaveras skull with the gravels in place in Bald Mountain, the characteristics and condition of the skull itself have been appealed to by advocates of its authenticity. The report on its physical characters, however, made by Jeffries Wyman, does not in any way aid the case. It is to be expected that a Tertiary skull would in some manner show or suggest inferior development, but this skull appears to represent a people equal or superior to the present Indian tribes of the region. Again, it is to be expected that some distinctive characteristic, some race peculiarity, would appear in the skull of a people separated by uncounted centuries from the present; that it would be longer or shorter, thicker or thinner, or more or less prognathous than the Indian skull, but Wyman has nothing more startling to say than that "in so far as it differs in dimensions from the other crania from California, it approaches the Eskimo." This vague variation is just as likely to be an individual peculiarity as a racial character. It need not be regarded as strange that the skull should be superior to the average Digger cranium, for no anthropologist would be willing to affirm that the Diggers are the first and only people who have occupied this region during the present geological period. The chances are that the Shoshonean stock, to which these Diggers belong, is a somewhat recent intruder on the western slope of the sierra in California, and more than one of the present or past groups of Pacific coast Indians may have passed this way at some period in their history. The practical identity of the skull with modern crania speaks very eloquently against extreme antiquity.

I am glad to be able to introduce here a comparison, made by Dr. George A. Dorsey, of Chicago, between the Calaveras skull and a modern Digger skull obtained from a burial cave at Murphys, a few

miles east of the locality assigned to the more ancient relic. The modern skull undoubtedly belongs to the people now occupying the region and in all probability to the occupants of a village located within half a mile of the cave. It was obtained, with other human bones, from a slide or cone of loose débris just beneath the narrow, nearly vertical mouth of the cave, by W. J. Mercer, of Murphys, who owns the property. It still retains small portions of the lank black hair and of the partially desiccated fleshy parts, the latter still emitting an offensive odor, indicating recent inhumation.

Dr. Dorsey's report is as follows:

"A comparison of the skull of a Digger Indian of Calaveras County, California, forwarded by Professor Holmes, with a fossil skull from the same locality, described by Professor Whitney, has been made from the two views of the skulls published by Whitney and from the following description by Dr. Wyman, quoted by Professor Whitney on pages 272–273 of the 'Auriferous Gravels of the Sierra Nevada:' 'The volume of the frontal region is large, so that if the skull were viewed from above the zygomatic arches would be nearly concealed. As a large part of the occiput is destroyed, it is uncertain whether the head was long or broad. The face is somewhat deformed, the left orbit being smaller and the left cheek higher than the right, thus giving the whole an unsymmetrical appearance. The ridges over the orbits are strongly marked, and the lower border of the opening of the nostrils is not sharp, but, as in some of the crania of many savage races, is rounded, and the malar bones are prominent. The strongly marked borders of the orbits are the most striking features of the fragment.'

"Dr. Wyman also made six measurements of the fossil skull. These I also quote, placing beneath them in tabular form similar measurements made on the skull of the Indian (Digger):

	Breadth of cranium.	Breadth of frontal.	Frontal arch.	Length of frontal.	Height of cranium.	Zygomatic diameter.
	mm.	*mm.*	*mm.*	*mm.*	*mm.*	*mm.*
Fossil skull	150	101	300	128	134	145
Digger skull	152	104	336	120	141	148

"It will be seen at a glance that the Digger agrees in its measurements with the fossil skull more closely than does any of the other skulls measured, which were placed by Dr. Wyman side by side with the Calaveras skull. There is a considerable discrepancy in the measurements of the frontal arc, but as the skull measured by Dr. Wyman was fragmentary, this measurement would be hard to take with accuracy on the fossil skull, and hence the opportunity for error would be greatest. In all other respects there is no greater discrepancy between the Digger and the fossil skull than might be found in any two skulls of the same tribe. When one compares the Digger skull with the pictures of the fossil skull there is a pronounced resemblance. Both are male skulls, having a pronounced supraorbital ridge, prominent glabellas and mastoid processes, and sharp and pronounced temporal crests. Both skulls are of a rather marked prognathic type; in both skulls also the entire orbital rim is heavy and pronounced.

The Digger skull, like the Calaveras, has a large and highly developed exterior angular process. In the Digger skull also the volume of the frontal region is large, and in the *norma verticalis* the zygoma are nearly concealed. In another respect also the two crania are alike, the nasal crests in the Digger skull having the same rounded and sloping surface which is one of the points emphasized by Dr. Wyman and which the engraving brings out so well. In regard to the orbital openings, while measurements are not at hand for the Calaveras skull, there is a general similarity between the two, and in both crania there is a broad infraorbital space.

"While the comparison of an actual skull with the drawings of a fragment of another must be unsatisfactory, yet the conclusion is necessary that the two skulls have the same general features and may easily be pronounced of one and the same type."

Professor Whitney lays much stress upon the fact that the specimen is undoubtedly a fossil.

"Chemical analysis proves that it was not taken from the surface, but that it was dug up somewhere, from some place where it had been long deposited, and where it had undergone those chemical changes which, so far as known, do not take place in objects buried near the surface."

If there was a trick on the part of fun-loving miners, "they must themselves," he adds, "have obtained from somewhere the object thus used; and as all the diggings in the vicinity are in gravels intercalated between volcanic strata, it becomes, really, a matter of but little consequence, from a geological point of view, from whose shaft the skull was taken."[1] It would appear that Whitney failed to notice that, although the gravels were originally wholly intercalated with strata of volcanic materials, they have been exposed in many places by the erosion of valleys, that they outcrop on the hillsides and lie uncovered in the valleys, and that any of the modern tribes may have buried their dead in previously undisturbed Tertiary river gravels. I learned of more than one case of this kind; and when so buried, there is no reason why the osseous remains, especially if deeply covered by over-deposits of shifting materials, should not have assumed in a comparatively short period of time exactly the conditions characterizing a fossil. Such comparatively recent burials in exposed very ancient river gravels may readily have taken place within less than a hundred yards of the Mattison mine, in fact in the actual beds exposed in the mine (Fig. 5,) since these outcrop in the slopes of Bald mountain.

The term "fossil" really signifies little in this connection, although assumed by some to signify much. No one would venture to assert that a skull might not lose nearly all its organic matter, and that a large portion of the phosphate of lime might not be replaced by the

[1] Auriferous Gravels, p. 271.

carbonate in a few hundred years if the conditions were reasonably favorable to the change. That such changes do not readily take place very near the surface is probably true; but we must not lose sight of the fact that, setting aside the possibility of the accumulation of deep overplacements, burial in caves and pits was practiced in this section and that these receptacles are sometimes of very considerable depth. Bodies cast in are rapidly covered up and are subject to just such conditions as those favoring fossilization.

It should be noted that silicification of the osseous matter of the skull is not mentioned; iron and lime from surface coatings merely. Iron is everywhere and its reactions are rapid; and in a region abounding in limestone formations calcareous matter is freely dissolved, carried, and redeposited by the waters. The conditions characterizing the skull are just such as might be expected in a skull coming from one of the limestone caves, crevices, or pits of the district. The thin film of calcareous matter coating the skull and extending throughout the porous filling makes it heavy, but does not necessarily indicate a prolonged period of inhumation.

It would appear from statements made by Scribner (in Hudson's paper, already quoted) that Whitney descended into the mine and examined the gravel bed from which the skull is said to have been obtained, but in his monograph the latter states that he failed to accomplish this on account of the water in the mine. He says that "the excavation has remained filled with water during the whole time since the skull came into my possession." (P. 271.) However, some one must have succeeded in overcoming the difficulty, as Dr. W. H. Dall states[1] that while in San Francisco in 1866 he compared the material attached to the skull with portions of the gravel from the mine and that they were alike in all essentials. But even if the material from the mine is like that attached to the skull, nothing is proved, as the same may well be true of materials from many parts of the Angels district. The peculiar agglomeration of earth, pebbles, and bones is readily explained by referring to conditions existing in the limestone caverns and crevices of the region where the calcareous accretions bind together bones, gravel (very generally present), cave earth, and whatever happens to be properly associated, in just such manner as that illustrated in the specimen under discussion.

Again, much stress is laid on the fact that the skull obtained by Whitney "had been broken in such a manner as to indicate great violence," as if subject to severe blows while swept by a torrent over a bed of bowlders. When it is remembered that the fractures exhibited by the skull are fresh and sharp, this highly imaginative statement (previously quoted in full) loses its force, for the tossing in a

[1] Proceedings of the Academy of Natural Sciences of Philadelphia, 1899.

torrent over bowlders would not only have bruised and abraded the sharp edges of the bone, but the loose earth, broken bones, wampum, and shells, instead of being packed into the skull, would have been quickly dislodged and widely scattered by the rushing waters. The facts are that the conditions of fracture and the impacting of bones of more than one individual, along with other miscellaneous articles, in the cavities of the skull, are just such consequences as would result from pitching body after body into an Indian burial pit, where young and old were jammed into a conglomerate mass and covered with earth, gravel, and stones.

The presence of a wampum bead embedded with the earth, bones, and pebbles in the skull is a strong argument against antiquity. It is not claimed that this shell bead is fossilized, and it would seem that it resembles in every way—size, shape, manner of boring, and degree of elaboration—the concavo-convex beads made from clam shells and worn by members of nearly every Indian family in California. That a Tertiary people should have made and worn the identical form seems highly improbable.

The small snail shell, the fragile *Helix mormonum*, found also in the skull, is much more at home in a modern burial place than in the torrent-swept bed of a Tertiary river. The species is recent, and I am not aware that it has been found in Tertiary formations.

It thus appears that the so-called Calaveras skull exhibits nothing in its character, condition, or associated phenomena incompatible with the theory of recent origin and very much that may be justly construed as favoring that theory.[1]

The skull at Cambridge.—On returning to the East I took the first opportunity of visiting Cambridge for the purpose of examining the Calaveras skull. Professor Putnam very kindly removed the specimen from its resting place and permitted me to examine it at leisure and to handle the loose materials—the lime-cemented earth, the bits of bones, and the shell bead—detached by Professor Wyman. I had looked forward with great interest to this glimpse of the specimen about which so much has been said and upon which so much has been and is predicated, and was prepared to be duly impressed with its character as a fossil, but I was distinctly disappointed. The importance of the skull as an index of antiquity has been overestimated. I find myself confirmed in the conclusions forced upon me by a consideration of the evidence already presented, namely, that the skull was never carried and broken in a Tertiary torrent, that it never came from the old gravels in the Mattison mine, and that it does not in any way rep-

[1] Contemporaneously with the preliminary publication of the present paper in the American Anthropologist, a short paper, written by Prof. W. P. Blake, of the University of Arizona, and referring to many of the questions here presented, appeared in the Journal of Geology of the Chicago University for October and November, 1899.

resent a Tertiary race of men. If the existence of Tertiary man in California is finally proved, it will be on evidence other than that furnished by the Calaveras skull.

Notwithstanding the above decided averments I must allow that with respect to the question of Tertiary man in California no final conclusion can as yet be drawn. I do not regard the investigation as satisfactorily completed and desire in the present writing only to state the problems and present the evidence in a way that will tend to bring out and establish the truth.

SUMMARY.

A brief summary of the arguments for and against the great antiquity of man in the gold belt of California may well be presented here for convenience of reference. The principal considerations arrayed in support of the affirmative are as follows:

(1) During the three or four decades succeeding the discovery of gold in California the miners of the auriferous belt reported many finds of implements and human remains from the mines. The formations most prominently involved are of Neocene age; that is to say, the middle and later portions of the Tertiary.

(2) Most of the objects came from surface mines, but some were apparently derived from tunnels entering horizontally or obliquely and to great depths and distances beneath mountain summits capped with Tertiary lavas, leading to a belief in their great age.

(3) The finds were very numerous and were reported by many persons, at various times, and from sites distributed over a vast area of country. They were made, with one exception, by inexpert observers—by miners in pursuit of their ordinary calling—but the statements made by the finders are reasonably lucid and show no indications of intentional exaggeration or attempted deception.

(4) The stories as recorded are uniform and consistent in character, and the objects preserved are, it is claimed, of a few simple types, such as might be expected of a very ancient and primitive people. The evidence, coming from apparently unrelated sources, is described as remarkable for its coherency.

(5) The reported finding of an implement in place in the late Tertiary strata of Table Mountain by Mr. Clarence King is especially important and gives countenance to the reports of inexpert observers.

(6) The osseous remains recovered are, in some cases, said to be fossilized, having lost nearly all their animal matter, and some are coated with firmly adhering gravels resembling those of the ancient deposits. These conditions give rise to the impression of great age.

(7) The flora and fauna with which the human remains and relics appear to be associated indicate climatic conditions and food supply favorable to the existence of the human species. It is a noteworthy

(6) Objects of art from the auriferous gravels are said to be of the most primitive character, and, in large measure, peculiar to the gravels. When critically examined, however, they are found to belong to the polished-stone stage and to duplicate modern implements in every essential respect. They are such as may have fallen in from Indian camp sites or been carried in by the Indians themselves. They are made from varieties of stone belonging to formations ranging from the oldest to the youngest found in the district, and have been shaped by the ordinary processes employed by our aborigines. They evidently served purposes identical with the corresponding implements of our Indian tribes.

(7) None of these objects show evidence of unusual age, and none bear traces of the wear and tear that would come from transportation in Tertiary torrents. These striking facts relating to the condition of the human and cultural remains confirm and enforce the impressions received from a study of the geological and biological history of the region.

(8) The case against antiquity is strengthened again by a study of the recent history of California. All, or nearly all, of the phenomena relied upon to prove antiquity can readily be accounted for without assuming a Tertiary man. Indian tribes have occupied the region for centuries. They buried their dead in pits, caves, and deep ravines, where the remains were readily covered by accumulations of débris or of calcareous matter deposited by water. As soon as mining operations began, the region became noted as a place of skulls.

(9) Coupled with the above is the fact that no other country in the world has been so extensively and profoundly dug over as this same auriferous gravel region. The miners worked out the ossuaries and, at the same time, undermined the village sites, and thousands of the native implements and utensils were introduced into the mines and became intermingled with the gravels. Implements and utensils may also have been introduced into the deep mines by their owners who were helpers in the mining work.

(10) When these objects began to be observed by the miners, individuals interested in relics commenced making collections, but neither miners nor collectors understood the need of discrimination, the fact that the objects came from the mines being to them satisfactory evidence that they belonged originally in the gravels.

(11) Again, it is possible that deception was often practiced. A mining camp is the natural home of practical joking, and the notion that finds of human relics in the gravels tended to excite heated discussion would spread quickly from camp to camp until the whole region would be affected.

(12) The testimony for antiquity is greatly weakened by the facts (1) that the finds on which it is based were made almost wholly by

inexpert observers, and (2) that all observations were recorded at second hand. Affidavits can not redeem it. Nothing short of abundant expert testimony will convince the critical mind that a Tertiary race of men using symmetrically shaped and beautiful implements, wearing necklaces of wampum and polished beads of marble or travertine bored accurately with revolving drills, and having a religious system so highly developed that at least two forms of ceremonial stones were specialized, could have occupied the American continent long enough to develop this marked degree of culture without leaving some really distinctive traces of its existence, something different from the ordinary belongings of our present Indian tribes.

EXPLANATION OF PLATE XVI.

View from "Cape Horn," on the Central Pacific Railway, near Colfax, California, looking down the valley of the North Fork of the American River.

This valley serves as an illustration of the vast erosion that has taken place since the period to which auriferous gravel man is assigned. The depth of the gorge at the base of the distant plateau-like ridge is 2,000 feet. The gold-bearing gravels, said to yield such plentiful remains of man, were laid down in the beds of Tertiary rivers that meandered the region before the present great valleys were conceived. Many of the ancient channels buried in Tertiary volcanic deposits have been explored for gold, and the tunnels follow the winding water courses through the very crests of the distant ridges seen in this picture.

PLATE XVI.

LOOKING DOWN NORTH FORK OF AMERICAN RIVER FROM CAPE HORN.

A PROBLEM IN AMERICAN ANTHROPOLOGY.[1]

By Frederic Ward Putnam.

While engaged in writing the address which I am to read to you this evening the sad news reached me of the death, on July 31, of our president of five years ago, Dr. D. G. Brinton. Although not unexpected, as his health had been failing since he was with us at the Boston meeting, where he took his always active part in the proceedings of Section H and gave his wise advice in our general council, yet his death affects me deeply. I was writing on a subject we had often discussed in an earnest but friendly manner. He believed in an all-pervading psychological influence upon man's development, and claimed that American art and culture were autochthonous, and that all resemblances to other parts of the world were the results of corresponding stages in the development of man; while I claimed that there were too many root coincidences, with variant branches, to be fully accounted for without also admitting the contact of peoples. Feeling his influence while writing, I had hoped that he would be present to-night, for I am certain that no one would have more readily joined with me in urging a suspension of judgment, while giving free expression to opinions, until the facts have been worked over anew and more knowledge attained.

His eloquent tongue is silent and his gifted pen is still, but I urge upon all who hear me to-night to read his two addresses before this association—one as vice-president of the anthropological section in 1887, published in our thirty-sixth volume of Proceedings; the other as retiring president in 1895, published in our forty-fourth volume. In these addresses he has, in his usual forcible and comprehensive manner, presented his views of American anthropological research and of the aims of anthropology.

Dr. Brinton was a man of great mental power and erudition. He was an extensive reader in many languages, and his retentive memory enabled him to quote readily from the works of others. He was a

[1] Address of the retiring president of the American Association for the Advancement of Science, given at Columbus on August 21, 1899. Printed in Science, August 25, 1899.

prolific writer, and an able critic of anthropological publications the world over. Doing little as a field archæologist himself, he kept informed of what was done by others through extensive travels and visits to museums. By his death American anthropology has suffered a serious loss, and a great scholar and earnest worker has been taken from our association.

In the year 1857 this association met for the first time beyond the borders of the United States, thus establishing its claim to the name "American" in the broadest sense. Already a member of a year's standing, it was with feelings of youthful pride that I recorded my name and entered the meeting in the hospitable city of Montreal; and it was on this occasion that my mind was awakened to new interests which in after years led me from the study of animals to that of man.

On Sunday, August 16, while strolling along the side of Mount Royal, I noticed the point of a bivalve shell protruding from roots of grass. Wondering why such a shell should be there and reaching to pick it up, I noticed, on detaching the grass roots about it, that there were many other whole and broken valves in close proximity—too many, I thought, and too near together to have been brought by birds, and too far away from water to be the remnants of a muskrat's dinner. Scratching away the grass and poking among the shells, I found a few bones of birds and fishes and small fragments of Indian pottery. Then it dawned upon me that here had been an Indian home in ancient times, and that these odds and ends were the refuse of the people—my first shell heap or kitchen midden, as I was to learn later. At the time this was to me simply the evidence of Indian occupation of the place in former times, as convincing as was the palisaded town of old Hochelaga to Cartier when he stood upon this same mountain side more than three centuries ago.

At that meeting of the association several papers were read, which, had there been a section of anthropology, would have led to discussions similar to those that have occurred during our recent meetings. Forty-two years later we are still disputing the evidence, furnished by craniology, by social institutions, and by language, in relation to the unity or diversity of the existing American tribes and their predecessors on this continent.

Those were the days when the theory of the unity of all American peoples, except the Eskimo, as set forth by Morton in his Crania Americana (1839), was discussed by naturalists. The volumes by Nott and Gliddon, Types of Mankind (1854), and Indigenous Races of the Earth (1857), which contains Meigs's learned and instructive dissertation, "The cranial characteristics of the races of men," were the works that stirred equally the minds of naturalists and of theologians regarding the unity or diversity of man—a question that could not

then be discussed with the equanimity with which it is now approached. The storm caused by Darwin's Origin of Species had not yet come to wash away old prejudices and clear the air for the calm discussion of theories and facts now permitted to all earnest investigators. Well do I remember when, during those stormy years, a most worthy bishop made a fervent appeal to his people to refrain from attending a meeting of the association, then being held in his city, on account of what he claimed to be the atheistic teachings of science. Yet ten years later this same venerable bishop stood before us, in that very city, and invoked God's blessing upon the noble work of the searchers for truth.

At the meeting of 1857 one of our early presidents, the honored Dana, read his paper, entitled "Thoughts on species," in which he described a species as "a specific amount or condition of concentrated force defined in the act or law of creation," and, applying this principle, determined the unity of man in the following words:

"We have, therefore, reason to believe, from man's fertile intermixture, that he is one in species, and that all organic species are divine appointments which can not be obliterated unless by annihilating the individuals representing the species."

Another paper was by Daniel Wilson, recently from Scotland, where six years before he had coined that most useful word, "prehistoric," using the term in the title of his volume, Prehistoric Annals of Scotland. In his paper Professor (afterwards Sir Daniel) Wilson controverted the statement of Morton that there was a single form of skull for all American peoples, north and south, always excepting the Eskimo. After referring to the views of Agassiz, as set forth in the volumes of Nott and Gliddon, he said:

"Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Tierra del Fuego to the Arctic Circle, was first propounded by Dr. Morton it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race, and to prove that, notwithstanding some partial deviations from the prevailing standard, the American Indian is essentially separate and peculiar—*a race distinct from all others.* The stronghold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents is the supposed uniformity of physiological and especially of physiognomical and cranial characteristics—an ethnical postulate which has not yet been called in question."

After a detailed discussion of a number of Indian crania from Canada and a comparison with those from other parts of America, as described by Morton, Wilson makes the following statements:

"But making full allowance for such external influences, it seems to me, after thus reviewing the evidence on which the assumed unity of

the American race is formed, a little less extravagant to affirm of Europe than of America that the crania everywhere and at all periods have conformed, or even approximated, to one type.

"As an hypothesis, based on evidence accumulated in the Crania Americana, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the northern continent, the conclusions still go far beyond anything established by absolute proof; and the subsequent labors of Morton himself, and still more of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing by further evidence a novel and ingenious hypothesis."

At the close of this instructive paper are the following words:

"If these conclusions, deduced from an examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed: That a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi; and in so far as the northern differ from the southern tribes they approximate more or less, in the points of divergence, to the characteristics of the Eskimo, that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent ethnologists to be physically a Mongol and Asiatic, if philologically an American."

The third paper of the meeting to which I shall refer was by another of our former presidents, the then well-known student of Indian institutions and the author of the League of the Iroquois (1851). In this paper on "The laws of descent of the Iroquois," Morgan discusses the league as made up of five nations, each of which was subdivided into tribes, and he explains the law of marriage among the tribes, the family relationship, and the descent in the female line as essential to the maintenace of the whole system. He then says:

"Now the institutions of all the aboriginal races of this continent have a family cast. They bear internal evidence of a common paternity, and point to a common origin, but remote, both as to time and place. That they all sprang from a common mind, and in their progressive development have still retained the impress of original elements, is abundantly verified. The Aztecs were thoroughly and essentially Indian. We have glimpses here and there at original institutions which suggest at once, by their sir[illegible]dred ones among the Iroquois and other Indian races of th[illegible]. Their intellectual characteristics, and the predominar[illegible]eir so[illegible] dition, are such as to leave no doubt upon [illegible]d the results of modern research upon this [illegible]t

clusion. Differences existed, it is true, but they were not radical. The Aztec civilization simply exhibited a more advanced development of those primary ideas of civil and social life which were common to the whole Indian family, and not their overthrow by the substitution of antagonistic institutions."

After calling attention to the fact that a similar condition exists among certain peoples of the Pacific Islands, he writes:

"Whether this code of descent came out of Asia or originated upon this continent is one of the questions incapable of proof; and it must rest, for its solution, upon the weight of evidence or upon probable induction. Its existence among American races whose languages are radically different, and without any traditional knowledge among them of its origin, indicates a very ancient introduction, and would seem to point to Asia as the birthplace of the system."

It would be interesting to follow the succeeding meetings of the association and note the recurring presentation of views which the quotations I have given show to have been most seriously discussed over a generation ago. An historical review of the literature of American anthropology during the present century would also be interesting in this connection. It is probable, however, that a review of this literature for the first half of the century would reveal the fact that the writers, with here and there a notable exception, were inclined to theorize upon insufficient data and devoted little time to the accumulation of trustworthy facts. The presentation and discussion of carefully observed facts can almost be said to have begun with the second half of the century, and this is the only part of the subject that now commands serious attention.

A reference to the very latest résumé of this subject, as presented in the History of the New World called America, by Edward J. Payne, Vol. II., Oxford, 1899, is instructive here. In this volume Mr. Payne expresses his belief in the antiquity and unity of the American tribes, which he considers came from Asia in preglacial and glacial times, when the northwestern corner of America was connected with Asia, and when man "as yet was distinguished from the inferior animals only by some painful and strenuous form of articulate speech and the possession of rude stone weapons and implements, and a knowledge of the art of fire kindling. Such, it may be supposed, were the conditions under which man inhabited both the Old and the New World in the paleo-ethnic age. * * * Even when a geological change had separated them [the continents] some intercourse by sea was perhaps maintained—an intercourse which became less and less, until the American branch of humanity became practically an isolated race, as America itself has become an isolated continent." (Preface.)

Mr. Payne discusses the growth of the languages of America, the various social institutions and arts, and the migrations of these early savages over the continent, north and south, during the many centuries

following, as one group after another grew in culture. He considers all culture of the people autochthonous.

"It may, however, be suggested that, as in the Old World, the earlier and the smaller tribes tend to dolichocephaly, while the better developed ones are rather brachycephalous—a conclusion indicating that the varying proportions of the skull should be taken less as original evidence of race than as evidence of physical improvement."

This volume by Mr. Payne is replete with similar statements of facts and theories, and shows how difficult it is for us to understand the complications of the subject before us. It can not be denied that, taking into consideration the number of authors who have written on this subject, Mr. Payne is well supported in his theory of the autochthonous origin of all American languages, institutions, and arts; but the question arises: Has not the old theory of Morton, the industrious and painstaking pioneer of American craniology, been the underlying cause of this, and have not the facts been misinterpreted? At the time of Morton the accepted belief in the unity and universal brotherhood of man was about to be assailed, and it seems, as we now look back upon those times of exciting and passionate discussions, that Morton may have been influenced by the new theory which was so soon to become prominent—that there were several distinct creations of species of the genus *Homo* and that each continent or great area had its own distinct fauna and flora. Certainly Morton ventured to make a specific statement from a collection of crania which would now be regarded as too limited to furnish true results.

The anthropologist of to-day would hardly venture to do more than to make the most general statements of the characters of any race or people from the examination of a single skull; although, after the study of a large number of skulls from a single tribe or special locality, he would probably be able to select one that was distinctly characteristic of the special tribe or group to which it pertained.

Relatively long and narrow heads and short and broad heads occur almost everywhere in greater or less proportion. In determining the physical characters of a people, so far as this can be done from a study of crania, the index of the height of the skull is quite as important as that of its breadth. These indices simply give us the ready means of expressing by figures the relative height and breadth of one skull in comparison with another, a small part of what the zoologist would consider in describing, for instance, the skulls of the different species of the genus *Homo*. So in our craniological studies we should determine the relative position, shape, and proportions of the different elements of the skull. In fact, we should approach the study of human crania with the methods of the zoologist, and should use tables of figures only so far as such tables give us the means of making exact comparisons. Here, again, are the anthropologists at a disadvantage,

inasmuch as it is only very recently that we are approaching a standard of uniformity in these expressions. It is now more than ever essential that the anthropologists should agree upon a method of expressing certain observed facts in somatology, so that the conscientious labors of an investigator who has had a special opportunity for working upon one group of man may be made available for comparison by investigators of other groups.

Probably the old method, still largely in vogue, of stating averages is responsible for many wrong deductions. If we take 100 or more skulls of any people, we shall find that the two extremes of the series differ to a considerable extent from those which naturally fall into the center of the series. These extremes, in the hands of a zoologist, would be considered the subvarieties of the central group or variety. So in anthropology we should take the central group of the series as furnishing the true characters of the particular variety or group of man under consideration, and should regard the extremes as those which have been modified by various causes. It may be said that this central group is defined by stating the mean of all the characters, but this is hardly the case, for by giving the mean of all we include such extraneous characters as may have been derived by admixture or from abnormal conditions.

The many differing characteristics exhibited in a large collection of crania brought together from various portions of America, North and South, it seems to me, are reducible to several great groups. These may be generally classed as the Eskimo type, the northern and central or so-called Indian type, the northwestern brachycephalic type, the southwestern dolichocephalic type, the Toltecan brachycephalic type, and the Antillean type, with probably the ancient Brazilian, the Fuegian, and the pre-Inca types of South America. Each of these types is found in its purity in a certain limited region, while in other regions it is more or less modified by admixture. Thus the Toltecan, or ancient Mexican, type (which, united with the Peruvian, was separated as the Toltecan family even by Morton) occurs, more or less modified by admixture, in the ancient and modern pueblos and in the ancient earthworks of our central and southern valleys. In Peru, more in modern than in ancient times, there is an admixture of two principal types. At the north of the continent we again find certain traits that possibly indicate a mixture of the Eskimo with the early coast peoples both on the Pacific and on the Atlantic sides of the continent. The north-central Indian type seems to have extended across the continent and to have branched in all directions, while a similar but not so extensive branching, northeast and south, seems to have been the course of the Toltecan type.

This is not theorizing upon the same facts from which Morton drew the conclusion that all these types were really one and the same.

Since Morton's time we have had large collections of crania for study, and the crania have been correlated with other parts of the skeleton and with the arts and institutions of the various peoples.

Although these relations have been differently interpreted by many anthropologists who have treated the subject, yet to me they seem to indicate that the American continent has been peopled at different times and from various sources; that in the great lapse of time since the different immigrants reached the continent there has been in many places an admixture of the several stocks and a modification of the arts and customs of all; while natural environment has had a great influence upon the ethnic development of each group. Furthermore, contact of one group with another has done much to unify certain customs; while "survivals" have played an active part in the adoption and perpetuation of arts and customs not native to the people by whom they are preserved.

The Inca civilization, a forcible one coming from the north, encroached upon that of the earlier people of the vicinity of Lake Titicaca, whose arts and customs were, to a considerable extent, adopted by the invaders. It is of interest here to note the resemblance of the older Andean art with that of the early Mediterranean, to which it seemingly has a closer resemblance than to any art on the American continent. Can it be that we have here an æsthetic survival among this early people, and could they have come across the Atlantic from that Eurafric region which has been the birthplace of many nations? Or is this simply one of those psychical coincidences, as some writers would have us believe? The customs and beliefs of the Incas point to a northern origin and have so many resemblances to those of the ancient Mexicans as hardly to admit of a doubt that in early times there was a close relation between these two widely separated centers of ancient American culture. But how did that pre-Inca people reach the lake region? Is it not probable that some phase of this ancient culture may have reached the Andes from northern Africa? Let us consider this question in relation to the islands of the Atlantic. The Canary Islands, as well as the West Indies, had long been peopled when first known to history; the Caribs were on the northern coast of South America, as well as on the islands; and in the time of Columbus native trading boats came from Yucatan to Cuba. We thus have evidence of the early navigation of both sides of the Atlantic, and certainly the ocean between could easily have been crossed.

One of the most interesting as well as most puzzling of the many phases of American archæology is the remarkable development of the art of the brachycephalic peoples, extending from northern Mexico northeastward to the Mississippi and Ohio valleys, then disappearing gradually as we approach the Alleghenies and, farther south, the Atlantic coast, also spreading southward from Mexico to Honduras,

and changing and vanishing in South America. Unquestionably of very great antiquity, this art, developed in the neolithic period of culture, reached to the age of metals, and had already begun to decline at the time of the Spanish conquest. How this remarkable development came to exist amid its different environments we can not yet fully understand; but the question arises: Was it of autochthonous origin and due to a particular period in man's development, or was it a previously existing phase modified by new environment? For the present this question should be held in abeyance. To declare that the resemblance of this art to both Asiatic and Egyptian art is simply a proof of the psychical unity of man is assuming too much and is cutting off all further consideration of the subject.

The active field and museum archæologist who knows and maintains the association of specimens as found, and who arranges them in their geographical sequence, becomes intimately in touch with man's work under different phases of existence. Fully realizing that the natural working of the human mind under similar conditions will to a certain extent give uniform results, he has before him so many instances of the transmission of arts, symbolic expressions, customs, beliefs, myths, and languages that he is forced to consider the lines of contact and migration of peoples as well as their psychical resemblances.

It must be admitted that there are important considerations, both physical and mental, that seem to prove a close affinity between the brown type of eastern Asia and the ancient Mexicans. Admitting this affinity, the question arises: Could there have been a migration eastward across the Pacific in neolithic times, or should we look for this brown type as originating in the Eurafric region and passing on to Asia from America? This latter theory can not be considered as a baseless suggestion when the views of several distinguished anthropologists are given the consideration which is due to them. On the other hand, the theory of an early migration from Asia to America may also be applied to neolithic time.

However this may have been, what interests us more at this time, and in this part of the country, is the so-called "Mound Builder" of the Ohio Valley. Let us first clear away the mist which has so long prevented an understanding of this subject by discarding the term "Mound Builder." Many peoples in America, as well as on other continents, have built mounds over their dead, to mark important sites and great events. It is thus evident that a term so generally applied is of no value as a scientific designation. In North America the term has been applied even to refuse piles. The kitchen middens or shell heaps which are so numerous along our coasts and rivers have been classed as the work of the "Mound Builder." Many of these shell heaps are of great antiquity, and we know that they are formed of the refuse gathered on the sites of the early peoples.

From the time of these very early deposits to the present such refuse piles have been made, and many of the sites were reoccupied, sometimes even by a different people. These shell heaps, therefore, can not be regarded as the work of one people. The same may be said in regard to the mounds of earth and of stone so widely distributed over the country. Many of these are of great antiquity, while others were made within the historic period and even during the first half of the present century. Some mounds cover large collections of human bones; others are monuments over the graves of noted chiefs; others are in the form of effigies of animals and of man; and, in the South, mounds were in use in early historic times as the sites of ceremonial or other important buildings. Thus it will be seen that the earth mounds, like the shell mounds, were made by many peoples and at various times.

There are, however, many groups of earthworks which, although usually classed as mounds, are of an entirely different order of structure and must be considered by themselves. To this class belong the great embankments, often in the form of squares, octagons, ovals, and circles, and the fortifications and singular structures on hills and plateaus, which are in marked contrast to the ordinary conical mounds. Such are the Newark, Liberty, Highbank, and Marietta groups of earthworks, the Turner group, the Clark or Hopewell group, and many others in Ohio and in the regions generally south and west of these great central settlements; also, the Cahokia Mound opposite St. Louis, the Serpent Mound of Adams County, the great embankments known as Fort Ancient, which you are to visit within a few days, the truly wonderful work of stone known as Fort Hill in Highland County, and the strange and puzzling walls of stone and cinder near Fosters Station.

So far as these older earthworks have been carefully investigated, they have proved to be of very considerable antiquity. This is shown by the formation of a foot or more of vegetable humus upon their steep sides, by the forest growth upon them, which is often of primeval character, and by the probability that many of these works, covering hundreds of acres, were planned and built upon the river terraces before the growth of the virgin forest.

If all mounds of shell, earth or stone, fortifications on hills, or places of religious and ceremonial rites, are classed irrespective of their structure, contents, or time of formation, as the work of one people, and that people is designated "the American Indian" or the "American race," and considered to be the only people ever inhabiting America, North and South, we are simply repeating what was done by Morton in relation to the crania of America—not giving fair consideration to differences while overestimating resemblances. The effort to affirm that all the various peoples of America are of one race

has this very year come up anew in the proposition to provide "a name which shall be brief and expressive" and at the same time shall fasten upon us the theory of unity—notwithstanding the facts show diversity—of race.

Let us now return to the builders of the older earthworks, and consider the possibility of their having been an offshoot of the ancient Mexicans. Of the crania from the most ancient earthworks we as yet know so little that we can only say that their affinities are with the Toltecan type; but of the character of the art, and particularly the symbolism expressing the religious thought of the people, we can find the meaning only by turning to ancient Mexico. What Northern or Eastern Indian ever made or can understand the meaning of such sculptures or such incised designs as have been found in several of the ancient ceremonial mounds connected with the great earthworks? What Indian tribe has ever made similar carved designs on human and other bones, or such singular figures, cut out of copper and mica, as were found in the Turner and Hopewell groups? Or such symbolic animal forms elaborately carved in stone, and such perfect terra-cotta figures of men and women as were found on the sacrificial altars of the Turner group? What meaning can be given to the Cincinnati Tablet, or to the designs on copper plates and shell disks from some of the southern and western burial and ceremonial mounds? I think we shall search in vain for the meaning of these many objects in the North or East, or for much that resembles them in the burial places of those regions. On the other hand, most of these become intelligible when we compare the designs and symbols with those of the ancient Mexican and Central American peoples. The Cincinnati Tablet, which has been under discussion for over half a century, can be interpreted and its dual serpent characters understood by comparing it with the great double image known in Mexico as the Goddess of Death and the God of War. The elaborately complicated designs on copper plates, on shell disks, on human bones and on the wing bones of the eagle can in many instances be interpreted by comparison with Mexican carvings and with Mexican modes of symbolic expression of sacred objects and religious ideas. The symbolic animals carved on bone or in stone and the perfection of the terra-cotta figures point to the same source for the origin of the art.

In connection with the art of the builders let us consider the earth structures themselves. The great mound at Cahokia, with its several platforms, is only a reduction of its prototype at Cholula. The fortified hills have their counterparts in Mexico. The serpent effigy is the symbolic serpent of Mexico and Central America. The practice of cremation and the existence of altars for ceremonial sacrifices strongly suggest ancient Mexican rites. We must also recall that we have a connecting link in the ancient pueblos of our own Southwest, and that

there is some evidence that in our Southern States, in comparatively recent times, there were a few remnants of this old people. It seems to me, therefore, that we must regard the culture of the builders of the ancient earthworks as one and the same with that of ancient Mexico, although modified by environment.

Our Northern and Eastern tribes came in contact with this people when they pushed their way southward and westward, and many arts and customs were doubtless adopted by the invaders, as shown by customs still lingering among some of our Indian tribes. It is this absorption and admixture of the peoples that has in the course of thousands of years brought all our American peoples into a certain conformity. This does not, however, prove a unity of race.

It is convenient to group the living tribes by their languages. The existence of more than a hundred and fifty different languages in America, however, does not prove a common origin, but rather a diversity of origin as well as a great antiquity of man in America.

That man was on the American continent in quaternary times, and possibly still earlier, seems to me as certain as that he was on the Old World during the same period. The Calaveras skull, that bone of contention, is not the only evidence of his early occupation of the Pacific coast. On the Atlantic side the recent extensive explorations of the glacial and immediately following deposits at Trenton are confirmatory of the occupation of the Delaware Valley during the closing centuries of the glacial period and possibly also of the interglacial time. The discoveries in Ohio, in Florida, and in various parts of Central and South America all go to prove man's antiquity in America. Admitting the great antiquity of one or more of the early groups of man on the continent, and that he spread widely over it while in the palæolithic and early neolithic stages of culture, I can not see any reason for doubting that there were also later accessions during neolithic times and even when social institutions were well advanced. While these culture epochs mark certain phases in the development of a people, they can not be considered as marking special periods of time. In America we certainly do not find that correlation with the Old World periods which we are so wont to take for granted.

We have now reached the epoch of careful and thorough exploration and of conscientious arrangement of collections in our scientific museums. It is no longer considered sacrilegious to exhibit skulls, skeletons, and mummies in connection with the works of the same peoples. Museums devoted primarily to the education of the public in the æsthetic arts are clearing their cases of heterogeneous collections of ethnological and archæological objects. Museums of natural history are being arranged to show the history and distribution of animal and vegetable life and the structure of the earth itself. Anthropological museums should be similarly arranged and, with certain

gaps, which every curator hopes to fill, they should show the life and history of man. To this end the conscientious curator will avoid the expression of special theories, and will endeavor to present the true status of each tribe or group of man in the past and in the present, so far as the material at his command permits. A strictly geographical arrangement is, therefore, the primary principle which should govern the exhibition of anthropological collections. A special exhibit may be made in order to illustrate certain methods by which man in different regions has attained similar results, either by contact or by natural means. Another exhibit may be for the purpose of showing the distribution of corresponding implements over different geographical areas. These and similar special exhibits are instructive, and under proper restrictions should be made, but unless the design of each exhibit is clearly explained, the average visitor to a museum will be confused and misled, for such objects so grouped convey a different impression than when exhibited with their associated objects in proper geographical sequence.

The anthropology of America is now being investigated, and the results are being made known through museums and publications as never before.

The thoroughly equipped Jesup North Pacific expedition, with well-trained anthropologists in charge, was organized for the purpose of obtaining material, both ethnological and archæological, for a comparative study of the peoples of the northern parts of America and Asia. Although only in the third year of its active field work, it has already furnished most important results and provided a mass of invaluable authentic material.

The Hyde expedition, planned for long-continued research in the archæology and ethnology of the southwest, a successor in regard to its objects to the important Hemenway expedition, is annually adding chapters to the story of the peoples of the ancient pueblos.

The results of the extensive explorations by Moore of the mounds of the southern Atlantic coast are being published in a series of important monographs.

The Pepper-Hurst expedition to the Florida Keys has given information of remarkable interest and importance from a rich archæological field before unknown.

The United States Government, through the Bureau of Ethnology of the Smithsonian Institution, has given official and liberal support to archæological and ethnological investigations in America.

The constantly increasing patronage by wealthy men and women of archæological research at home, as well as in foreign lands, is most encouraging.

The explorations in Mexico and in Central and South America, the publication in facsimile of the ancient Mexican and Maya codices,

the reproduction by casts of the important American sculptures and hieroglyphic tablets, all have been made possible by earnest students and generous patrons of American research.

The numerous expeditions, explorations, and publications of the Smithsonian Institution and of the museums of Washington, Chicago, Philadelphia, New York, and Cambridge, are providing the student of to-day with a vast amount of authentic material for research in American and comparative anthropology.

The Archæological Institute of America, the American Folk-Lore Society, and the archæological and anthropological societies and clubs in active operation in various parts of the country, together with the several journals devoted to different branches of anthropology, give evidence of widespread interest.

Universities are establishing special courses in anthropology, and teachers and investigators are being trained. Officers of anthropological museums are preparing men to be field workers and museum assistants. The public need no longer be deceived by accounts of giants and other wonderful discoveries. The wares of the mercenary collector are now at a discount since unauthentic material is worthless.

Anthropology is now a well-established science; and with all this wealth of materials and opportunities, there can be no doubt that in time the anthropologists will be able to solve that problem, which for the past half century has been discussed in this association—the problem of the unity or diversity of prehistoric man in America.

ON SEA CHARTS FORMERLY USED IN THE MARSHALL ISLANDS, WITH NOTICES ON THE NAVIGATION OF THESE ISLANDERS IN GENERAL.[1]

By Captain Winkler,
Of the German Navy.

In July, 1896, I was stationed for a short time in Jaluit, on the Marshall Islands, during the annual circuit of inspection. While there I received from Dr. Irmer, royal inspector of lands, among other things, two sea charts of the Marshall islanders, made of a number of sticks lashed together in a rude latticework, and on this at various points were tied small shells. Dr. Irmer confessed that he was unable to explain the meaning and function of the charts, for great secrecy was preserved among the islanders on this score and only a few of the old chiefs, indeed, were in possession of the secret. He had sought to secure their interpretation in his official capacity, but to no purpose. He laid it on my conscience, since ethnologists are greatly interested in such matters, and since a thorough explanation of the charts had not been made, to try my skill therein, and he promised to bring all his influence to bear on my behalf to this end.

The chief, Lojak, who was one of the most skillful local pilots, was induced to give me his interpretations, which Dr. Irmer's native servant, Ladjur, would interpret. One forenoon an impressive scene was enacted in Dr. Irmer's quarters, when Lojak, with the greatest secrecy, first closed all the windows, in spite of the 34° C. heat, having threatened Ladjur with death if he divulged the tabooed mystery; but the result of the long sweat bath was a complete negative. From other persons on the archipelago I gathered what they had learned concerning the interpretation of the charts, to the effect that the mussels on them indicated the islands and that the sticks represented the currents, that the natives knew these currents, and that on a journey one man from the bow of the canoe looked over the water and in the easiest manner, by the water indications and the chart, directed his course.

[1] Translated from Marine-Rundschau, Berlin, 1898, pt. 10, pp. 1418–1439, with plates from the United States National Museum and other collections.

All my objections that the current can not be seen in open water and all my cross-questionings to secure a more reasonable explanation availed nothing, so that I had to content myself with this, coming to the conclusion that Marshall islanders must possess a sixth sense, lacking in us, which enabled them to perceive more than we. As I afterwards found out, this misunderstanding was altogether due to false interpretation, coupled with my own limited experience in following the thought and expressions of the natives.

Both charts were hung in my cabin, and the next year, during my stay in the South Sea, Australia, and New Zealand, because of their construction, they formed the theme of many a conversation with my visitors, especially English naval officers and gentlemen in Sydney and New Zealand familiar with the Pacific Ocean. The same testimony came from all, that no one could tell the use of the charts, but the greatest interest was shown and a desire to know more about them.

In 1897, shortly before I made a second cruise to the Marshall Islands, I was interested to meet in Samoa the explorer, Dr. Benedict Friedlander, who begged me, when convenient, to seek an explanation of the mysterious charts, saying that the Polynesian Society, of which he was a member, would lay great stress upon the investigation. Dr. Friedlander also gave me a drawing of one of the two charts now in my possession, which had been illustrated in the Polynesian Society's Journal, with the request to seek the decipherment of the lines thereon. For explanation of the chart there was merely the assertion that they were a means "to teach the youth the direction of the currents."

So I determined to do my best on my second cruise, and I believe that, favored by fortune, since I had the kind assistance of two officials as interpreters, I made out a tolerably correct explanation, which I will now set forth. The publication of my results was made in the Marine Rundschau, in order to render them accessible to all my comrades who might have the opportunity to study more extensively in order to come to a complete solution of the problem. If something has been already attained herein, still there must be haste. The Marshall islanders now make their longer journeys only in European-buit schooners, with the aid of a compass, using charts of the archipelago issued by us, and prefer the patent log. The employment of the old charts was only little known, and they are no longer studied, so that, in fact, on the islands, no further information about the use of the charts is to be had.

In order to give the greatest possible number of hints to those who wish to pursue the subject further, I shall here report the sources from which I have obtained my information and the names of those natives from whom perhaps something more may be gained.

In the second cruise of His Majesty's ship *Bussard*, to Jaluit, in

MARSHALL ISLANDS CANOE.

Collection of Rev. C. F. Rife.

November, 1897, I observed lying at anchor the German schooner *Neptune*, Captain Kessler. He had already spent a decade in the Marshall and Gilbert groups, was quite familiar with the native languages, and friendly with the chiefs, with one of whom, Nelu, he was in fraternal relations. Now I had someone who could help me. Captain Kessler, who had no accurate knowledge concerning the charts, nevertheless showed the greatest willingness and interest and promised hearty cooperation.

Then began a strenuous, monotonous, and patient research. Chief Nelu, who did not wish to conceal aught from his brother Kessler, was first pumped. He told us all that he knew, and gave us pleasure with his willingness, but when, in the evening, I collected all that had been heard and noted down and tried to put it into form I found so many contradictions that pretty much all that had been written had to be crossed out. We came to the conclusion that Nelu was not sufficiently trained, and through incessant drinking of beer, which furnished his sole nourishment, had become too stupid to be able to render a clear explanation.

It was now to look up Chief Lojak, who at first was not willing to speak out plainly, but when he heard that Nelu had told us all he knew, was more cordial and willing to answer questions. Here also great patience was demanded. These hour-long sessions and squeezings were not to the liking of the king, as he called himself. It was not easy for him to express himself correctly, and frequently we had to interrupt our sessions when his confusion became uncomfortable.

Once Lojak told me with seeming frankness that I was the dumbest churl he had ever seen; daily he told me the same, and that every day I came again with the same stupid questions; generally he would have no more to say to me, and only a glass of sack, which the old man loved, would make him friendly again.

As an extreme measure, I had hanging in my cabin a showy uniform coat which I promised Lojak if he would answer all my questions. The hint had its effect, for another chief frequently in company with Lojak, named Kabua, had before that received from a commandant such a garment, in which he, much to Lojak's envy, had appeared on festive occasions. To have in sight a better coat seemed to him a piece of the best luck that could happen.

In gathering help from all sides, we came upon another good leader, though there stood many wide gaps in our knowledge; for now fortune gave me an exceptional help in the person of a half-breed named Joachim de Brun, called Jochem, who came to Jaluit once in a while to consult the resident physician. He was a son of a Portuguese in Likieb, who built a schooner for the chief there.

Jochem was an intelligent man, spoke English and the Marshall

language, knew well the islanders, among whom he grew up, and was also a good sailor. After Jochem had taken part in our conferences, he remarked that Lojak did not get the matter quite straight, and did not understand it all correctly; he did not say the same thing every day; he was not competent to do that, because his chief assistant in sailing was always a native of lower rank, called Laumanuan, who was now living with Lojak. It did not seem expedient for the present to use him to correct his chief when he was in error, but Jochem could privately learn from him afterwards, especially at home, the correct version. In this inquiry, when things were not clear, I had a conference with Lojak in his hut while Jochem and Laumanuan remained outside. At last, in this way, we succeeded in clearing up the greater part of the doubtful points and securing the interpretation of Charts I–IV, as well as the meaning of their symbols. Moreover, we obtained, through our efforts with Lojak, a new chart, numbered V, from Chief Langenat, of the island Mille, in the Ratak chain, who was at that time a guest of Lojak's.

As reported to me, the other chiefs understood little more of the ancient lore, only Chief Muridjil, in the northern part of the Ratak chain, had some reputation as an old sailor. From Jochem's statement, this man's knowledge would turn out not much better than Lojak's. Muridjil had a native named Burido as assistant and right-hand man, who was required to be versed in the sailor's art. In Jaluit were also Chiefs Kabua, Litokwa, and Launa, who were skillful men, but Kabua was not there on my second visit. Litokwa and Launa knew less than Lojak about interpreting the charts.

General results have been enlarged for me in a valuable manner by an aged man, Mr. Capelle, in Jaluit, known under the name of "the old gentleman," a merchant living there, who had been already more than thirty years on the islands, and formerly was one of the best informed settlers in the South Sea. Misfortune had overtaken him in business, but now he was getting on his feet again. Mr. Capelle had, during his entire sojourn in the South Sea, kept a diary, from which he had given me notes on the subject in hand, and out of which, when it was properly classified, was furnished other interesting material.

In the same manner as to Captain Kessler and Joachim de Brun, I owe also to the imperial magistrate, Herr Senfft; to the president of the Jaluit society, Herr Huetler, and to other gentlemen of Jaluit many thanks for their substantial help in my work, both through their own efforts and their influence with the natives.

The so-called charts do not deserve the name in our sense, but they merely serve to bring to view the water condition, as well for the instruction of the chief's sons, who have to be initiated into the secrets of navigation, as for the settling of differences between chiefs piloting

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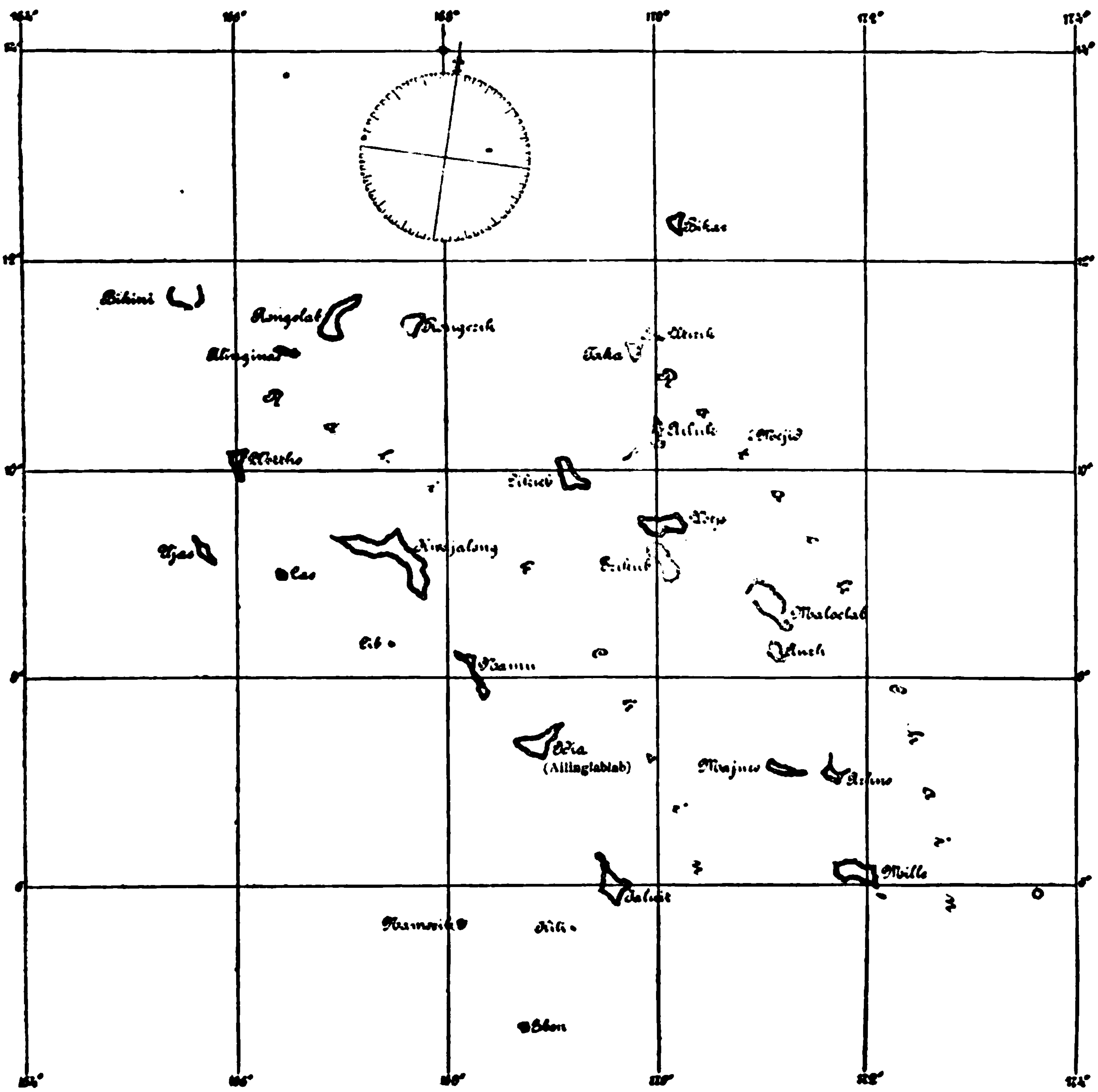
a boat when the water indications are not plain and varying interpretations have been made. Only in one chart, illustrated here as Chart II, in the first line, can the geographic positions of the islands be made out.

As already said, the charts consist of a system of little sticks tied together with shells fastened on them. The mussels represent neither determined nor undetermined islands. The sticks are designed chiefly



Map of Marshall Islands.

to bring to view the direction of the principal Dunungs[1] (not the currents, as was formerly explained to me, erroneously), the course of

[1]Captain Winkler uses the word Dünung (plural Dünungen) for the special water conditions noticed by the chiefs. As there is no English equivalent I have anglicised the term, and will use dunung and dunungs as equivalents. These dunungs evidently mean the great swells as they adapt themselves to the configuration of the islands. Dr. Bastian suggests that the rippling of the water on the side of the canoe assists in the interpretation.—TRANSLATOR.

these in their contact with the islands, and they are used in discussions arising concerning the crossings of the different dunungs, which furnish the principal guides for navigation. Moreover, the several sticks indicate the visible distances of the islands as well as some other lore useful in sailing. All these will be made clear in describing the charts. Before proceeding to the descriptions, it is necessary previously to give the meaning of certain native terms, since I shall, in describing the charts, repeat without translation only the Marshall Island expressions as they were delivered to me by the chiefs. In my interviews with them, in order to eliminate errors, the terms under consideration were always written with pencil on the chart when they were not involved in the general explanation. I have inserted the necessary verification in the proper place.[1]

By more careful scientific study some of the explanations, which did not at first appear tenable to me, may seem open to dispute. I must, therefore, emphasize the fact that I here give no theories that I have adopted and will eventually maintain, but repeat only the explanations furnished me, as the Marshall islanders themselves have laid them down, and as they answer to the conceptions held by them, and whose correctness I had no opportunity to control. It is, therefore, not unlikely that through wrong explanation or interpretation, in spite of the greatest caution and of the continual proving of what was heard, some errors may have crept into the returns. These I hope

[1] Capt. Joshua Slocum, who circumnavigated the globe alone in his little sloop, the *Spray*, furnishes the following account of the "dunungen" and the charts.

"The Marshall Island charts that I have seen consist of a frame of wood with strings stretched across from side to side. The strings, I understood, represent, one set, the mean direction of the trade winds, and the other set the waves or swells at right angles to the wind. Shells strung at various crossings represent islands, vaguely, in position of the lands known to have been reached by canoe, sailing in certain angles across the wind and waves or swells. During the season of the year when the trade winds sweep over their islands, it requires only the natural skill of a seaman to navigate in this way from one archipelago to another. And even if the direction of the trades varies considerably, the savage islanders know, by natural signs, when they do so, and how much to allow for the variation, as well as do the birds that come home to roost. It goes well with the canoemen usually if they are favored by a clear run, but a little dead reckoning or beating about confuses them.

"I rescued a party of Gilbert islanders some years ago. They had maps on a small scale and quite useless, translated, I should say, from Morse's Geography. On their charts their country was called Buckaroovoo, and Japan was Taiban. A thunder storm had driven them out of their course and they did not seem to know how to recover the lost ground by clawing to windward, so they were drifting hopelessly about the ocean, 600 miles out of their course, when my ship ran onto them and was instrumental in their return to Buckaroovoo.

"In the matter of the sixth sense, which folk-sailors seem to possess, I am reminded of Captain McKinnon, who thought nothing of a sail through fog without a compass, from port to port, depending on the direction of the wind for his courses. The wind's direction he judged by the density of the fog. He said that the compass bothered him, and as for charts, he carried them all in his head."—Note to the Translator.

will be discovered and corrected by those who are interested in the data here given, and who have more leisure than I can command.

The native words to be used are:

1. *Rilib*, in English, "backbone." Rilib signifies the eastward dunung. The line of direction for this dunung is indicated by a curve, with the explanation that in coming against an island the dunung is held up, arrested, through the heaping up of the water at a certain distance from the island, whereby the swell beyond would be forced or curved inward. The Rilib is quite worthy of note all the year round; it is in the Marshall group the strongest dunung.

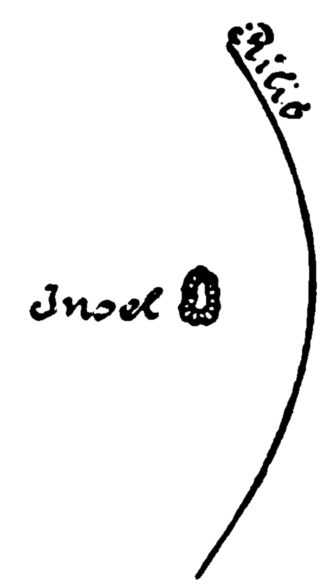


FIG. 1.—End of Rilib.

2. *Kaelib.* Kaelib is the name for the western dunung. It is to be seen throughout the whole year, but is not so strong as the Rilib. Unpracticed persons are able to detect the Kaelib only with the greatest difficulty.

3. *Bungdockerik*, "coming from the south." This term refers, as the name signifies, to the southern dunung, which arises in the southwest passage region. It is also to be observed throughout the whole year, and is quite as strong as the Rilib, especially in the southern part of the group.

4. *Bungdockeing*, "from the north." Bungdockeing is the northern dunung, having its origin from the ocean, and exhibiting itself most strongly in the northern part of the Marshall group.

5. *Boot*, "a knot, or node." Boot signifies the place of the knot, or the nodal point at which the dunung swells, diverted by an island, cross one another. Near every island, also, there exists a continuous series of Boots, which play a most important rôle in sailing.

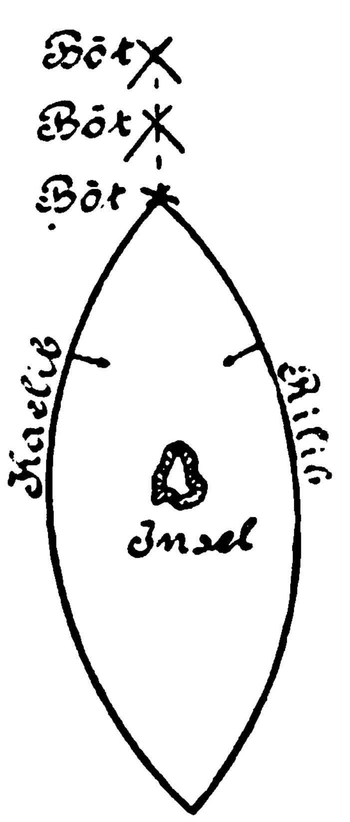


FIG. 2.—End of Kaelib.

6. *Okar*, "root." Here equivalent to saying: As the root, if you follow it, leads to the palm tree, so does this lead to the island. Okar is the continued series of Boots. When you have found the first Boot, then you get to the island by following the Okar.

7. *Rolok*, "something lost." Here with the meaning that you are out of your course. The term is applied to the remarkable dunung on the western side of the island, extending from the northern angle to the northwest, arising from the breakers of the Rilib. When one gets therein, he perceives that he has missed the island and is trending to the northwest.

8. *Nit in kot*, "a hole." Signifying a cage or trap in which birds are kept, with the meaning here that the navigator is in a cul-de-sac, and must go back. Nit in kot corresponds to the Rolok running from the southern point of the island to the southwest.

9. *Jur in okme*, "stakes." Meaning here that there is an obstruction in the way. They call Jur in okme the dunung arising out of

the breakers of the Kaelib, running from the north and the south point of the islands to the northeast and southeast.

10. *Aï*, "foam." They call the different observation distances from an island "Aï," for example: Djellad-aï.

(a) *Djellad-aï* means the distance at which palm trees may be seen from the mast of a canoe, reckoned at about 10 nautical miles.

(b) *Eged-aï* is the distance at which the island may be seen from the canoe, about 15 nautical miles.

(c) *Djuq-aï* is the distance at which land is no longer visible.

11. *Rear.* Rear is the east, that is, not the magnetic direction, but that from which the Rilib comes. For Rilib also the following terms are used:

12. *No in rear*, that is, "sea from eastward."

13. *Ei in Kabin do*, "current before a passage." It means the opposing current which the water flowing out of the lagoons through the movement of the tides occasions. These are often visible for a

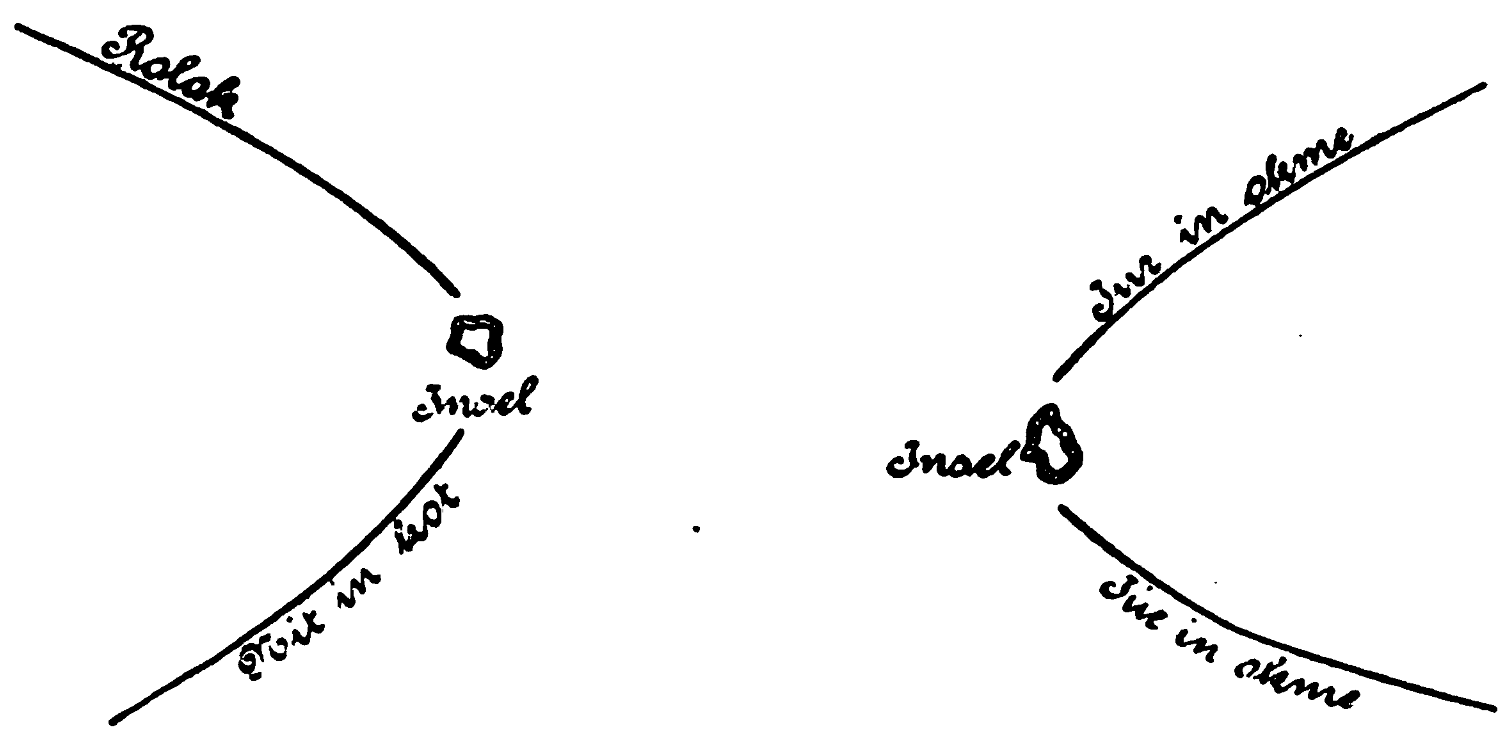


FIG. 3.—Rolok, Nit in kot, Jur in okme.

distance of 15 nautical miles. The directions are different, but the islanders know these and use them in orienting themselves. The Marshall group are made up chiefly of atolls, or reefs, about the islands, whose continuity is broken by passages. Inside these encircling reefs are found water basins called lagoons, for the most part open, and navigable by large ships. These lagoons have frequently considerable extent; that of Jaluit is 32 miles long from north to south; the greatest width reaches 20 nautical miles.

Among the charts seen by me and those that have come into my possession, three kinds are to be discriminated: Those that represent the entire group of islands, Ralik-and Ratak-chain together; those which represent only single parts of groups; and those which serve only for general instruction without referring to any particular islands.

The charts of entire group or of a chain are called Rebbelib; those of smaller sections of groups, Meddo; the instruction charts are termed Mattang.

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But on the Rebbelibs and the Meddos the locating of the islands for navigation is about the same, since the sailing extends only so far as to pass by means of the charts from one atoll to the next through the water indications on them.

I have, in all, five separate charts deposited in the Museum für Völkerkunde in Berlin, where they may be inspected. [NOTE.—Dr. A. Bastian reports that the first of these charts was received by the Berlin Museum in 1883 from Dr. Finsch, the second from Count Hernsheim, in Jaluit, in 1884. In addition Captain Schück, of Hamburg, has made sketches of all known specimens. Dr. A. B. Meyer reports a specimen in Dresden Museum.—Translator.] They are a Mattang; a Rebbelib for the whole island group; a Meddo for the southwestern part of the group; a Rebbelib for the Ralick chain, and a Rebbelib for the Ratack chain. Now these charts are not generally serviceable and established in the forms given, but they are made by chiefs for their individual use as reminders of the various things which they have to attend to in sailing, as well as for rendering clear the noteworthy signs in the tuition of the uninitiated.

Hence the repeated false and apparently wild information from the sticks. For example, in Chart III, Lojak told me that line HL was Bungdockerik and not Mille. When I called his attention to the fact that the former should be right under the latter instead of above, to the left, and that the line HL, according to his own earlier declarations, could mean nothing else than Bungdockeing, and at last also a Kaelib, Lojak asked me how I could possibly bring the little stick underneath when there was no place to tie it there? I might say what I pleased, he had fixed this stick as Bungdockerik; he knew what it ought to mean and that was enough. The impossibility of fastening the little sticks in their right places is at the outset the occasion for individual variations.

The interpretation of the charts is, for the reasons stated, always difficult, if one has not the maker of the chart himself as explainer; another, even an entirely competent navigator, can not under any circumstances read the deliverances of a chart which he himself has not made. [NOTE.—This acute observation is worthy of notice by every ethnologist. For example, the hundreds of totem posts and other complex mythological carvings in Alaska, the painted robes of the Plains Indians, and the sacred dolls of the Pueblo tribes can be interpreted only by those who make them. It is absolute folly to attempt to explain them without this.] For that reason I can get no explanation of the Samoan chart plan published by Dr. Friedlander in the Journal of the Polynesian Society. It was told me that one might read anything in the lines, it depended on how the chart was held and at what point the islands were supposed to be; but what the maker himself had thought about it and what he wished to show by the chart no one would ever know.

From Jochem I learned that he had seen in northern Ratack charts of entirely different form and application. I engaged Jochem to send to Jaluit such charts whenever he could procure them or could interest persons living there to obtain specimens of them.

With these prefatory remarks, it will now be in order for me to give the interpretation of those in hand.

CHART I.

Dimensions from A to B, 79 centimeters. It is a Mattang, or instruction chart, also one which makes clear only in a general way the navigation or the course of the dunungs between two islands. The islands

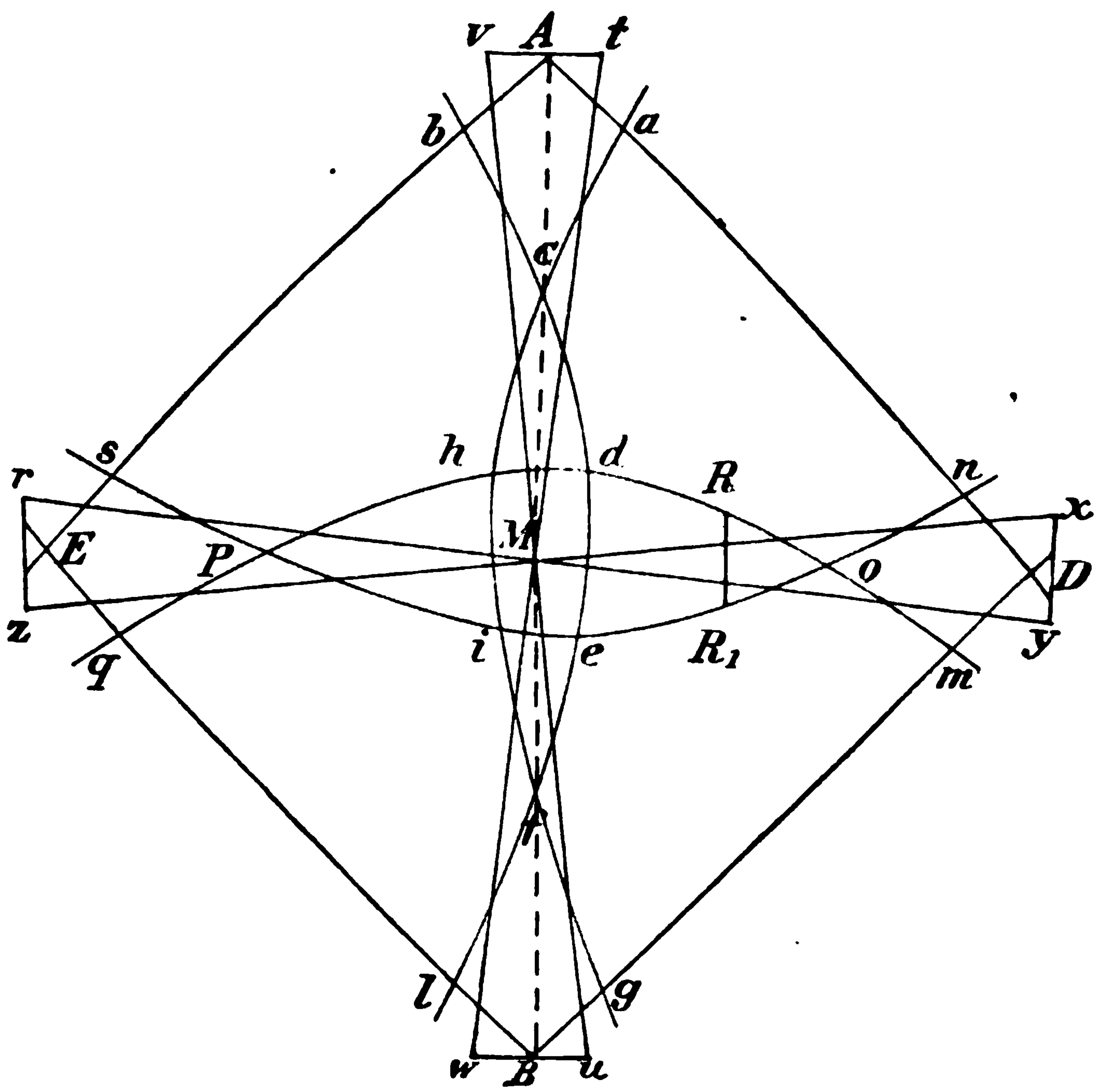


CHART I.—A Mattang, or instruction chart.

are in A and B fixed for north-south course, and in D and E for east-west course. The sides A D, D B, B E, and E A serve rather to hold all together, still D A and D B were pointed out as the Rilib or east channel for D; E A and E B the Kaelib, or west channel for E; A D and A E the Bungdockeing, or north channel for A; B D and B E the Bungdockerik, or south channel for B.

R R i points out the Rear, or east.

t M is an east dunung or Rilib for A.

v M is a Kaelib or dunung for A.

u M is a Rilib or east dunung for B.

w M is a Kaelib for B.

PLATE IV.

MARSHALL ISLANDS CANOE, SHOWING PLATFORM AND OUTRIGGER.

On these straight Rilibs or Kaelibs, crossing one another in M, is to be shown how, among the islands near together and under simple relations, one comes from A to B in straight lines, if he holds himself always between Rilibs and Kaelibs.

ac is another Rilib for A.

bc is another Kaelib for A.

gf is another Rilib for B.

ef is another Kaelib for B.

These lines are to show how the Rilib and Kaelib of A come in contact in boot, or "knot" c, the Rilib and Kaelib of B, in the boot, or "knot" f. If there were no current there, then would follow, from c to M and f to M, a further series of boots or crossings, which would form a direct okar or line of guides between A and B. On the curves c d e f and c h i f it will be made clear that the course of the Okar is not as a rule straight, but through the influence of currents the Okar is set to one side or the other—c d e f will show the course of the eastern current, c h i f of the western current.

Entirely analogous is the intent of the lines between D and E.

yM is a Bungdockerik or southern dunung for D.

xM is a Bungdockeing or northern dunung for D.

zM is a Bungdockerik for E.

rM is a Bungdockeing for E.

mo is a second Bungdockerik for D.

no is a second Bungdockeing for D.

qp is a second Bungdockerik for E.

sp is a second Bungdockeing for E.

The course of the Okar between D and E is shown on its southern offset by the line p i e o, in its northern by the line p h d o.

CHART II.

Dimensions of the chart from Bi to En 102 centimeters, from Nk to Mi 56 centimeters. It is a Rebbelib, or chart of an entire group, which does service indeed as a geographic chart. The little mussels tied on the sticks point out the different islands, whose localities in relation to one another also, according to our own charts, are given with tolerable accuracy. But that results in this case from the fact that this Rebbelib has been prepared after an acquaintance with our own charts. In former times the localities of the islands were inaccurately laid down.

From north and west the abbreviations on the chart mean as follows: Bi=Bikini; Rp=Rongelap; Rk=Rongerik; Br=Bikar; A=Ailinginae; Uk=Utirik; W=Wottho; T=Taka; U=Ujae; Ak=Ailuk; L=Lae; K=Kwadjelinn; Lh=Likieb; We=Wotje; L=Lib; N=Namu; E=Erikub; M=Maloelab; Ar=Aurh; Ab=Ailinglablab; Mo=Majuro; Ao=Arno; Ki=Killi; J=Jaluit; Nk=Namorik; Mi=Mille; En=Ebon.

Besides the islands, this chart contains some lines of dunungs for instruction and for orienting with the points of the compass.

The two curves on the right side of the chart from north to south are Rilibs; the one to left running north and south is a Kaelib. The lines rr and rr, west from the island of Killi, locate the Rilibs for this island, although the left-hand line is incorrectly laid down and ought to be on the right side of Killi Island. The straight line nr east from the island of Namorik shows a No-in-rear, "sea from the east," a

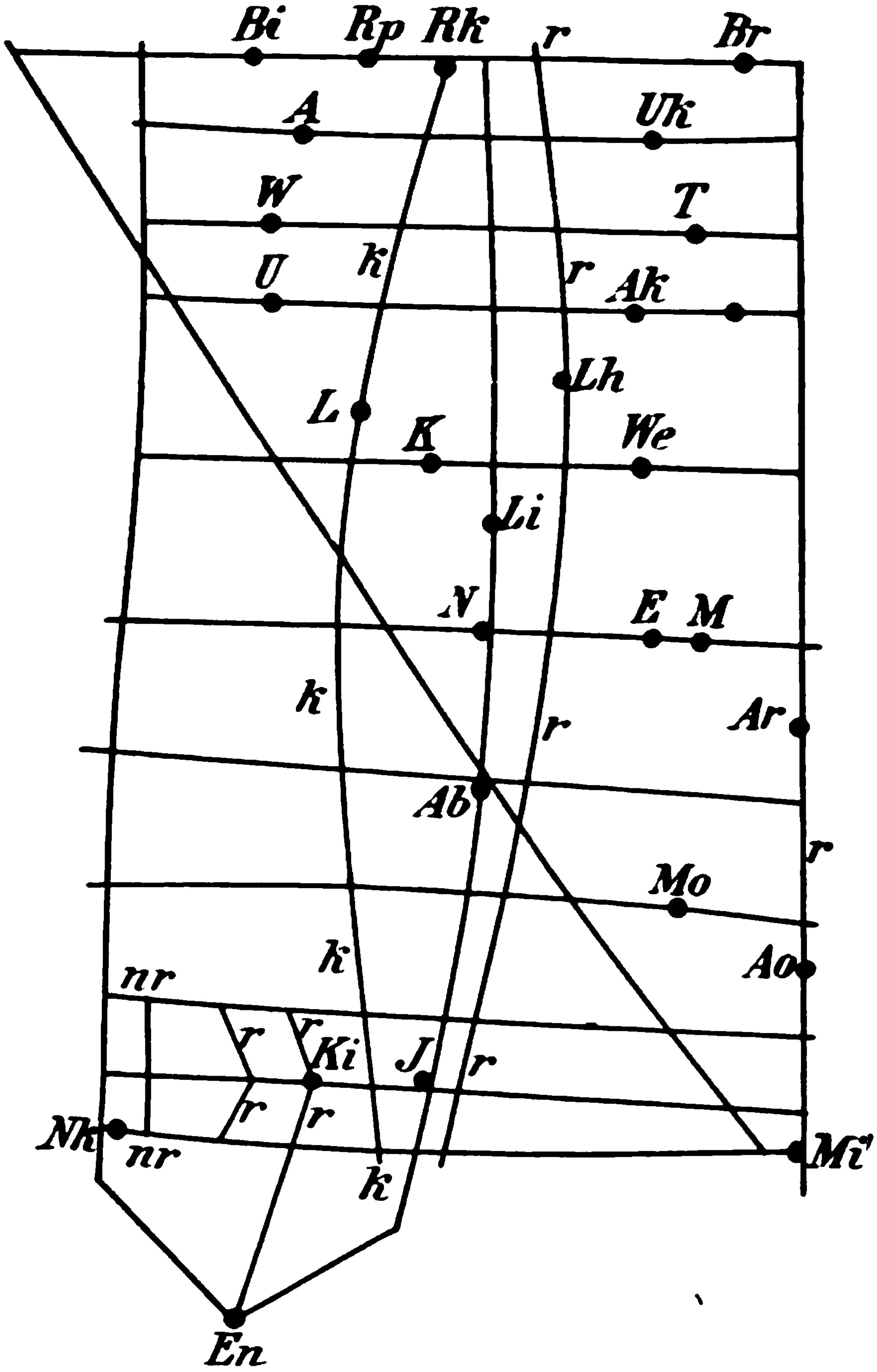


CHART II.—Rebbelib of a large group.

dunung running in a straight line from the east. Ki to En is the Okar, or direct course, between Killi Island and Ebon islands.

CHART III.

Dimensions of the chart from O to M 69 centimeters, from P to E 97 centimeters. It is a Meddo, or part of a group, which shows the position of the islands in the southwestern portion and contains likewise a series of lines for instruction.

HULL OF MARSHALL ISLANDS CANOE, SHOWING STRUCTURE.

United States National Museum collection.

The shell in point J stands for the island Jaluit; on E, for Ebon; on N, for Namorik; on K, for Killi; on M, for Mille; on A, for Ailinglablab.

The lines M G and M E (rrr) fix the Rilib, or east channel, for the island of Mille; the lines O A and O E (kk), the Kaelib, or west channel for the island of Namorik; the lines PQ, RS, TU (bn), give the Bungdockeings, or north dunungs; the lines OM, XZ, VW (bk) the Bungdockerik, or south dunung in general.

HL is to show a Bungdockerik for Mille Island, yet it is laid on the

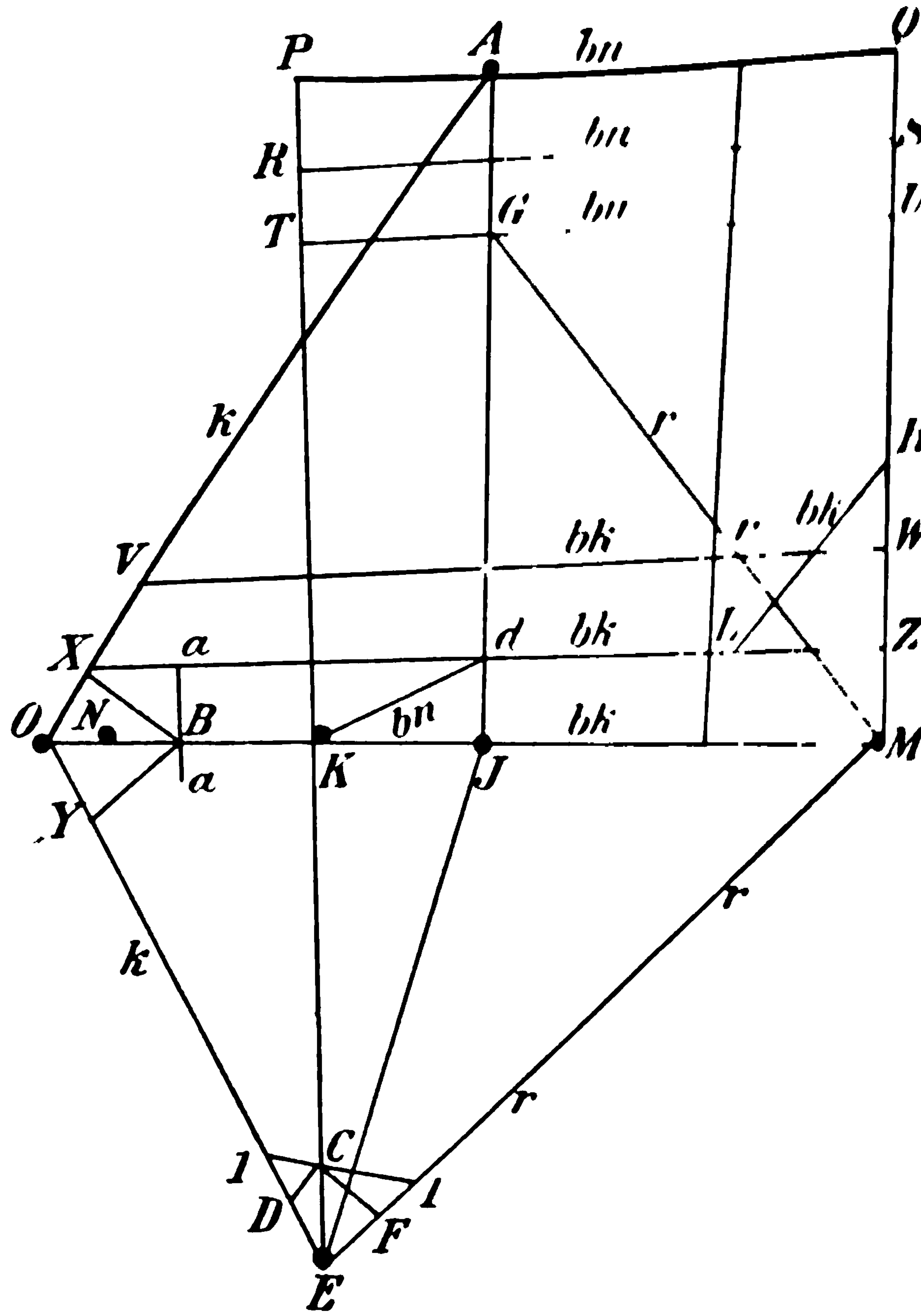


CHART III.—Meddo, covering part of a group.

left above instead of right and underneath, since there is no place on the chart to tie it correctly, as was explained further back.

Kd is intended to show a Bungdockeing, or north dunung, for Jaluit Island, at the same time a Jur-in okme, or "post," for Killi Island. X B is designed to show a Rolok, "lost course," Y B, a Nit in kot, or "cul-de-sac," for the point B. The line *aa* passing through B stands for the No-in-rear, or "sea from the east." The line 1 1 —— through

C is an Ai, land or distance mark, for Ebon Island. C F is a Rilib, or east dunung, C D a west dunung for Ebon Island, C is the Boot formed by the two on the Okar K E between Killi Island and Ebon Island.

CHART IV.

Dimensions from B to E, 156 centimeters; from B to U, 66 centimeters. It is a Rebbelib, or group chart, of the Ralik chain. The position of the islands is insufficiently indicated by the mussels, as may be seen by comparison with the charts. The interpreter Lojak did not understand it at all; at first he was in great doubt about it, since, as he said, it was, for the purpose of this Rebbelib, of no worth. Its chief design must be the positions of the different shallows and their course.

Beginning at the south the identification of the mussels is as follows: E = Ebon island; Nk = Namorik; Ki = Killi; J = Jaluit; Ab = Ailinglablab; Jt = Jabwat; N = Namu; L = Lib; K = Kwadjelinn; Rk = Rongerik; Rp = Rongelap; A = Ailinginae; B = Bikini; W = Wottho; U = Ujae.

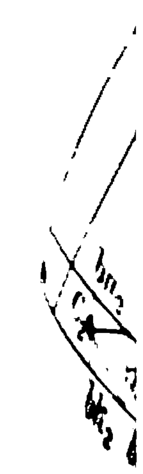
The three curves, r, r, r, extending on the right side from B to E stand for three Rilibs, the three on the left side for three Kaelibs. The Boots, or "knots," on the Okar extending from the island of Jaluit to Namorik are indicated at A_1 A_2, which are occasioned by the Bungdockerik, or south dunung, and the Bungdockeing, or north dunung. They ought properly to lie in points b_1 and b_2 running out from Jaluit Island, but here the explainer said, it is all the same: he knew what they ought to stand for, and knew, too, how to make them clear in their present places.

The Boots, or "knots," on the Okar leading from Jaluit Island to Ailinglablab Island are shown at c_1 and c_2, which are formed by the Rilibs r_1 and r_2, and Kaelibs k_1 and k_2 breaking on Jaluit island. Also here lies c_2, again too close to Ailinglablab. Explanation the same as at a_2. The Boots or "knots" on the Okar leading from Rongerik island to Kwadjelinn Island and then to Namu Island are shown by d_1 and d_2, formed by the Rilibs r_3 and r_4 and the Raelibs k_3, k_4. The Boots on the Okar leading from Kwadjelinn Island to Ujae Island marked e_1 and e_2 are formed from the Bungdockeings, or north dunungs, bn_1, bn_2, and the Bungdockeriks bk_1, bk_2. The guide on the chart passes, indeed, from Namu Island to Ujae Island, but the explainer, Lojak, accented the assertion again and again that it had to be the Okar from Kwadjelinn Island to Ujae Island, the mussel at N could just as well stand for Kwadjelinn as Namu, although he had previously designated the mussel at K as Kwadjelinn.

The line bn makes clear the northern Bungdockeing, bk the southern Bungdockerik, whose ends bn_1, bn_2, bk_1, and bk_2 turn into the Boots e_1 and e_2.

INSTRUCTION CHART IN UNITED STATES NATIONAL MUSEUM.

INSTRUCTION CHART IN UNITED STATES NATIONAL MUSEUM.



The curve k_5 passing from Ab through C, Nk, and E stands for a *Kaelib*, or west dunung.

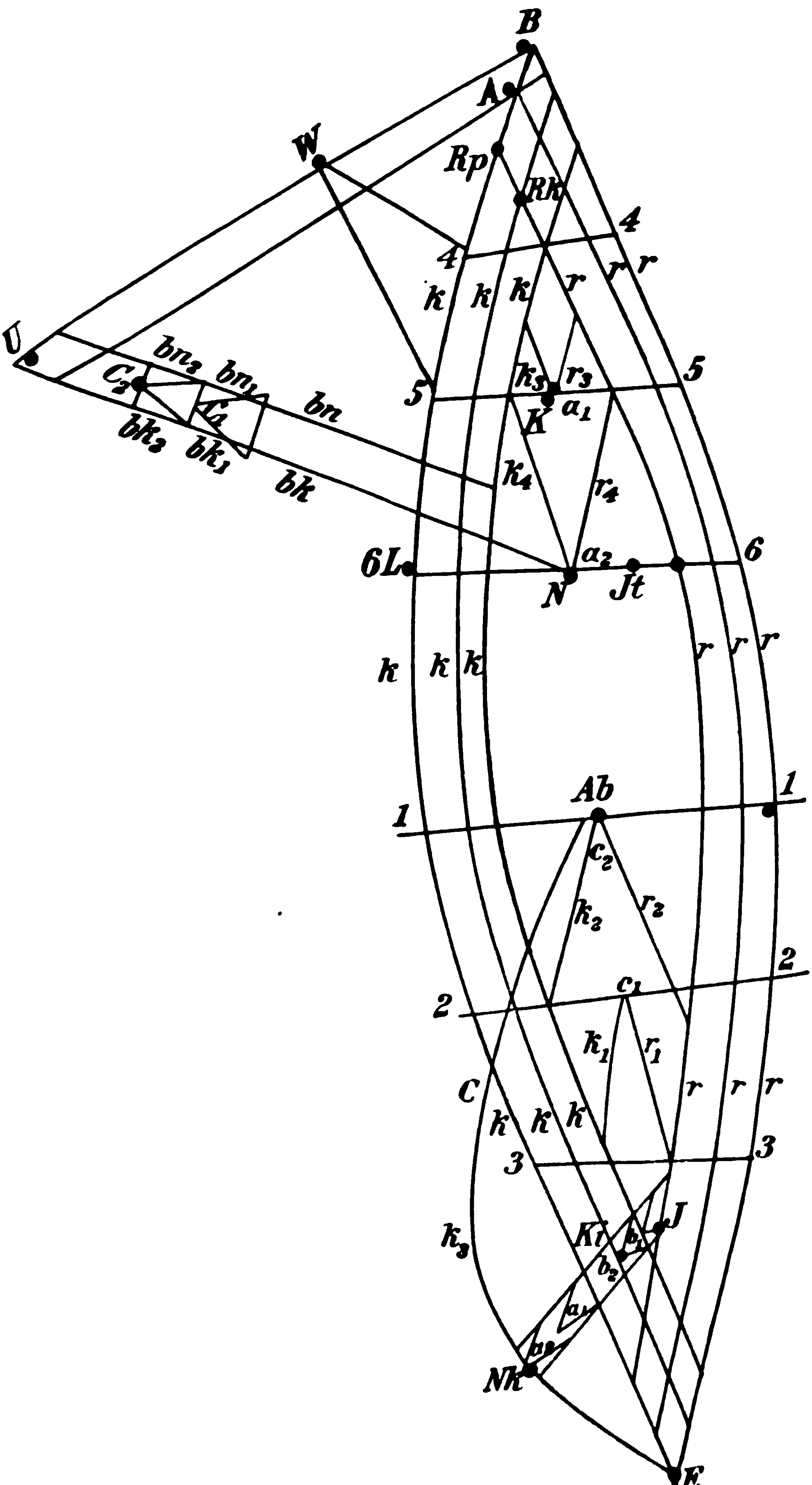


CHART IV. Rebbelib of Ratak chain.

- To one coming from the north the line 1 1 stands for the Djugaf of [illegible] the distance at which the island passes out of sight; 2 2,

The curve k_5 passing from Ab through C, Nk, and E stands for a Kaelib, or west dunung.

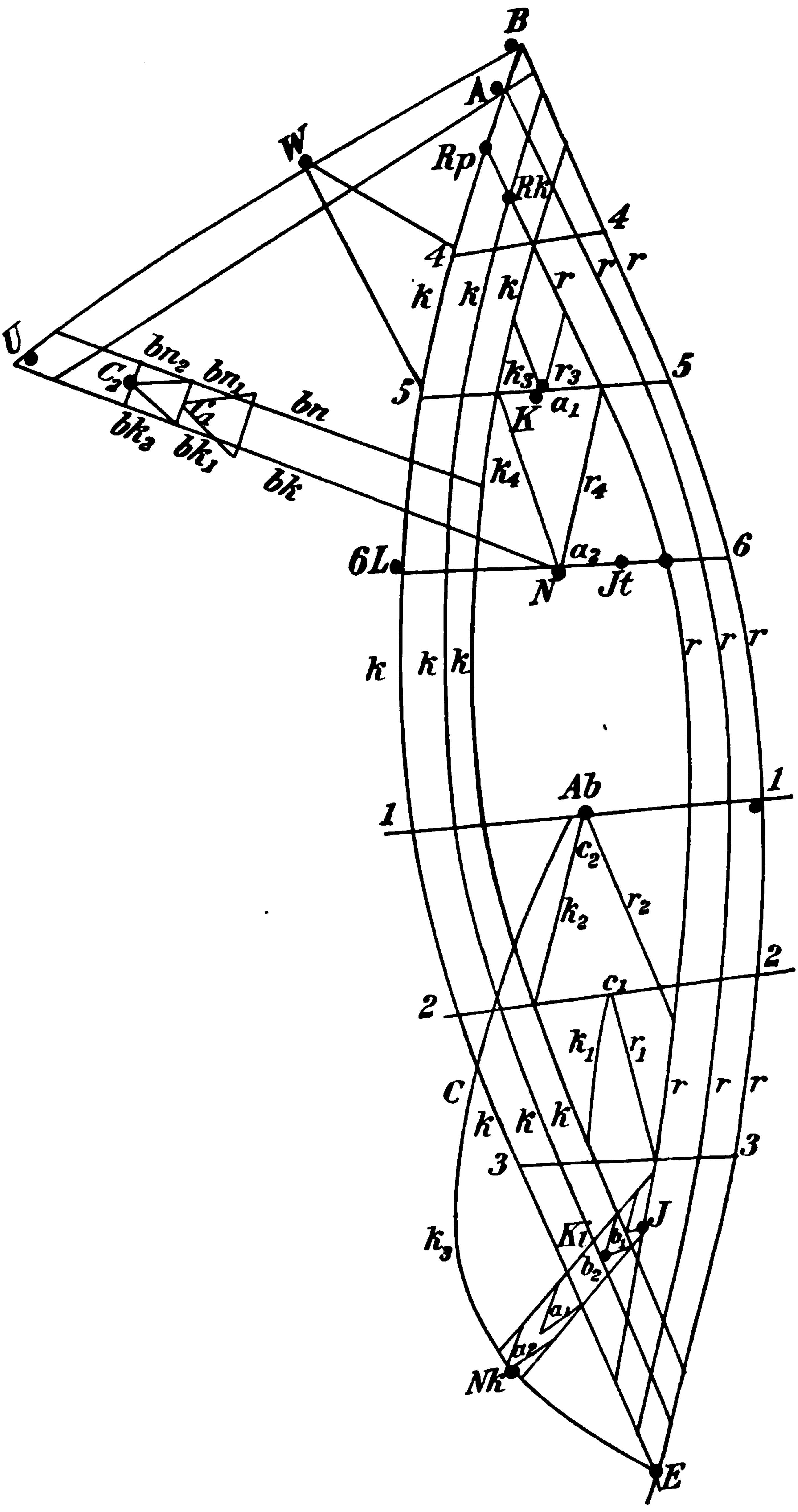


CHART IV.—Rebbelib of Ratak chain.

To one coming from the north the line 1–1 stands for the Djugaï of Jaluit—that is, the distance at which the island passes out of sight; 2–2,

for Egedaï, or distance at which the island may be seen from the canoe; 3-3, for the Djelladaï, or distance at which palm trees may be distinguished. The line 4-4, to one coming from the north, stands for the Djugaï of Namu Island; 5-5, for the Egedaï, and 6-6 for the Djelladaï. In like manner on these lines the Bungdockeings and the Bungdockeriks, or northern or southern dunungs, are made plain.

This Rebbelib, or chart, of the whole group is specially interesting, because upon it are set down the meetings of the Rilibs and Kaelibs, with Bungdockeings and Bungdockeriks which form the Okar leading to the island sought. It also furnished the basis for all navigation.

CHART V.

Dimensions of the chart from P to U, 103 centimeters; from S to H, 128 centimeters. It is a Rebbelib, or group chart, for the Ratak chain. The term Rebbelib, as it has been defined, is not applicable here, since the mussels point out no definite islands of the Ratak chain, but are introduced merely as examples, but the interpreters held, notwithstanding, to the term Rebbelib. There scarcely exists the necessity for a Rebbelib of the entire Ratak chain, since navigation there is only local.

The meaning of the lines is as follows:

AH locates a Bungdockerik; AP, an Okar, or sailing line, between islands A and P; HS, an Okar between H and S; LWVU, an Okar between these points.

The curve rrrr, between H and S, is a Rilib.

The curve kkkk, between H and S, is a Kaelib.

The curve bn, bn, between N and V, a Bungdockeing.

The curve bk, bk, between J and V, a Bungdockerik.

The line EC and EF must signify the Bungdockerik for an imaginary island at E; the line KG, a Bungdockeing for the island K.

The curve KC may point out the general course of a Bungdockerik; the line BD is a Rolok for the island B; the curve MXZ, the general course of a Bungdockeing, as also the line PRSTU; QO and QR fix the Bungdockerik for the island Q; the unnamed lines have no special meaning, the sticks serving only to hold the others together.

NAVIGATION IN THE MARSHALL GROUP.

The Marshall islanders are born sailors, as the position of their islands would occasion, and always pushed the art of navigation extensively. Longer journeys were specially undertaken in the Ralik chain, since its atolls and islands all belonged to the royal family and for that reason kept up a livelier commerce with one another. Only war expeditions were sailed from the Ralik to the Ratak chain.

In the Ratak chain navigation from atoll to atoll is not so lively,

MEDDO IN UNITED STATES NATIONAL MUSEUM.

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REBBELIB, ROYAL ETHNOGRAPHIC MUSEUM, BERLIN (VON LUSCHAU).

REBBELIB IN UNITED STATES NATIONAL MUSEUM.

since there a common possession as in the Ralik chain does not exist. On most of the atolls, in fact, two kings of different race are in power, who are at war, and therefore it is not safe to go there. For the first time, and in recent years, since they are under the German protector-

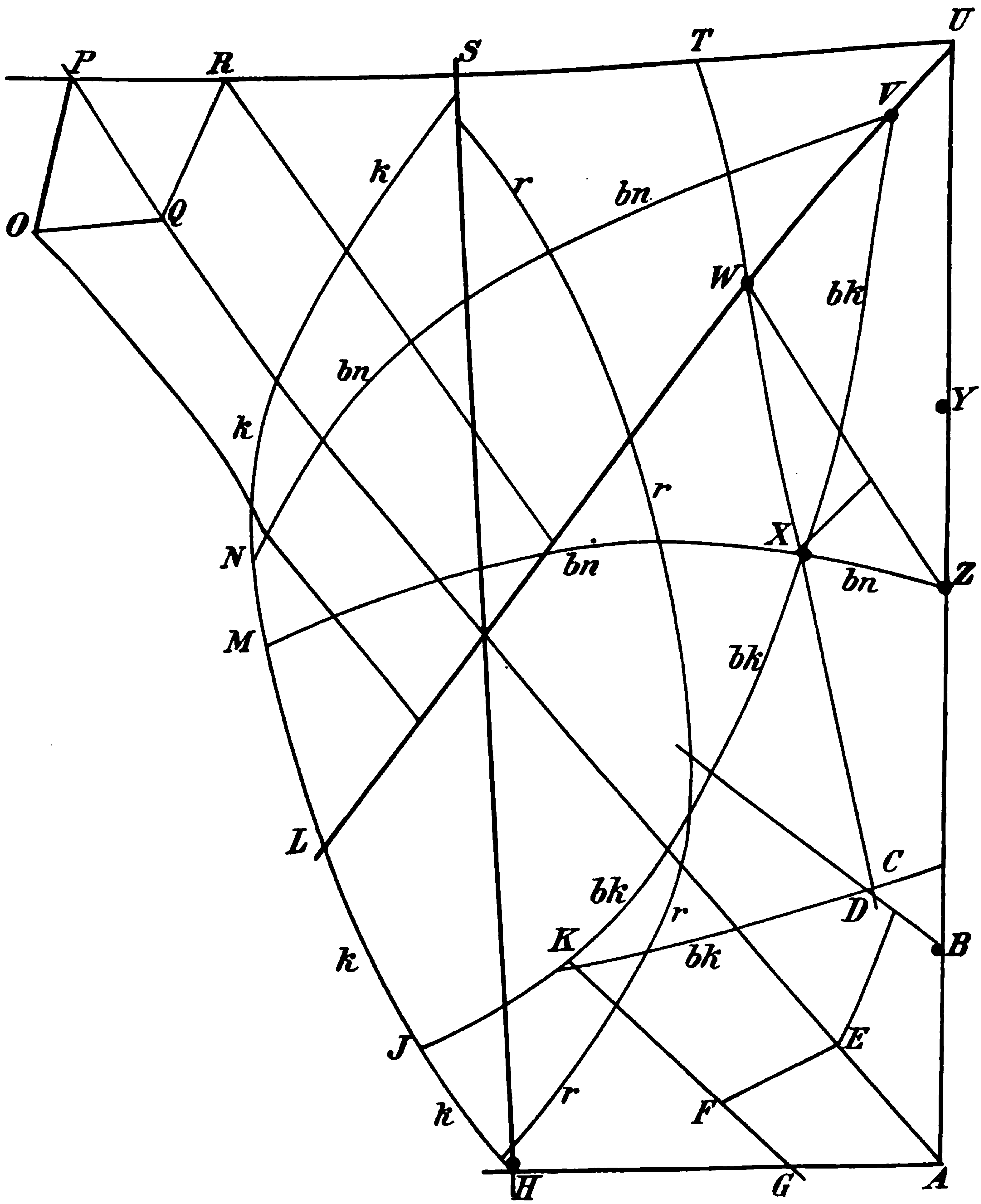


CHART V.—Rebbelib chart of Ratak chain.

ate and peace prevails everywhere, have they changed their relations. Only in the north of the Ratak chain several atolls belong to a people under Chief Muridjil, on whose part the practice exists of undertaking longer journeys, as has already been said.

To estimate the performance of the navigator, it must be recalled that his voyage never extends over the entire Marshall group, but is always sailed from one atoll to the one lying nearest. The distances from one atoll to another are, as the charts make plain, of no great extent, the longest in the Ralik chain being that between Jaluit and Ebon, 85 nautical miles; the distance from the Ralik to the Ratak chain between Jaluit and Mille covers about 120 nautical miles.

As has been shown in describing the charts, the navigator directs his attention in the first place to the dunungs. Whether the stars also were consulted I have not been able to settle definitely. The chiefs denied this, and replied to my questions that they did not specially use the stars, but could just as well find their way when the sky was covered as when the stars were out. On the other hand, Mr. Capelle declared that some old chiefs could direct their courses by the stars, and mentioned as an example that when he was once sailing on an American schooner from Jaluit to Ebon, in company with a chief, the latter remarked to him at night that they were not on the right course, since Ebon lay under a star further eastward. Next morning I found out that he had been right, since they had been standing west from Ebon.

The different dunungs are to be clearly made out by people versed in such things, from the canoes when the water is quiet, and they sail at no other times. As a rule, navigation begins at the close of June or about the 1st of July, and ends when the trades set in, so that the sailing period on the whole covers about four months. During this time, in every case, favorable weather is awaited before a voyage is entered upon, and at first they delay until it seems sure, from a well-known sea lore, that the good weather will last for several days.

The month of July was on this account favorable to setting out on a sea voyage, since then the breadfruit begins to ripen and upon all the islands abundance of this is at hand. A supply of provisions could be carried along only under cramped conditions, since the canoes, as a rule, were already so crowded with men that many times scarcely a decimeter was out of the water.

In such voyages generally a whole people took part, under guidance of their chief. Only entire flotillas and never single canoes take long voyages. The large canoes, which now, since the introduction of the schooners of European pattern, are no longer to be seen, were 50 to 60 feet long and held 40 to 50 persons. Smaller canoes for 10 to 15 persons, which yet exist in larger numbers, and as a rule serve only for commerce inside the lagoons, were frequently taken along.

The canoes consist of the hull, which is made up of large pieces of breadfruit wood sewed together, and the outrigger. Between these is a large platform on which men pass their time. The larger canoes have upon the platform a small hut for covering provisions and mats,

CHART IN ROYAL ETHNOGRAPHIC MUSEUM, DRESDEN (A. B. MEYER).

and to serve in sickness. In other parts of the canoe nothing was stowed, since this must be always unoccupied. On the starboard, upon the platform, were to be seen large hoods, in two parts, made from mats, for protecting the sails.

The sails, which were of finer mats, were well cared for. When out to sea, if there came a Bŏe or a rain storm the sail was at once taken down and covered with mats. The reason for thus protecting them must be the fact that the relatively very large sail in a wet condition would become too heavy. The mats are woven from Pandanus; the cordage is all made from cocoa palm fiber.

When the trades set in and the sea rises so that the low, full-packed canoes can no longer sail, the dunungs and the Kabbelungen were no longer perceivable, navigation ceased, and the canoes were drawn up and entirely overhauled. They were taken apart to renew the joints where the parts came together; the work was polished down with coral, the cordage set right, and new sails prepared.

A flotilla consisted usually of 25 to 30 canoes; there have been some containing as high as 80. The conducting of such a flotilla devolved on the chief and those subchiefs (Leotagetags) who had been initiated into the secrets of navigation. It was strongly and religiously forbidden to divulge anything concerning this art to the people; the chiefs wished to hold this knowledge for their sole benefit, partly for the elevation of their functions; partly to hinder their subjects from learning it in order to free themselves from the frequently tyrannical government of their chiefs.

The chiefs in piloting as a rule stayed together on one canoe, the pilot boat, the other canoes following this. Within the atoll I once saw in the lagoon of Majuro and twice in the lagoon of Arno whole flotillas sailing, when the chiefs there with their following were ordered to appear by His Majesty's ship *Bussard* and to give up their weapons, and they came, moving in close order. The sight was a beautiful one, and the management of the canoes made a fine impression for seamanship. The craft followed in single file and appeared to be used to manœuvring, since the slower ones, as among our own boats of the same size, in order to equal the speed of the superior canoes, took their paddles so as not to be left behind. Captain Kessler is reported to have heard that the canoes on their long journeys sailed abreast in order to reach their destination in better form, but this has not been confirmed, although it may have been practicable. From the explanations received by me, the canoes always followed their leader in single file. The chiefs on the pilot boat act as watch, one on the stern, one on the bow to inspect the water. The latter was the principal officer. In order to be sure that he was on the outlook and kept his eye on the water indications he had to sing continuously. His highest art would consist in keeping on the Okar, between the Rilib and the Kælib, or

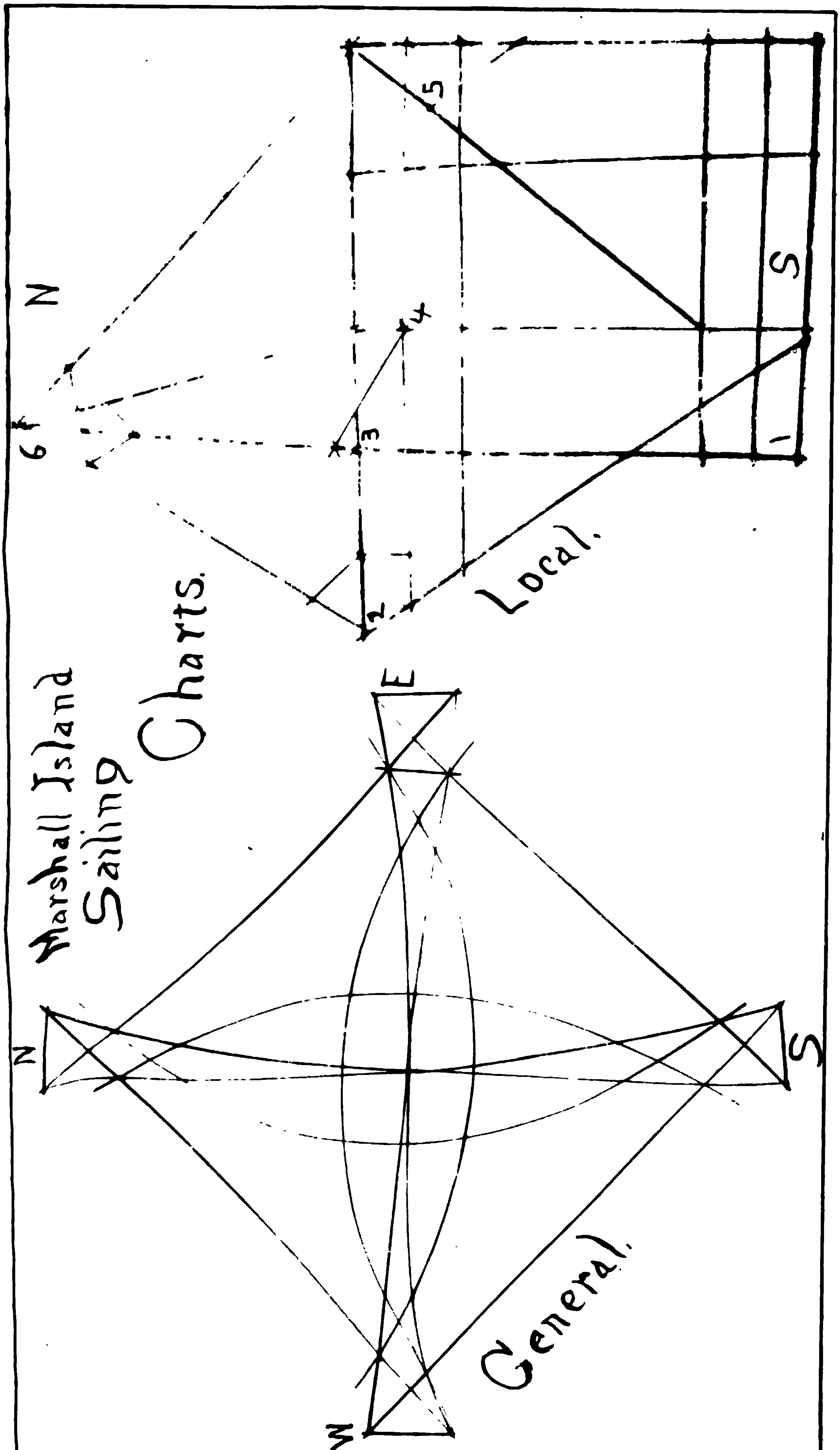
between the Bungdockerik and Bungdockeing. If he got away from the Okar, he had from the well-known marks to find it again. That an extraordinary talent for observation and also great discipline was necessary for this goes without saying. That it was possible to the chiefs I must believe from the explanations given to me, which were always the same. I have had a series of journeys between various islands pointed out to me, but always got the same directions.

That the Dunungs and the Kabbelungs in quiet water are very remarkable in the Marshall group, I have convinced myself; but the statement of the chiefs that the Kabbelungs arising near two neighboring atolls run into each other in such a manner that a canoe, by following the boots of one atoll, must come to the boots of the other, and that from these an okar is formed, though not in a direct line, are assertions yet lacking in clearness, and they are difficult to put in form.

So, it is not quite conceivable that one may know, when he has gotten out of the okar, eastward or westward, or likewise northward or southward, whether in the first case the Rilib or Bungdockeing, or in the second case the Kaelib or Bungdockerik, is perceptibly the preponderating phenomenon. I have long positively refused to admit this possibility, but must agree in the presence of universally expressed opinions. Joachim de Brun told me that at first he had not believed in these appearances in the water, but on a cruise which he had shared his attention had been called by the chiefs and now he himself had witnessed that the differences could be clearly observed. Probably also, for the art of navigation here described, many other local phenomena are at hand, closely associated with the peculiar situation of this thickly set island region.

My doubts in the beginning, whether such a variety of signs could altogether be taken into consideration, and my opinion that very probably the chiefs had made the Rilibs with the help of the heavens the ground of their courses pursued, were opposed by the charts themselves with their many lines and symbols, which would be entirely unnecessary. The existence of the charts, the fixed meaning of the lines on them, prove quite conclusively that the water phenomena noted on them must have been used in navigation.

If the course be lost, and the Dunungs are not recognized so that the okar can be found again, the chiefs then try to make the islands by means of the Rolok, perhaps, or Nit-in-kot, or Jur-in-okme, and if all these efforts fail the case is desperate. In the northern part of the archipelago the struggle would be to keep inside the group, in order somewhere to strike another island, and this generally succeeded; in the southern part, on a voyage between Jaluit, Ebon, and Namorik, the Boots were, under unfavorable conditions, entirely obliterated, of which some examples will be given at the close.



MARSHALL ISLANDS SAILING CHARTS.

Collection of Rev. C. F. Rife.

It was much easier to keep in the right course sailing with a favorable wind than against the wind. As an example of this the voyage from Jaluit to Mille was cited. As a ground therefor it was insisted that north of the okar from Jaluit to Mille the Rilib is much stronger than the Bungdockerik, southward the Bungdockerik is stronger than the Rilib. On this route it was not the Bungdockerik and Bungdockeing that were taken together into consideration, but the Bungdockerik and the Rilib, since in this region the Bungdockeing is scarcely perceptible. The Bungdockerik and the Rilib, on the contrary, are sharply marked, the first running from southeast to northwest, the other east-northeast to west-southwest, so that they turn one upon the other nearly at right angles, a thing which is very apparent to the eye.

In crossing over from Jaluit, the navigator lies first on the starboard bow till the Bungdockerik is much stronger, then he turns and lies on the port bow until the Rilib is the stronger. He need not be so observant that he stay immediately between them, and should be able in his crossing to mark the greater strength of the one or the other dunung, since they run against the canoe forward, entirely distinct and much more striking than when he is sailing with the wind abaft and on a straight course.

The navigator at first makes extended tacks, perhaps six hours long, then gradually shorter. When he arrives at a distance of about 25 nautical miles from Mille, he traces his way by the crossing of the Rolok and the Nit-in-Kot, till in the Djelladai (that is, by means of the palm trees), the island comes into view.

The capability of the navigator, his skill in observing the water and drawing the right conclusions therefrom, came into play, especially under unfavorable conditions, when good weather fails and the flotilla is surprised by bad weather. As a good example in this line, I was told that a chief on the voyage with his flotilla from Ebon to Mamorik encountered so much bad weather, and for that reason had so often to lower sail, that he occupied eight days in the passage. That he did this in spite of the equatorial current, running here from 30 to 40 nautical miles in twenty-four hours and frequently much stronger, must certainly be confessed to have been a great accomplishment.

The journey is not in all cases successful, and there are several sad misfortunes to record. First, from the testimony of Europeans, such cases have been quite numerous, but on close enquiry they can not be substantiated, so I shall close with enumerating the actual instances recounted by Capelle and those mentioned in the oral traditions of the chiefs, which I have reduced to five:

1. About 1830, a flotilla of over 100 canoes set out on a voyage. It was destroyed, and only one boat, with the chief's daughter, Ligibberik, on board, drove on an island in the ocean; the others were never heard from.

2. About 1855, five canoes would pass from Ebon to Jaluit, but they were wrecked on Kusaie; these came back.

3. At the close of 1860, Chief Larjojak, with 35 canoes, set out from Jaluit to Killi, designing to sail farther, from Killi to Namorik. Nothing was again seen of this flotilla. Light winds were blowing when they left Killi; after that it was stormy.

4. In October, 1860, Chief Jurmella, brother of Lojak, endeavored to go from Majuro to Jaluit with 22 canoes. Of these, one canoe with Chief Lamedo on board was driven near Panope in the Carolines; the rest were never heard from.

5. In 1885, 10 canoes, with a war party of 150 men, under chiefs Langeo and Leilik set out from Majuro to Aurh; nothing more was ever seen of them.

The loss of single boats has been more frequent. This has happened when a canoe in the night has strayed from the pilot boat and afterward was betrayed and sold, or when single families would desert their kindred and go off alone from the island. But there were never any persons of importance in these canoes.

MODEL OF MARSHALL ISLANDS CANOE, SHOWING STRUCTURE AND MAT COVER SAIL.

United States National Museum collection.

MODEL OF MARSHALL ISLANDS CANOE, WITH SAIL SET.

United States National Museum collection.

THE PEOPLING OF THE PHILIPPINES.[1]

By Rud. Virchow.

[Translated, with notes, by O. T. Mason.]

PART I.

Since the days when the first European navigators entered the South Sea, the dispute over the source and ethnic affiliations of the inhabitants of that extended and scattered island world has been unsettled. The most superficial glance points out a contrariety in external appearances, which leaves little doubt that here peoples of entirely different blood live near and among one another. And this is so apparent that the pathfinder in this region, Magellan, gave expression to the contrariety in his names for tribes and islands. Since dark complexion was observed on individuals in certain tribes and in defined areas, and light complexion on others, here abundantly, there quite exceptional, writers applied Old World names to the new phenomena without further thought. The Philippines set the decisive example in this. Fernando Magellan first discovered the islands of this great archipelago in 1521, March 16. After his death the Spaniards completed the circle of his discoveries. At this time the name of Negros was fixed,[2] which even now is called Islas de los Pintados. For years the Spaniards called the entire archipelago Islas de Poniente; gradually, after the expedition of Don Fray Garcia Jofre de Loaisa (1526), the new title of the Philippines prevailed, through Salazar. The people were divided into two groups, the Little Negroes or Negritos and the Indios. It is quite conceivable that involuntarily the opinion prevailed that the Negritos had close relationship with the African blacks, and the Indios[3] with the lighter-complexioned inhabitants of India, or at least of Indonesia.

[1] Translated from Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin. Berlin, 1897, January–June, 279–289.

[2] Note.—The island of Negros received its name because it was peopled chiefly by a dark, woolly-haired race, while in other islands these were confined to the interior. Cf. A. B. Meyer, Negritos, 1899, p. 16.—Translator.

[3] This word, except in an historical sense, should never be used for non-Negrito Filipinos.

However, it must be said here that the theory of a truly African origin of the Negritos has been advanced but seldom, and then in a very hesitating manner.[1] The idea that with the present configuration of the eastern island world, especially with their great distances apart, a variety of mankind that had never manifested any aptitude for maritime enterprises should have spread themselves over this vast ocean area, in order to settle down on this island and on that, is so unreasonable that it has found scarcely a defender worth naming. More and more the blacks are coming to be considered the original peoples, the "Indios" to be the intruders. For this there is a quite reasonable ground, in that on many islands the blacks dwell in the interior, difficult of access, especially in the dense and unwholesome mountain forests, while the lighter complexioned tribes have settled the coasts. To this are added linguistic proofs, which place the lighter races, of homogeneous speech, in linguistic relations with the higher races, especially the Malays. Dogmatically it has been said that originally these islands had been occupied entirely by the primitive black population, but afterwards, through intrusions from the sea, these blacks were gradually pressed away from the coast and shoved back into the interior.

The problem, though it appears simple enough, has become complicated more and more through the progress of discovery, especially since Cook enlarged our knowledge of the oriental island world. A new and still more pregnant contrast then thrust itself to the front in the fact that the blacks and the lighter-colored peoples are each separated into widely differing groups. While the former hold especially the immense, almost continental, regions of Australia (New Holland) and New Guinea, and also the larger archipelagos, such as New Hebrides, Solomon Islands, Fiji (Viti) Archipelago—that is, the western areas—the north and east, Micronesia and Polynesia, were occupied by lighter-colored peoples. So the first division into Melanesia and Polynesia has in latest times come to be of value, and the dogma once fixed has remained. For the Polynesians are by many allied to the Malays, while the blacks are put together as a special ethnological race.

For practical ethnology this division may suffice. But the scientific

[1] NOTE.—A striking analogy should not be overlooked; it is the custom of the Negritos to file the front teeth to a point. This custom is widely spread in central Africa, but it is not common in the South Sea. Jagor, Reise in den Philippinen, Berlin, 1880, p. 374, Pl. II, figs. 4–6. [Compare this note with A. B. Meyer's remarks in Die Negritos. 1899, p. 69. Filing the incisors in serrate form is practiced, not by all Negritos, but only by Luzon tribes (Zeitschr. f. Ethnol., 1873, p. 92). Besides the custom in Africa, it will be found with some tribes in New Guinea, some in the Mantawi Islands (southwest of Sumatra), and in Java (see A. B. M., Mittheil. Anthrop. Gesellsch. in Wien, 1874, IV, p. 239, and VII, p. 215, 1877; Ausland, 1883, p. 401; and Max Uhle, in Abhandl. of the Dresden Museum, 1886–87, No. 4, 18 pp., on the ethnological significance of filing the teeth).—TRANSLATOR.]

man will seek also for the blacks a genetic explanation. The answer has been furnished by one of the greatest ethnologists, Theodor Waitz,[1] who, after he had exposed the insufficiency of the accepted formulas, came to the conclusion that the differentiation of the blacks from the lighter peoples might be an error. He denied that there had been a primitive black race in Micronesia and Polynesia; in his opinion we have here to do with a single race. The color of the Polynesians may be out and out from natural causes different; indeed, "their entire physical appearance indicates the greatest variability." Herein the whole question of the domain of variation is sprung with imperfect satisfaction on the part of those travelers who give their attention more to transitions than to types. Among these are not a few who have returned from the South Sea with the conviction that all criteria for the diagnosis of men and of races are valueless.

Analytical anthropology has led to other and often unexpected results. It has proved that just that portion of South Sea population which can apparently lay the strongest claim to be considered a homogeneous race must be separated into a collection of subvarieties. Nothing appears more likely than that the Negritos of the Philippines are the nearest relatives to the Melanesians, the Australians, the Papuans; and yet it has been proved that all these are separated one from another by well-marked characters. Whether these characters place the peoples under the head of varieties, or whether, indeed, the black tribes of the South Sea, spite of all differences, are to be traced back to one single primitive stock, that is a question of prehistory for whose answer the material is lacking.[2] Were it possible to furnish the proof that the black populations of the South Sea were already settled in their present homes when land bridges existed between their territory and Africa, or when the much-sought Lemuria still existed, it would not be worth the trouble to hunt for the missing material. In our present knowledge we can not fill the gaps, so we must yet hold the blacks of the Orient to be separate races.[3]

The hair furnished the strongest character for diagnosis, in which, not alone that of the head is under consideration; the hair, therefore, occupies the foreground of interest. Its color is of the least importance, since all peoples of the South Sea have black hair. It is more the structure and appearance which furnish the observer convenient starting points for the primary classification. Generally a twofold division

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[1]Anthropologie der Naturvölker, Vol. V; The South Sea Islanders, Part II; The Micronesians and Northwestern Polynesians. Leipzig, 1870, pp. 33–36. Finsch, Verh. d. Berliner Anthrop. Ges., 1882, p. 164.

[2]NOTE.—The reader must consult, on the identity of Negritos with Papuans, A. B. Meyer in Zeitschrift für Ethnologie, Verhandl., Berlin, 1875, p. 47, and the Distribution of the Negritos, Dresden, 1899, pp. 76–87.—TR.

[3]On Lemuria cf. A. R. Wallace, Geog, Distrib. of Animals, 1876, I, p. 272, and Island Life, 1880, p. 394.—TR.

satisfies. The blacks, it is said, have crisped hair, the Polynesians and light-colored peoples have smooth hair. But this declaration is erroneous in its generality. It is in no way easy to declare absolutely what hair is to be called crisp, and it is still more difficult to define in what respects the so-called crisp varieties differ one from another. For a long time the Australian hair was denominated crisp, until it was evident that it could be classed neither with that of the Africans nor with that of the Philippine blacks. Semper, one of the first travelers to furnish a somewhat complete description of the physical characters of the Negritos, describes it as an "extremely thick, brown-black, lack-luster, and crisp-woolly crown of hair."[1] Among these peculiarities the lack-luster is unimportant, since it is due to want of care and uncleanliness. On the contrary, the other data furnish true characters of the hair, and among them the crisp-woolly peculiarity is most valuable.

On the terms "wool" and "woolly" severe controversies, which have not yet closed, have taken place among ethnologists during the last ten years. Also the lack of care, especially the absence of the comb, has here acted as a disturbing cause in the decision. But there is yet a set of peoples, which were formerly included, that are now being gradually disassociated, especially the Australians and the Veddahs, whose hair, by means of special care, appears quite wavy if not entirely sleek and smooth. Generally it is frowzy and matted, so that its natural form is difficult to recognize. To it is wanting the chief peculiarity, which obtrudes itself in the African blacks so characteristically that the compact spiral form which it assumes from its root, the so-called "pepper-corn," is selected as the preferable mark of the race. The peculiar nappy head has its origin in the spiral "*rollchen*." As to the Asiatic blacks this has been for a long time known among the Andamanese; it has lately been noticed upon the Sakai of Malacca, and it is to be found also among the Negritos of the Philippines, as I can show by specimens. Therefore, if we seek ethnic relationships for the Negritos of the Philippines, or as they are named, the Ætas (Etas, Itas), such connections obtrude themselves with the stocks named, and the more strongly since they all have brachycephalic, relatively small (nannocephalic) heads and through their small size attach themselves to the peculiar dwarf tribes.

I might here comment on the singular fact that the Andaman Islands are situated near the Nicobars in the Indian Ocean, but that the populations on both sides of them are entirely different. In my own detailed descriptions which treat of the skulls and the hair specially,[2] it is affirmed that the typical skull shape of the Nicobarese is dolichocephalic and that "their hair stands between the straight

[1] Die Philippinen und ihre Bewohner, Würzburg, 1869, p. 49.

[2] Verhandl. der Berliner Anthrop. Gesellschaft, 1885, pp. 104, 109.

hair of the Mongoloid and the sleek, though slightly curved or wavy, hair of the Malayan and Indian peoples;" their skin color is relatively dark, but only so much so as is peculiar to the tribes of India. With the little blacks of the Andamans there is not the slightest agreement. In this we have one of the best evidences against the theory of Waitz-Gerland that the differences in physical appearance are to be attributed to variation merely. I will, however, so as not to be misunderstood, expressly emphasize that I am not willing to declare that the two peoples have been at all times so constituted; I am now speaking of actual conditions.

In the same sense I wish also my remarks concerning the Negritos to be taken. Not one fact is in evidence from which we may conclude that a single neighboring people known to us has been Negritized. We are therefore justified when we see in the Negritos a truly primitive people. As they are now, they were more than three hundred and fifty years ago when the first European navigators visited these islands. About older relationships nothing is known. All the graves from which the bones of Negritos now in possession were taken belong to recent times, and also the oldest descriptions which have been received, so far as phylogeny is concerned, must be characterized as modern.

The little change in the mode of life made known through these descriptions in connection with the low grade of culture on which these impoverished tribes live amply testify that we have before us here a primitive race. * * *

[The question whether we have to do with older, independent races in the Malay Archipelago or with mixtures is everywhere an open one.—TRANSLATOR.]

Whoever would picture the present ethnic affiliations of the light-colored peoples of the Philippines will soon land in confusion on account of the great number of tribes. One of the ablest observers, Ferd. Blumentritt,[1] mentions, besides the Negritos, the Chinese and the whites, not less than 51 such tribes. He classifies them in one group as Malays, according to the plan now customary. This division rests primarily on a linguistic foundation. But when it is noted that the identity of language among all the tribes is not established and among many not at all proved, it is sufficiently shown that speech is a character of little constancy, and that a language may be imposed upon a people to the annihilation of their own by those who belong to a different linguistic stock. The Malay Sea is filled with islands on which tarry the remnants of peoples not Malay.

For a long time, especially since the Dutch occupation, these old

[1] Versuch einer Ethnographie der Philippinen, Petermann's Mittheilungen, Gotha, 1882, No. 67.

populations have received the special name of Alfuros.[1] But this ambiguous term has been used in such an arbitrary and promiscuous fashion that latterly it has been well-nigh banished from ethnological literature. It is not long ago that the Negritos were so called. But if the black peoples are eliminated, there remains on many islands at least an element to be differentiated from the Malay, chiefly through the darker skin color, greater orthocephaly, and more wavy, quite crimped hair. I have, for the different islands, furnished proof, and will here only refer to the assertion that "a broad belt of wavy and curly hair has pressed itself in between the Papuan and the Malay, a belt which in the north seems to terminate with the Veddah, in the south with the Australian." One can not read the accounts of travelers without the increasing conviction of the existence of several different, if not perhaps related, varieties of peoples thrust on the same island.

From this results the natural and entirely unprejudiced conclusion, which has repeatedly been stated, that either a primitive people by later intrusions has been pressed back into the interior or that in course of time several immigrations have followed one another. At the same time it is not unreasonable to think that both processes went on at the same time, and indeed this conception is strongly brought forward. So Blumentritt assumes that there is there a primitive black people and that three separate Malay invasions have taken place. The oldest, whose branches have many traits in accord with the Dayaks of Borneo, especially the practice of head-hunting; a second, which also took place before the arrival of the Spaniards, to which the Tagals, Visayas, Vicols, Ilocanes, and other tribes belong; the third, Islamitic, which emigrated from Borneo and might have been interrupted by the arrival of the Spaniards, and with which a contemporaneous immigration from the Moluccas went on. It must be said, however, that Blumentritt admits two periods for the first invasion. In the earliest he places the immigration of the Igorrotes, Apayos, Zambales—in short, all the tribes that dwelt in the interior of the country later and were pressed away from the coast, therefore, actually, the mountain tribes. To the second half he assigns the Tinguianes, Catalanganes, and Irayas, who are not head-hunters, but Semper says they appear to have a mixture of Chinese and Japanese blood.[3]

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[1]A. Lesson. Les Polynesians, Paris, 1880, Vol. I, pp. 267, 283. [On this objectionable word see A. B. Meyer, The Distribution of the Negritos, Dresden, 1899, Stengel, p. 7.—Tr.]

[2]R. Virchow, Alfuren-Schädel von Ceram und von den Molucken. Verhandl. Berl. Anthrop. Gesellschaft, 1882, p. 78; 1889, pp. 159, 170. [Whether this be a new type or mixture cf. J. G. F. Riedel, De sheik en Kroesharige Rassen tlesschen Selebès en Papua, 1886.—Translator.]

[3]Note.—The dates for these several migrations are given as follows: First migration, 200 B. C.; second migration, 100–500, A. D., bringing the alphabet; third migration, fourteenth and fifteenth centuries, Islamitic. But these dates represent only opinions up to date, from which more thorough inquiry must set out.—Translator.

Against this scheme many things may be said in detail, especially that, according to the apparently well-grounded assertions of Müller-Beeck, the going of the Chinese to the Philippines was developed about the end of the fourteenth century, and chiefly after the Spaniards had gotten a foothold and were using the Mexican silver in trade. At any rate, the apprehension of Semper, which rests on somewhat superficial physiognomic ground, is not confirmed by searching investigations. So the head-hunting of the mountain tribes, so far as it hints at relations with Borneo, gives no sure chronological result, since it might have been contemporaneous in them and could have come here through invasion from other islands.

The chief inquiry is this: Whether there took place other and older invasions. For this we are not only to draw upon the present tribes, but if possible upon the remains of earlier and perhaps now extinct tribes. This possibility has been brought nearer for the Philippines through certain cave deposits. We have to thank, for the first information, the traveler Jagor, whose exceptional talent as collector has placed us in the possession of rich material, especially crania. To his excellent report of his journey I have already dedicated a special chapter, in which I have presented and partially illustrated not only the cave crania, but also a series of other skulls. An extended conference upon them has been held in the Anthropological Society.[1]

The old Spanish chroniclers describe accurately the mortuary customs which were in vogue in their time. The dead were laid in coffins made from excavated tree trunks and covered with a well-fitting lid. They were then deposited on some elevated place, or mountain, or river bank, or seashore. Caves in the mountains were also utilized for this purpose. Jagor describes such caves on the island of Samar, west of Luzon, whose contents have recently been annihilated.[2]

The few crania from there which have been intrusted to me bear the marks of recent pedigree, as also do the additional objects. Unfortunately, Dr. Jagor did not himself visit these interesting caves, but he has brought crania thence which are of the highest interest, and which I must now mention.

The cave in question lies near Lanang,[3] on the east coast of Samar, on the bank of a river, it is said. It is, as the traveler reports, celebrated in the locality "on account of its depressed gigantic crania, without sutures." The singular statement is made clear by means of a well-preserved example, which I lay before you. The entire cra-

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[1] NOTE.—In the matter of evidence for high antiquity and separate race furnished by incrusted cave crania, Prof. William H. Holmes's paper on the Calaveras skull (printed in this volume), should be studied, in which serious doubts are thrown upon the value of such relics as witnesses.—TRANSLATOR.

[2] F. Jagor, Grabstätten zu Nipa-Nipa. Zeitschrift für Ethnologie, 1869, I, p. 80.

[3] Die Philippinen und ihre Bewohner. Verh. der Berliner Anthrop. Gesellsch., 1870, session of 25th of January.

nium, including the face, is covered with a thick layer of sinter, which gives it the appearance of belonging to the class of skulls with Leontiasis ossea. It is, in fact, of good size, but through the incrustation it is increased to gigantic proportions. It is true, likewise, that it has a much flattened, broad and compressed form. The cleaning of another skull has shown that artificial deformation has taken place, which obviously was completed before the incrustation was laid on by the mineral water of the cave. I will here add that on the testimony of travelers no Negritos were on Samar. The island lies in the neighborhood of the Visayas. Although no description of the position of the skull is at hand and of the skeleton to which it apparently belonged, it must be assumed that the dead man was not laid away in a coffin, but placed on the ground; that, in fact, he belonged to an earlier "period." How long ago that was can not be known, unfortunately, since no data are at hand; however, the bones are in a nearly fossilized condition, which allows the conclusion that they were deposited long ago.

The deformation itself furnishes no clue to a chronological conclusion. In Thévenot[1] is found the statement that, according to the account of a priest, probably in the 16th century, the custom prevails in some of the islands to press the heads of new-born babes between two boards, also to flatten the forehead, "since they believed that this form was a special mark of beauty." A similar deformation, with more pronounced flattening and backward pressure of the forehead, is shown on the crania which Jagor produced from a cave at Caramuan in Luzon. There are modes of flattening which remind one of Peru. When they came into our hands it was indeed an immense surprise, since no knowledge of such deformation in the South Sea was at hand. First our information led to more thorough investigations; so we are aware of several examples of it from Indonesia and, indeed, from the South Sea (Mallicolo). However, this deformation furnishes no clue to the antiquity of the graves.

[Chinese and Korean pottery are said to have been found with the deformed crania. Similar deformations exist in the Celebes, New Britain, etc. Head-shaping has been universal, cf. A. B. Meyer, Über Kunstliche deformirte Schädel von Borneo und Mindanao and über die Verbreitung der Sitte der Kunstlichen Schädeldeformirung, 1881, 36 pp., 4°.—TRANSLATOR.]

I have sawed one of these skulls in two along the sagittal suture. The illustration gives a good idea of the amount of compression and of the violence which this skull endured when quite young. The cranial cavity is inclined backward and lengthened, and curves out above, while the occiput is pressed downward and the region of the front fontanelle is correspondingly lacking. Likewise, a considerable

[1] Rélations des diverses voyages curieux. Paris, 1591 (1663).

SKULL FROM CAVE IN PHILIPPINES.

thickness of the bone is to be noted, especially of the vertex. The upper jaw is slightly prognathous and the roof of the mouth unusually arched. (Pl. I.)

For the purpose of the present study, it is unnecessary to go further into particulars. It might be mentioned that all Lanang skulls are characterized by their size and the firmness of bone, so that they depart widely from the characteristics of the other Philippine examples known to me. Similar skulls have been received only from caves, which exist in one of the little rocky islands east from Luzon. They suggest most Kanaka crania from Hawaii, and Moriori crania from Chatham islands, and they raise the question whether they do not belong to a migration period long before the time of the Malays. I have, on various occasions, mentioned this probable pre-Malayan, or at least proto-Malayan, population which stands in nearest relation to the settling of Polynesia. Here I will merely mention that the Polynesian sagas bring the progenitor from the west, and that the passage between Halmahera (Gilolo) and the Philippines is pointed out as the course of invasion.

At any rate, it is quite probable that the skulls from Lanang, Cragaray, and other Philippine islands are the remains of a very old, if not autochthonous, prehistoric layer of population. The present mountain tribes have furnished no close analogies. As to the Igorrotes, which Blumentritt attributes to the first invasion, I refer to my description[1] given on the ground of chronological investigations; according to the account given by Hans Meyer[2] the disposal of the dead in log coffins and in caves still goes on. Of the skulls themselves, none were brachycephalous; on the contrary, they exhibit platyrrhine and in part decidedly pithecoid noses. On the whole, I came to the conclusion, as did earlier Quatrefages and Hamy, that "they stand next in comparison with the Dayaks of Borneo," but I hold yet the impression that they belong to a very old, probably pre-Malay, immigration.[3]

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[1] Schädel der Igorroten. Verhl. der Berliner Anthrop. Gesellsch., 1883, pp. 390, 399. [On the Igorrotes see A. B. Meyer, Negritos, 1899, p. 12, note 2. TRANSLATOR.]

[2] Die Igorroten von Luzon, p. 386.

[3] With this study of crania should be read Dr. A. B. Meyer, on craniological data and their value, in The Distribution of the Negritos, Dresden, 1899, in which he says: "The form of the skull in general is variable and can not be regarded as a permanent character in the development of the races." The reader must not neglect Dr. Meyer's publications, since in them he has the results of careful studies on the spot: Volume VIII, of the folio publications of the Dresden Royal Ethnographic Museum, 1890, on the tribes of Northern Luzon; Volume IX, of the same, on the Negritos, 1893; Album of Philippine Types, 1885, 32 plates, 4°; ditto, 1891, 50 plates; and The Distribution of the Negritos in the Philippine Islands and Elsewhere, Dresden. The last three are published by Stengel & Co., Dresden. The little book on distribution is in English, and contains, in addition to most useful information, a list of Blumentritt's publications.—TRANSLATOR.

PART II.[1]

When, on the 18th of March, 1897, I made a communication on the population of the Philippines, a bloody uprising had broken out everywhere against the existing Spanish rule. In this uprising a certain portion of the population, and indeed that which had the most valid claim to aboriginality, the so-called Negritos, were not involved. Their isolation, their lack of every sort of political, often indeed of village organization, also their meager numbers, render it conceivable that the greatest changes might go on among their neighbors without their taking such a practical view of them as to lead to their engaging in them. Thus it can be understood how they would take no interest in the further development of the affair.

Since then the result of the war between Spain and the Americans has been the destruction of Spanish power, and the treaty of Paris brought the entire Philippine Archipelago into the possession of the United States of America. Henceforth the principal interest is centered upon the deportment of the insurgents, who have not only outlived the great war between the powers, but are now determined to assert, or win, their independence from the conquerors. These insurgents, who for brevity are called Filipinos, belong, as I have remarked, to the light-colored race of so-called Indios, who are sharply differentiated from the Negritos. Their ethnological position is difficult to fix, since numerous mixtures have taken place with immigrant whites, especially with Spaniards, but also with people of yellow and of brown races—that is, with Mongols and Chinese.[2] Perhaps here and there the importance of this mixture on the composite type of the Indios has been overestimated; at least in most places positive proof is not forthcoming that foreign blood has imposed itself upon the bright-colored population. Both history and tradition teach, on the contrary, as also the study of the physical peculiarities of the people, that among the various tribes differences exist which suggest family traits. To this effect is the testimony of several travelers who have followed one another during a long period of time, as has been developed especially by Blumentritt.

In this connection it must not be overlooked that all these immigrations, howsoever many they be supposed to have been, must have come this way from the west. Indeed, a noteworthy migration from the east is entirely barred out, if we look no farther back than the Chinese and Japanese. On the contrary, all signs point to the assumption that from of old, long before the coming of Portuguese and

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[1] Sitzungsberichte der Königlichen Preussischen Akademie der Wissenschaften zu Berlin. Berlin, 1899, Vol. III, 19th January, pp. 14–26.

[2] NOTE.—A brief résumé of these many mixtures is given in Tour du Monde, 27th May and 3d June, 1882; see also statement in this translation.—TRANSLATOR.

Spaniards, a strong movement had gone on from this region to the east, and that the great sea way which exists between Mindanao and the Sulu islands on the north and Halmahera and the Moluccas in the south was the entrance road along which those tribes, or at least those navigators whose arrival peopled the Polynesian Islands, found their way into the Pacific Ocean. But also the movement of the Polynesians points to the west, and if their ancestors may have come from Indonesia there is no doubt that in their long journeys eastward they must have touched at the coasts of other islands on their way, especially the Philippines. Polynesian invasions of the Philippines are not supposed to have closed when a migration of peoples or of men passing out to the Pacific Ocean laid the foundation of a large fraction of the population of the archipelago. It is known that now and then single canoes from the Palawan or the Ladrone islands were driven upon the east coast of Luzon, but their importance ought not to be overestimated. The migration this way from the west must henceforth remain as the point of departure for all explanations of this eastern ethnology. [These statements are well enough for working hypotheses, but actual proofs are not at hand. Ratzel, Berl. Verhandl., etc., Phil. Hist. Class, 1898, I, p. 33.—TRANSLATOR.]

Now, how are the local differences of various tribes to be explained, when on the whole the place of origin was the same? Is there here a secondary variation of the type, something brought about through climate, food, circumstances? It is a large theme, which, unfortunately, is too often dominated by previously-formed theories. The importance of "environment" and mode of life upon the corporeal development of man can not be contested, but the measure of this importance is very much in doubt. Nowhere is this measure, at least in the present consideration, less known than in the Philippines. In spite of wide geological and biological differences on these islands, there exists a close anthropological agreement of the Indios in the chief characteristics, and the effort to trace back the tribal differences that have been marked to climatic and alimentary causes has not succeeded. The influence of inherited peculiarities is also more mighty here, as in most parts of the earth, than that of "milieu."

If we assume, first, that the immigrants brought their peculiarities with them, which were fixed already when they came, we must also accept as self-evident that the Negritos of the Philippines do not belong to the same stock as the more powerful, bright-colored Indios. As long as these islands have been known, more than three centuries, the skin of the Negritos has been dark brown, almost black, their hair short and spirally twisted, and just as long has the skin of the Indios been brownish, in various shades, relatively clear, and the hair has been long and arranged in wavy locks. At no time, so far as known, has it been discovered that among a single family a pronounced variation

from these peculiarities had taken place. On this point there is entire unanimity. In case of the Negritos there is not the least doubt; of the Indios a doubt may arise, for, in fact, the shades of skin color appear greatly varied, since the brown is at times quite blackish, at times yellowish, almost as varied as is the color of the sunburnt hair. But even then the practiced eye easily detects the descent, and if the skin alone is not sufficient the first glance at the hair completes the diagnosis. The correct explanation of individual or tribal variations is difficult only with the Indios, while no such necessity exists in the case of the Negritos. But among the Indios these individual and tribal variations are so frequent and so outspoken that one is justified in making the inquiry whether there has not developed here a new type of inherited peculiarities. If this were the case, it must still be held that already the immigrant tribes had possessed them.

Now, history records that different immigrations have actually taken place. Laying aside the latest before the arrival of the Spaniards, that of the Islamites, in the fourteenth and the fifteenth centuries, there remains the older one. If ethnologists and travelers in general come to the conclusion concerning Borneo—and it is to be taken as certain—that the differences now existing among the wild tribes of this island are very old, it ought not be thought so wonderful if, according to the conditions of the tribes which have immigrated thence, there should exist on the Philippines near one another dissimilar though related peoples. This difference is not difficult to recognize in manners and customs—a side of the discussion which is further on to be treated more fully. We begin with physical characteristics.

Among these the hair occupies the chief place. To be sure, among all the Indios it is black, but it shows not the slightest approach to the frizzled condition which is such a prominent feature in the external appearance of the Negritos and of all the Papuan tribes of the East. This frizzled condition may be called woolly, or in somewhat exaggerated refinement in the name may be attributed to the term "wool," all sorts of meanings akin to wool; in every case there is wanting to all the Indios the crinkling of the hair from its exit out of the follicle, whereby would result wide or narrow spiral tubes and the coarse appearance of the so-called "peppercorn." The hair of all Indios is smooth and straightened out, and when it forms curves they are only feeble, and they make the whole outward appearance wavy or, at most, curled.

But within this wavy or curled condition of the hair there are again differences. In my former communication I have attended to examinations which I made upon a large number of islands in the Malay Sea, and in which it was shown that a certain area exists which begins with the Moluccas and extends to the Sunda group, in which the hair

shows a strong inclination to form wavy locks, indeed passes gradually into crinkled, if not into spiral, rolls. Such hair is found specially in the interior of the islands, where the so-called aboriginal population is purer and where for a long time the name of Alfuros[1] has been conferred on them. On most points affinity with Negritos or Papuans is not to be recognized. Should such at any time have existed, we are a long way from the period when the direct causes therefor are to be looked for. In this connection the study of the Philippines is rich with instruction. In the limits of the almost insular, isolated Negrito enclave, mixtures between Negritos and Indios very seldom surprise one, and never the transitions that can have arisen in the post-generative time of development. [The island of Negros, on the contrary, is peopled by such crossbreeds.—TRANSLATOR.]

If there are among the bright-colored islanders of the Indian Ocean Alfuros and Malays close together there is nothing against coming upon this contrast in the Philippine population also. Among the more central peoples the tribal differences are so great that almost every explorer stumbles on the question of mixture. There not only the Dayaks and the other Malays obtrude themselves, but also the Chinese and the Mongolian peoples of Farther India. Indeed, many facts are known, chiefly in the language,[2] the religion, the domestic arts, the agriculture, the pastoral life which remind one of known conditions peculiarly Indian. The results of the ethnologists are so tangled here that one has to be cautious when one or another of them draws conclusions concerning immigrations, because of certain local or territorial specializations. Of course, when a Brahmanic custom occurs anywhere it is right to conclude that it came here from India. But before assuming that the tribe in which such a custom prevails itself comes from Hither or Farther India, the time has to be ascertained to which the custom is to be traced back. The chronological evidence leads to the confident belief that the custom and the tribe immigrated together.

Over the whole Philippine Archipelago religious customs have changed with the progress of external relations. Christianity has in many places spread its peculiar customs, observances, and opinions, and changed entirely the direction of thought. On closer view are to be detected in the midst of Christian activities older survivals, as ingredients of belief which, in spite of that religion, have not vanished. Before Christianity, in many places, Islam flourished, and it is not

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[1] On this objectionable name, see supra, p. 514. That the term does not connote hair characters cf. A. B. Meyer, Sitzungsb. d. Phil. Hist. Classe der kaiserlichen Akademie der Wissensch. Wien, 1882, Vol. CI, p. 550.—TRANSLATOR.

[2] Don T. H. Pardo de Tavera, El sanscrito e la lengua tagalog. Paris, 1887.

surprising to witness, as on Mindanao, Christian and Mohammedan beliefs side by side. But, before Islam, ancestor worship, as has long been known, was widely prevalent. In almost every locality, every hut has its Anito with its special place, its own dwelling; there are Anito pictures and images, certain trees and, indeed, certain animals in which some Anito resides. The ancestor worship is as old as history, for the discoverers of the Philippines found it in full bloom, and rightly has Blumentritt[1] characterized Anito worship as the ground form of Philippine religion. He has also furnished numerous examples of Anito cult surviving in Christian communities.

Chronology has a good groundwork and it will have to observe every footprint of vanishing creeds. Only, it must not be overlooked that the beginning of the chronology of religion has not been reached, and that the origin of the generally diffused ancestor worship, at least on the Philippines, is not known. If it is borne in mind that belief in Anitos is widely diffused in Polynesia and in purely Malay areas, the drawing of certain conclusions therefrom concerning the prehistory of the Philippines is to be despaired of.

Next to religious customs, among wild tribes fashions are most enduring. Little of costume is to be seen, indeed, among them. Therefore, here tattooing asserts its sway. The more it has been studied in late years the more valuable has been the information in deciding the kinship relations of tribes. Unfortunately, in the Philippines the greater part of the early tattoo designs have been lost and the art itself is also nearly eliminated. But since the journey of Carl Semper[2] it has been known that not only Malays but also Negritos tattoo; indeed, this admirable explorer has decided that the "Negroes of the East Coast" practice a different method of tattooing from that of the Marivales in the west, and on that account they attain different results. In the one case a needle is employed to make fine holes in the skin in which to introduce the color; in the other long gashes are made. In the latter case prominent scars result; in the former a smooth pattern. But these combined patterns are on the whole the same, instead of rectilinear figures. Schadenburg has the operations commence with a sharpened bamboo on children 10 years of age.[3] Among the wild tribes of the light-colored population tattooing is not less diffused, but the patterns are not alike in the different tribes. Isabelo de los Reyes reports that[4] the Tinguianes, who inhabit the mountain forests of the northern cordilleras of Luzon, produce figures of stars, snakes, birds, etc., on children 7 to 9 years old. Hans Meyer describes the pattern

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[1] Der Ahnencultus und die religiösen Anschauungen der Malaien des Philippinen-Archipels. Wien, 1882, p. 2. (From Mittheil. der K. K. Geograph. Gesellschaft).

[2] Die Philippinen und ihre Bewohner. Würzburg, 1869, pp. 50, 137.

[3] Zeitschrift für Ethnologie, 1880, XII, p. 136.

[4] Die Tinguianen (Luzon). Translated from the Spanish by F. Blumentritt (Mitth. der K. K. Geograph. Ges. in Wien), 1887.

of the Igorrotes.[1] There appears to exist a great variety of symbols; for example, on the arms, straight and crooked lines crossing one another; on the breast, feather-like patterns. Least frequently he saw the so-called Burik designs, which extended in parallel bands across the breast, the back, and calves, and give to the body the appearance of a sailor's striped jacket. It is very remarkable that the human form never occurs.

What is true concerning tattooing on so many Polynesian islands holds also completely here. But reliable descriptions are so few, and especially there is such a meager number of useful drawings, that it would not repay the trouble to assemble the scattered data. At least it will suffice to discover whether among them there are genuine tribal marks or to investigate concerning the distribution of separate patterns. Those known show conclusively that in the matter of tattooing the Filipinos are not differentiated from the islanders of the Pacific; they form, moreover, an important link in the chain of knowledge which demonstrates the genetic homogeneity of the inhabitants. The tattooings of the eastern islanders are comparable only to those of African aborigines, with which last they furnish many family marks, made out and recognized. It is desirable that a trustworthy collection of all patterns be collected before the method becomes more altered or destroyed.

Next to the skin, among the wild tribes the teeth are modified in the most numerous artificial alterations. The preferable custom, common in Africa, of breaking out the front teeth in greater or less number has not, so far as I remember, been described among the Filipinos; I only mention that while I was making a revision of our Philippine crania, two of them turned up in which the middle upper incisors had evidently been broken out for a long time, for the alveolar border had shrunk into a small quite smooth ridge, without a trace of an aveolus. It is otherwise with the pointing of the incisors, especially the upper ones, which, also is not common. I must leave it undecided whether the sharpening is done by filing or by breaking off pieces from the sides. The latter should be in general far more frequent. In every case the otherwise broad and flat teeth are brought to such sharp points as to project like those of the carnivorous animals. I have met with this condition several times on Negrito skulls and furnished illustrations of them.[2] On a Zambal skull, excavated by Dr. A. B. Meyer and which I lay before you, the deformation is easy to be seen. I called attention at the time to the fact that among the Malays an entirely different method of modifying the teeth is in vogue, in which a horizontal filing on the front surface is practiced and the sharp

[1] Verhandl. der Berliner Gesellsch. für Anthropologie, 1883, p. 380.

[2] Abhandlung über alte und neue Schädel, in F. Jagor's Reisen in den Philippinen. Berlin, 1873, p. 374, Pl. II, figs. 4 and 5.

lower edge is straightened and widened. Already the elder Thévenot has accented this contrast when he says:

"These cause the teeth to be equal, those file them to points, giving them the shape of a saw."[1]

This difference appears to have held on till the present; at least no skull of an Indio is known to me with similar deformation of the teeth. This custom of the Negritos is so much more remarkable since the chipping of the corners of the teeth is widely spread among the African blacks.

The other part of the body used most for deformation—the skull—is in strong contrast to the last-named custom. Deformed crania, especially from older times, are quite numerous in the Philippines; probably they belong exclusively to the Indios. If they exist among the Negritos, I do not know it; the only exception comes from the Tinguianes, of whom J. de los Reyes reports their skulls are flattened behind (por detrás oprimido). Such flattening is found, however, not seldom among tribes who have the practice of binding children on hard cradle boards—chiefly among those families who keep their infants a long time on such contrivances. A sure mark by which to discriminate accidental pressure of this sort from one intentionally produced is not at hand; it may be that in accidental deformation oblique position of the deformed spot is more frequent; at any rate, the difference in the Philippines is a very striking one, since there not so much the occiput as the front and middle portions suffer from the disfigurements, and thereby deformations are produced that have had their most perfect expression among the ancient Peruvians and other American tribes.

I have discussed cranial deformation of the Americans in greater detail, where I exhibit the accidental and the artificial (intentional) deformation in their principal forms.[2] The result is that in large sections of America scarcely any ancient skulls are found having their natural forms, but that the practice of deformation has not been general; moreover, a number of deformation centers may be differentiated which stand in no direct association with one another. The Peruvian center is far removed from that of the northwest coast, and this again from that of the Gulf States. From this it must not be said that each center may have had its own, as it were, autochthonous origin. But the method has not so spread that its course can be followed immediately. Rather is the supposition confirmed that the method is to be traced to some other time, therefore that somewhere there must have been a place of origin for it. On the Eastern Hemisphere, and especially in the region here under consideration, the relations are

[1] G. A. Baer (Verhandl. d. Berliner Anthrop. Gesellschaft, 1879, p. 331) says that such an operation obtains only among Negritos of pure blood.

[2] Crania ethnica Americana. Berlin, 1892, p. 5, and figs.

apparently otherwise. Here exist, so far as known, great areas entirely free from deformation; small ones, on the other hand, full of it. There are here, also, deformation centers, but only a few. Among these, with our present knowledge, the Philippines occupy the first place.

The knowledge of this, indeed, is not of long duration. Public attention was first aroused about thirty years ago concerning skulls from Samar and Luzon, gathered by F. Jagor from ancient caves, to furnish the proof of their deformation. Up to that time next to nothing was known of deformed crania in the oriental island world. First through my publication[1] the attention of J. G. Riedel, a most observant Dutch resident, was called to the fact that cranial deformation is still practiced in the Celebes, and he was so good as to send us a specimen of the compressing apparatus for delicate infants (1874).[2] Compressed crania were also found. But the number was small and the compression of the separate specimens was only slight. In both respects what was observed in the Sunda islands did not differ from the state of the case in the Philippines. Through Jagor's collections different places had become known where deformed crania were buried. Since then the number of localities has multiplied. I shall mention only two, on account of their peculiar locality. One is Cagraray, a small island east of Luzon, in the Pacific Ocean, at the entrance of the Bay of Albay;[3] the other, the island of Marindúque, in the west, between Luzon and Mindoro. From the last-named island I saw, ten years ago, the first picture of one in a photograph album accidentally placed in my hands. Since then I had opportunity to examine the Schadenberg collection of crania, lately come into the possession of the Reichsmuseum, in Leyden, and to my great delight discovered in it a series of skulls which are compressed in exactly the same fashion as those of Lanang. It is said that these will soon be described in a publication.

It is of especial interest that this method has been noted in the Philippines for more than three hundred years. In my first publication I cited a passage in Thévenot where he says, on the testimony of a priest, that the natives on some islands had the custom of compressing the head of a newborn child between two boards, so that it would be no longer round, but lengthened out; also they flattened the forehead, which they looked upon as a special mark of beauty. This is, therefore, an ancient example. It is confirmed by the circumstance that these crania are found especially in caves, from the roofs of which mineral waters have dripped, which have overlaid the bones partly

[1] Zeitschr. für Ethnologie, 1870, Vol. II, p. 151.

[2] The same, III, p. 110, Pl. V, fig. 1; Verhandl. Anthrop. Ges., Vol. VI, p. 215; Vol. VII, p. 11; Vol. VIII, p. 69; Vol. IX, p. 276.

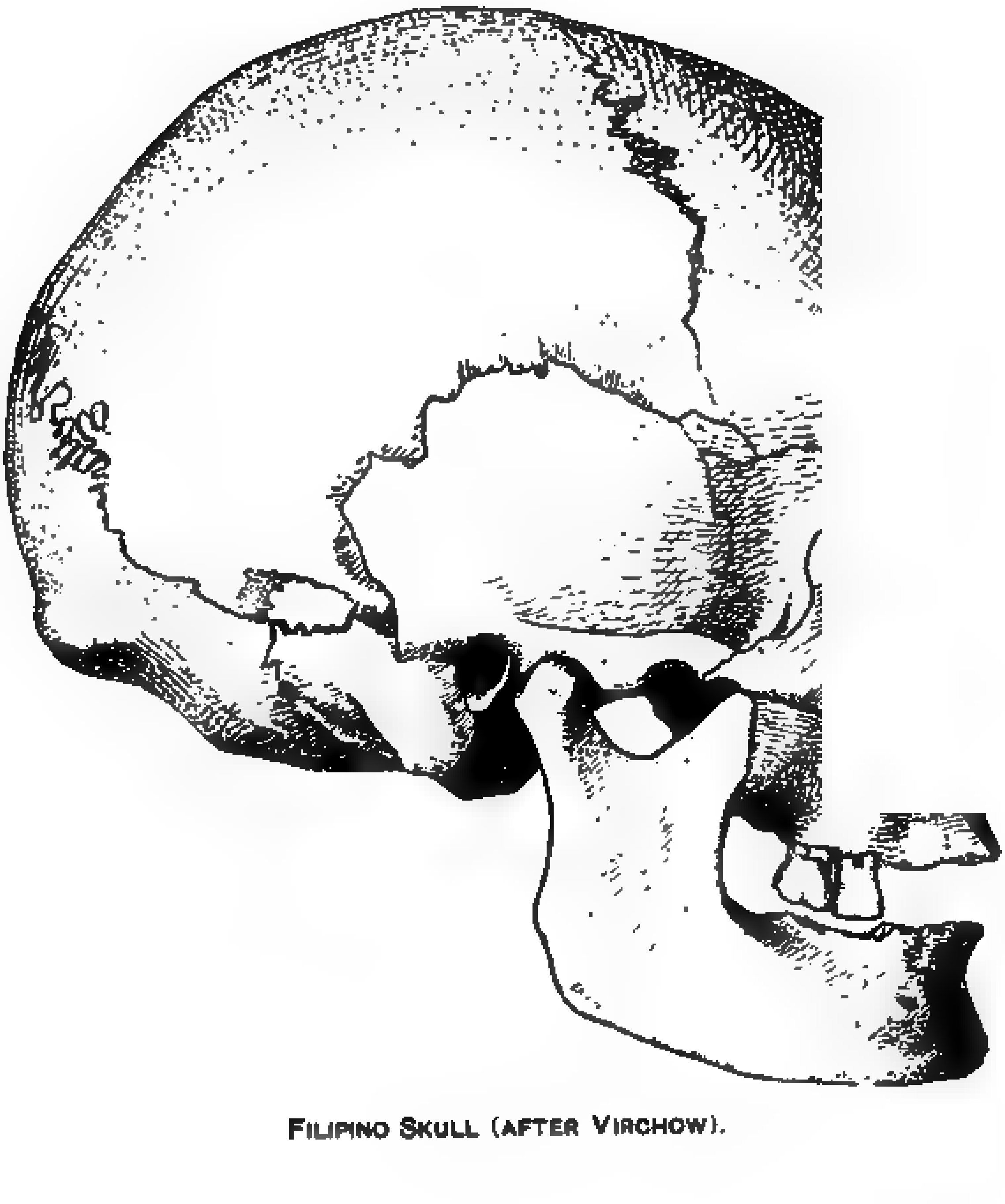
[3] Verhandl. der Berliner Anthrop. Gesellsch., 1879, Vol. XI, p. 422; 1889, Vol. XXI, p. 49.

with a thick layer of calcareous matter. The bones themselves have an uncommonly thick, almost ivory, fossil-like appearance. Only the outer surface is in places corroded, and on these places saturated with a greenish infiltration. It is to be assumed, therefore, that they are very old. I have the impression that they must have been placed here before the discovery of the islands and the introduction of Christianity. Their peculiar appearance, especially their angular form and the thickness of the bone, reminds one of crania from other parts of the South Sea, especially those from Chatham and Sandwich Islands. I shall not here go further into this question, but merely mention that I came to the conclusion that these people must be looked upon as proto-Malayan.

The changes which will take place in the political condition of the Philippines may be of little service to scientific explorations at first; but the study of the population will be surely taken up with renewed energy. Already in America scholars have begun to occupy themselves therewith. A brief article by Dr. Brinton is to be mentioned as the first sign of this.[1] But should the ardent desire of the Filipinos be realized, that their islands should have political autonomy, it is to be hoped that, out of the patriotic enthusiasm of the population and the scientific spirit of many of their best men, new sources of information will be opened for the history and the development of oriental peoples. To this end it may be here mentioned, by the way, that the connecting links of ancient Philippine history and the customs of these islands, as well with the Melanesians as with the Polynesians of the south, are yet to be discovered.

As representatives of these two groups, I present, in closing, two especially well-formed crania from the Philippines. One of them, which shows the marks of antiquity that I have set forth, belongs to Indio (Pl. II). It has the high cranial capacity of 1,540 cubic centimeters, a horizontal circumference of 525 millimeters, and a sagittal circumference of 386 millimeters; its form is hypsidolicho, quite on the border of mesocephaly: Index of width, 75.3; index of height, 76.3. Besides, it has the appearance of a race capable of development; only, the nose is platyrrhine (index, 52.3), as among so many Malay tribes, and in the left temple it bears a *Processus frontalis squamæ temporalis* developed partly from an enlarged fontanelle. The other skull (Pl. III) was taken from a Negrito grave of Zambales by Dr. A. B. Meyer. It makes, at first glance, just as favorable an impression, but its capacity is only 1,182 cubic centimeters; therefore 358 cubic centimeters less than the other. Its form is orthobrachycephalic; breadth index, 80.2; height index, 70.6. As in single traits of development, so in the measurements, the difference and the debased character of this race obtrude themselves. Only, the nasal index is somewhat smaller; on the whole, the nose has in its separate parts a decidedly pithecoid form.

[1] The Peoples of the Philippines, Washington, D. C., 1898. American Anthropologist.



FILIPINO SKULL (AFTER VIRCHOW).

NEGRITO SKULL (AFTER VIRCHOW).

LIST OF THE NATIVE TRIBES OF THE PHILIPPINES AND OF THE LANGUAGES SPOKEN BY THEM.[1]

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[Translated, with introduction and notes, by O. T. MASON.]

INTRODUCTION.

Ethnology relates to groups of human beings that are characterized by names. Professor Blumentritt has done wisely, therefore, in laying the foundation for Philippine ethnology, to collect in a scrupulous manner such titles as are known to have belonged to groups of the population.

The names in Professor Blumentritt's list were conferred for various reasons, among which the following are most important:

1. Blood kinship or race, the biological concept, with the classific terms species, subspecies, variety, race, breed, mixture.
2. Speech, language, the linguistic concept, with the classific terms monosyllabic, incorporated, inflected, family, language, dialect.
3. Tribal organization, the political concept, with the classific terms nation, kingdom, republic, confederacy, tribe, phratry, gens.
4. Location, the geographic concept, with the classific terms derived from continents, oceans, river basins, islands, natural features.

These classific concepts should be kept apart carefully in the student's mind, although each one of them may be made helpful in perfecting the comprehending of the others.

1. The present population of the Philippines is one of the most interesting of ethnologic combinations, since in its veins flows in larger or smaller proportion the blood of all the types of mankind, to wit: Negrito, Papuan, and African negro; Mongol and Malay; American Indian; Hamite, Semite, and Aryan, if not an earlier Allophyllian white ingredient. In what degree this is true of any one and how far it has influenced the naming of tribes, remains to be ascertained. The two aboriginal races most in evidence in the islands are the Negrito and the Malay.

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[1]Translated from Zeitschrift der Gesellschaft für Erdkunde zu Berlin. Berlin, 1890, Vol. XXV, pp. 127-146. With plates from Hilder Collection, Bureau of American Ethnology.

2. The language problem, it is repeated, has in itself naught to do with blood or breeds of men. Though the population of the Philippine Archipelago is about as large as that of the whole Western Hemisphere when Magellan, in August, 1521, reached Mindanao and Cebu, no such riches of linguistic families ever existed in the islands as in America. Already a small number of grammars and dictionaries have been published by Catholic missionaries and others, but, according to those best authorized to speak, the linguistic task remains to be done.

3. On the third point, that of sociology and politics, the gentile and tribal organization, it may be said that the Negritos are not believed to possess any plan of organization, which is only a confession of ignorance. Blumentritt applies the terms "hord," "stamm," "tribus," "volk," and "volkstamm," as he was obliged to do, much as English and American ethnologists write the words horde, people, tribe, and folk, meaning aggregations in general where the specific plan of organization is not understood.

4. Place names for settlements and groups of inhabitants are of least importance in ethnology. The Eskimo add the syllable "mute" to the name of a locality to denote the people camped there. So, in the Philippines, as Blumentritt explains, many of his folk names are place names (toponyms), throwing no light on breed, speech, or political organization.

To unravel the mysteries set forth by the foregoing is the opportunity of the ethnologist. It needs only to look back upon the bloody horrors enacted in our own history through lack of knowledge concerning the social organization and prejudices of the Indians to awaken the liveliest sympathies and cooperation of the statesmen and philanthropists in the ethnology of the Philippines.

General table on the Philippine and Sulu islands from census of 1887.

Islands and groups.	Square miles.	Population, 1887.	Natives, independent or not counted.	Total.	Per square mile.
Luzon and islands near	109,206	3,443,000	? 150,000	3,600,000	33
Mindoro—Masbate islands	15,358	126,000	? 100,000	225,000	15
Visayas Archipelago	54,788	2,181,000	? 200,000	2,400,000	44
Mindanao (12)	97,968	209,000	? 400,000	600,000	62
Calamianes and Paluan	14,123	22,000	? 50,000	72,000	5
Sulu Islands and Basilan	4,739	4,000	? 100,000	104,000	22
Total	296,182	5,985,000	1,000,000	7,001,000	181

For a list of Blumentritt's papers, especially those in which the Negritos are described, consult A. B. Meyer, The Distribution of the Negritos: Dresden, 1899, Stengel, pp. 7-11. The other publications of Dr. Meyer on the Philippine islanders are Vol. VIII, folio series, Royal Ethnographic Museum in Dresden, 1890; Die Philippinen, I,

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Nord Luzon (with A. Schadenberg). Vol. IX, same series, 1893, II, Negritos. Album von Philippinen, Typen, 32 plates, Dresden, Stengel & Co., 1885, 4°. Album von Philippinen, Typen, Nord Luzon (with A. Schadenberg), 50 plates, Dresden, Stengel & Co., 1891, 4°. The map accompanying this paper is from Blumentritt's Razas del Archipelago Filipino, in the Bulletin of the Geographic Society of Madrid, 1890.

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PHILIPPINE TRIBES AND LANGUAGES.

Notwithstanding the rich literature concerning the peoples and languages of the Philippine Archipelago, there is no book or publication in which are catalogued the names of the tribes and the languages, and this appears the more inexcusable, since both Spanish and Philippine writers, with few exceptions, handle these names very carelessly, so that great confusion must ensue. The prevailing bad form in the Philippines, of transferring the name of one people or family (Stamm) to another, who possess similarities of any kind with the first, either in manner of life, or even only in culture grade in the widest sense of the term,[1] has its counterpart in a second bad fashion of making several peoples out of one by replacing the folk name with the tribal names.[2] Only with the greatest pains and thought is it possible to extricate one's self from this labyrinth of nomenclature. After thorough search, I am convinced that many names reported to me must be eliminated, since they owe their existence to mistakes in penmanship or printing, to ridicule, misunderstanding, or error, as I have proved in single instances. However, I have been convinced that by a closer and intelligent exploration of the archipelago, it would not only be possible to make many corrections, particularly in orthography, but that new names would also be added, especially from northern Luzon and from the interior of other islands.

I have introduced into this catalogue all the variations of published names known to me, and briefly the description of tribal locations and reports on their culture grades, especially their religion. Besides the Negritos, I differentiate only Malay peoples (Stamme) in general, because here regard for different principles of grouping and subdividing of the Malay race would appear to serve no good end and perhaps prove troublesome. Obsolete forms of names are carefully marked with a cross (†). Where I, as with the Talaos, Mardicas, and Cafres, take note of foreign peoples or castes on the islands, it is because Spanish authors have erroneously set them down as Philippine. On the other hand, in order to draw attention to a few names customary

[1] Compare the article: Igorrotes. [2] Compare the articles: Quiangan and Silipan.

in the country for races and castes, I have included the following, not belonging here in strict accordance with the title of this article: Castila, Cimarrones, Indios, Infieles, Insulares, Mestizos, Montaraz, Peninsulares, Remontados, and Sangley.[1]

Abacas.—Heathen Malay people, who lived in the dense forests of Caraballo Sur (Luzon). Warlike, probably head-hunters. In the last century they were Christianized, and in their territory the parish of Caranglan (province of Nueva Ecija) was founded, where their descendants lived as peaceful Christians. They have a language of their own, but appear now to be thoroughly Tagalized.

Abra-Igorrotes, *Igorrotes of Abra.*—Collective title for the head-hunters living in the province of Abra (Luzon). Belong for the most part to the Guinaanes.

Abúlon.—The name of a group of wild peoples living in the mountain regions of Zambales. They are perhaps identical with the Zambales and Igorrotes.

Adang.—A folk with a language of their own, who dwell about a mountain of the same name in the province of Ilocos Norte. According to the Augustians P. Buzeta and P. Bravo, they are a mixture of Malays and Negritos. But the first-named element is more prevalent than the second. Their customs resemble those of the Apayaos, their next neighbors; still they do not appear to be head-hunters.

Aëta, see *Negrito.* [Variants: Aheta, Eta, Aita, Aigta, Ita, Atta, Agta, Inagta, Até, Atá, etc., from the Tagalog, *ita*, *itim*, Malay *itam*, Bicol, *ytom*, black].

Agutainos.—Name of the natives of Malay race in the island of Agutaya, in the Cuyo archipelago (province of Calamianes). They have their own dialect, called Agutaino; are Christianized and civilized.

Alibáon, *Alibabáun.*—Not the name of a people, but, it seems, a title of the Moro chief, settled on the bay of Daváo.

Alimut.—This name is cited in the form Igorrotes of Alimut. Supposed to be the tribe of head-hunters who lived in June, 1889, in the lately erected comandancia Quiangan and on the banks of the river Alimut. In this case they should belong to the Mayoyao or Ifugao family (Luzon.)

Altasanes or Altabanes.—In both forms a head-hunting people of northwestern Nueva Vizcaya (Luzon) is known. The correct spelling of the name should be decided. They appear to have no language of their own and perhaps belong to the Mayoyaos and Ifugaos.

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[1]Note.—Blumentritt published his first list in his work entitled Versuch einer Ethnographie der Philippinen, Sixty-seventh Supplement to Petermann's Mittheilungen, 1882; Brinton reports good dictionaries and grammars by Spanish missionaries in Bicol, Bisaya, Ibanac, Ilocan, Pampango, and Tagala. Vocabularies of the Bontoc, Banaul, Ilocan, and Lepanto, of northern Luzon, by Schadenburg, were printed in Verhandlungen der Anthropologischen Gesellschaft in Berlin, in 1889; American Anthropologist, 1898, Vol. XI, p. 302.

FILIPINO SERVANTS—GABINO, LAURENCIO, CRISPINO, AND EUARO.

From Hilder collection, Bureau of American Ethnology, 1900.

Apayaos.—Warlike head-hunters, having their own language and dwelling in the northwestern portion of the province of Cagayan (Luzon) and the adjoining portions of Ilocos Norte and Abra. Buzeta and Bravo report that they are not full-blood Malays, but mixed with Negritos. It must not be forgotten, however, that the Spanish authors have such mixtures ready made. Dark hair is a mixture of Negrito blood; clear skin or yellowish is the result of crossing with Chinese or Japanese. They are partly Christianized. Some Spanish authors declare their language to be Mandaya, but this is improbable.

Variants: Apayos, Apoyaos. [Consult also Vol. VIII, folio series of the Royal Ethnographic Museum in Dresden, by A. B. Meyer, with A. Schadenburg.]

Aripas.—A Malay language, spoken by a peaceable people. They live near Nacsiping and Tubang (Luzon). They are heathen, but a portion of them have been converted to Christianity. With these new Christians the village of Aripa has been founded.

Atás (also *Atáas, Itaas*).—(1) A powerful people of unknown origin, who occupy the head waters of the rivers Dávas, Tugánay, and Libagánum, and their country extends in the eastern portion of the province of Misámis (Mindanao) to the home of the Buquidnones. Little is known about the Atás; they appear to be a mixture of Negritos and Malays. They have a language of their own. Their name means "dwellers in highlands." Variants: Ataas, Itaas. (2) A mixture of Bicols and Negritos in Camarines Sur. [On the confounding of Atás with Aetas, consult A. B. Meyer, 1899, p. 18. The Atás are not pure Negritos.—*Tr.*]

Até.—Name which the Tagbanuas of Palawan (Paragua) give to the Negritos.

Atta.—Dialect spoken by the Negritos of the province of Cagayan (Luzon).

Baganis.—No people is known under this name, as Moya erroneously asserts; it is the title conferred on every Manobo warrior who has slain seven enemies.

Bagobos.—A heathen and bloodthirsty people of Malay derivation and with an idiom of their own. Their home is at the foot of the volcano of Apo (Dávao, in Mindanao). There are detached Christian settlements of them.[1]

Balugas.—(1) Collective title for dark mixed people of Malay and Negrito race, derived from the Tagala word baloga, "black mixed one." Balugas are to be found in several portions of central Luzon. (2) Some authors identify Ætas with Balugas. Cámarca calls the

[1][See A. B. Meyer (1899, p. 18) referring to Album de las diferentes Razas de Mindanao, Fotografias del R. P. Algué, S. J., and Schadenburg, Zeitschrift für Ethnologie, 1885, No. 1. The latter observed crossbreeds between Malays and Negritos among the Atás in the southeast of Mindanao; in the former, two half-breeds, Atás, are represented, and a Bagobo, from Mount Apo, with curly hair.—TRANSLATOR.]

black, woolly savages of the mountains in Camumusan "Negros Balugas," so it seems that in certain regions more or less pure-blooded Negritos were called by this name.

Bánaos.—[In northern Luzon. See A. B. Meyer, with A. Schadenberg, in Vol. VIII, folio series of the Royal Ethnographic Museum, in Dresden.]

Bangal-Bangal.—The Dulanganes are so called by the Moros.

Bangot.—A name conferred on various bands of Manguianes in Mindoro, for the place and mode of life. So called are (1), by the Socol and Bulalacao, those Manguianes who inhabit the plains; and (2) those Manguianes of Mongoloid type who have their dwelling places on the banks of the streams south of Pinamalayan.

Banuaon.—Name of the Manobo tribe from which the Christian settlement of Amporo, in the district of Surigas (Mindanao), was formed.

Barangan.—Name borne by those Manguian hordes who occupy the most elevated stations in the Mangarin Mountains (Mindoro).

Bàtak.—Another name for the Tinitianos, especially those that dwell in the neighborhood of Punta Tinitia and the Bubayán Creek, on the island of Palawan.

Batan.—The inhabitants of Batanes Island were and are enumerated by Spanish authors among the Ibanags or Cagayanes. According to Dr. T. H. Pardo this is incorrect, for their idiom differs not only from the Ibanag but from all others in the Philippines, having the sound of "tsch," unknown elsewhere in the archipelago, and a nasal sound like that of the French "en." They are therefore to be separated from the Cagayanes.

Bayabonan.—Name of a supposed Malay people with a language of their own, living as neighbors to the Gamunanges on the mountain slopes eastward from Tuao, in Cagayan (Luzon). They are heathen and little is known of them save the name.

Beribi.—Manguianes domiciled between Socol and Bulalacao, living on the mountains. (Compare Bangot.)

Bicol.—Autonym of those natives of Malay race who inhabit the peninsula of Camarines in Luzon and some outlying islands. On the arrival of the Spaniards they were somewhat civilized and had a kind of writing. They are Christians, still a section of them live under the names Igorrotes, or Cimarrones, mostly mixed with Negrito blood, in the wilds of Isarog, Iriga, Buhi, Caramuan, etc., wild, and plunged in the deepest heathendom. The official spelling of the name is Vicol. This is clear, since in Spanish the letter v, especially before e or i, is sounded like German b.

Bilanes.—A Malay people occupying, according to latest accounts, a larger area than I have attributed to them in my ethnographic chart of Mindanao, here thoroughly penetrated also by other stocks. The

NATIVES OF DAGUPAN.

From Hilder collection, Bureau of American Ethnology, 1900.

Sarangani islands, lying off the southern point of Mindanao, are inhabited by them. They are heathen, of peaceable disposition. Their language is characterized by the possession of the letter f. The proper form of their name ought to be Buluan, so that they have the same title as the lake. They must then at first have been called Tagabuluan (Taga = whence, from there). (Compare Tagabélíes.)

Variants: Buluanes, Buluan, Vilanes, Vilaanes.

Bisayas.—Officially written Visayas. A Malay people who, on the arrival of the Spaniards, had a culture and an art of writing of their own. They inhabit the islands named after them, besides the northern and the eastern coast of Mindanao, with small intrusions of heathen populations that have become Visayised since the converted tribes—Manobos, Buquidnones, Subanos, Mandayas, etc.—have been taught the Visaya language in the schools. Also Zamboango and Cottabató show Visaya settlements. Among them are to be counted the Mundos. At the time of the discovery they painted (or tattooed) their bodies, on which account they received from the Spaniards the name of Pintados, which stuck to them even till the eighteenth century. They are Christians. Their language is divided into several dialects, of which the Cebuano and Panayano are most important. [Compare Calamiano, Halayo, Hiliguayna, Caraga. Blumentritt places their number at 2,500,000 and upward. Globus, 1896, LXX, p. 213.]

Bontok-Igorrotes.—Collective name of the head-hunting peoples living in the province of Bontok, to whom also the Guinaanes belong.

Bouayanan.—A heathen folk in the interior of Palawan. The name appears to mean "crocodile men."

Buhuanos, *Bujuanos.*—A heathen folk related to the Igorrotes (head-hunters ?), dwelling in the province of Isabela de Luzon. They are warlike in nature.

Bulalacaunos.—A wild people of Malay race (without Negrito mixture ?), having its own (?) idiom. It is to be found in the interior of the northern part of the island of Palawan (Paragua) and in Calamianes islands.

Buluanes, see *Bilanes.*

Bunganannes.—A warlike, head-hunting (?) people, who live in the provinces of Nueva Vizcaya and Isabela de Luzon. Except the name, almost nothing is known of them, and in my view this is not certain.

Bukidnones, *Buquidnones.*—A heathen Malay people living in the eastern part of the district of Misámis (Mindanao), from Ibigan to Punta Divata (the coast is settled chiefly by Visayas), and along the Rio de Tagoloan. Lately they have been partly Christianized. The Spaniards conferred on them the name of Monteses, "dwellers in the mountains," which is a translation of their name.

Bukil, *Buquil.*—Name of different Manguiana tribes of Mindoro: (1) the Manguianes mixed with Negrito blood, whose homes are in

the vicinity of Bacóo and Subaan; (2) those that dwell on the spurs of the mountains between Socol and Bulalacao, and show a pure Malay type; (3) in Pinamalayan they are called Manguianes of Mongoloid type, who inhabit the plains; (4) the Manguianes who dwell on the banks of the rivers are named Mangarin. In view of the fact that Bukil is identical with Bukid, and can be applied only to tribes living in mountain forests, it appears to me that the settlements given under 3 and 4 are incorrect.

Buquitnon.—A "race" by this name, on the island of Negros, until recently unknown (used in La Oceania Española, Manila, August 9, 1889, copied from the Provenir de Visayas). The Buquitnon are said to be a heathen tribe of about 40,000 souls that has its homes on the mountains of Negros, not massed together and not to be distinguished from the Visayas living on the coast. Whether the Carolanos are identical with them is hard to say. The name Buquitnon and also Buquidnon in Mindanao means mountaineers, upland forest dwellers, yet are the Buquitnon, of Negros, and the Buquidnon, of Mindanao, to be strongly distinguished from each other.

Buriks.—Under this name figures a pretended Igorrote people in all publications devoted to the Igorrotes, but Dr. Hans Meyer found that Burik applies to any Igorrote who is tattooed in a certain manner. I did not believe this until a Philippine friend, Eduardo P. Casal, wrote that the Igorrotes in the Philippine Exposition in Madrid, in 1887, had confirmed the statement of Dr. Meyer.

Busaos.—From Spanish accounts the Busaos are a separate division of Igorrotes. Dr. Hans Meyer has reported that the Basaos, or Bisaos, through manner, costume, and custom, are to be numbered rather with the Guiaanes and Bontok-Igorrotes than with the Igorrotes proper.

Cafres.—No native people by this name. The Papuan slaves brought to Manila by the Portuguese at the end of the sixteenth and the beginning of the seventeenth century were so called. [The abolition of slavery under Philip II arrested this traffic.]

Cagayanes.—A Malay language group. Their dwelling places are the Rio Grande de Cagayan (Luzon) from Furao to the mouth, the Babuyanes and Batanes islands, although the people of the last named are by some authors made an independent stock. [Compare Batan.] The Cagayanes had at the time of the Spanish discovery a civilization of their own. They are Christians. Their language is Ibanag. From them are to be sharply discriminated the people of Cagayan, in Mindanao, belonging to the Visayan stock.

Caláganes.—A small Malayan people who live on the Casilaran Creek (Bay of Daváo, Mindanao). Partly converted to Christianity.

Calamiano.—Buzeta and Bravo understand by Calamiano a Visaya dialect which was made up of Tagalog mixed with Visaya and spoken by the Christians of northern Palawan (Paragua) and Calamianes

IGORROTES.

From Hilder collection, Bureau of American Ethnology, 1900.

islands. Père Fr. Juan de San Antonio has preached in Calamiano and composed in it a catechism. The existence of the Calamiano language should therefore be unassailable, but A. Marche has declared that it does not exist.

Calauas (pronounced Calawás).—A Malay people, heathen and peaceable. They live near Malauec, in the valleys of the Rio Chico de Cagayan (Luzon), and on the strip of land called Partido de Itavés. Their language is called Itavés also, but others declare their speech to be identical with the Malauec. The portion of the Calauas who hold the Itavés land are by some authors called Itaveses. I am not sure whether there may not have been a misunderstanding here.

Calibuganes.—So are called in western Mindanao the mixtures of Moros and Subanos.

Calingas.—(1) In northern Luzon, Calinga is the collective designation for "wild" natives, independent heathen, as, in northwestern Luzon, the word Igorrote is applied. (2) This term is specially attached (a) to that warlike people of Malay descent who live between Rio Cagayan Grande and Rio Abulug, and are marked by their Mongoloid type; (b) according to Semper, also the Irayas. [See Die Calingas, by Blumentritt, in Das Ausland, 1891, No. 17, pp. 328–331.]

Camucones, *Camocones.*—Name of the Moro pirates who inhabit the little islands of the Sulu group east of Tawi-tawi, and the islands between these and Borneo; but on the last the name Tirones is also conferred.

Cancanai, *Cancanay.*—Igorrote dialect spoken in the northwest of Benguet.

Caragas.—In older works are so named the warlike and Christian inhabitants of the localities subdued by the Spaniards on the east coast of Mindanao, and, indeed, after their principal city, Caraga. It has been called, if not a peculiar language, a Visaya dialect, while now only Visaya (near Manobo and Mandaya) is spoken, and an especial Caraga nation is no longer known. I explain this as follows: Already at that time newly arrived Manobos and Mandayas were settled who spoke Visaya only imperfectly. This Visaya muddle and the mixture of Visayas and newcomers are to be identified with the Caraga, if in the end, under the first, the Mandaya is not to be directly understood. Variants: Caraganes †, Calaganes (to be distinguished from Caláganes of Davao), Caragueños (now the name of the inhabitants of Daraga la Nueva and Caraga).

Carolanos.—Diaz Arenas so designates the heathen and wild natives who inhabit the mountain lands of Negros, especially the Cordillera, of Cauyau. They appear to be of Malay stock, transplanted Igorrotes from Negros. Practically nothing is known concerning them. Compare Buquitnon.

Castilas.—Native name for Spaniards and other Europeans in the Philippine Islands.

Catalanganes.—A Malay people of Mongoloid type. They live in the flood plain of the Catalangan river (province of Isabela de Luzon). They are heathen and peaceable, and have the same language as the Irayas. [Half Tagala and half Chinese, Brinton, American Anthropologist, 1898, XI, p. 302.]

Cataoan.—A dialect spoken by the Igorrotes of the district of Lepanto, living in the valley of the Abra River.

Catubanganes, or Catabangenes.—Warlike heathen, settled in the mountains of Guinayangan, in the province of Tayábas (Luzon). Through lack of available information nothing can be said about their race affiliations, whether they be pure Malay or Negrito-Malay. They are probably Remontados mixed with Negrito blood and gone wild.

Cebuano.—Dialect Visaya.

Cimarrones.—This characterization ("wild," "gone wild") is given to heathen tribes of most varied affiliations, living without attachment and in poverty, chiefly posterity of the Remontados. [See note by A. B. Meyer, 1899, p. 12.—TRANSLATOR.]

Coyuvos.—The natives of Cuyo archipelago (province of Calamianes), with exception of those who belong to the stock of Agutainos. According to A. Marche, the Coyuvos appear to be Christianized Tagbanuas. For that reason would the idiom called official Coyuvo be the Tagbanua.

Culámanes.—Another name for the Manobos, who live on the southern portion of the east coast of Dávao Bay, the so-called coast of Culaman.

Dadayag.—A Malay people, who occupy the mountain wilds in the western part of Cabagan (province of Cagayan). They have a language of their own and are warlike heathen as well as head-hunters.

Variant: Dadaya.

Dapitan (Nacion de) †.—Title conferred in the sixteenth century on the Visayas of the present comandancia of Dapitán (province of Misámis, Mindanao).

Dayhagang †.—According to S. Mas, before the arrival of the Spaniards, the progeny of Borneo-Malays and Negrito women were so called.

Dulanganes.—This heathen people occupy the southern part of the district of Dávao. The name signifies "wild men." It is not known whether they are pure bloods or Malays with infusion of Negrito blood. I believe that the Malay type predominates. Since they also bear the name of Gulanganes, perhaps, more properly, it is to be suspected that they form with the Mangulangas, Manguangas, and Guiangas (q. v.) a single linguistic group, or at least a stock closely related to them. This is merely a conjecture. By the Moros they are called Bangal-Bangal.

Dumagat.—A name conferred on the Negritos of the northeast coast

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From Hilder collection, Bureau of American Ethnology, 1900.

of Luzon and by older non-Spanish writers on coast dwellers of Samar, Leyte, and Mindoro. Latterly it has come about that the Tagal name Dumagat (from *dagat*, "sea," "dweller on the strand," "skillful sailor," etc.) has been taken for the name of a people. [A. B. Meyer, 1899, p. 11, calls the Dumagates Negrito half-breeds of the island of Alabat, quoting Steen Bille, Reise der Galathea, 1852, Vol. I, p. 451.—TRANSLATOR].

Durugmun.—The Manguianes of Mongoloid type are so called, who occupy the highest portions of the mountains around Pinamalayan (Mindoro). They are called also Buchtulan.

Etas, see *Negritos*.

Gaddanes.—A Malay head-hunting people, with a language of their own, settled in the provinces of Isabela and Cagayan, but especially in the comandancia of Saltan (Luzon). The Gaddanes of Bayombong and Bagabag are Christians; the rest are heathen.

Gamungan, *Gamunanganes.*—A Malay people having their own idiom, and inhabiting the mountain provinces in the eastern and northeastern portions of Tuao (province of Cagayan, Luzon). They are heathen.

Guiangas, *Guangas.*—A Malay people in the northeastern and northern part of Dàvao (Mindanao). They are heathen and do not differ greatly from the Bagobo, their neighbors; on the other hand, according to the accounts of the Jesuit missionaries, their speech differs totally from those of the heathen tribes near by, and for that reason it is difficult to learn. On account of their wildness they are much decried. The variants, Guanga and Gulanga, which mean "forest people," give rise to the bare suspicion that they are a fragment of the little-known tribe who, according to location, lived scattered in southern Mindanao under the names: Manguangas, Mangulangas, Dulanganes.

Guimbajanos (pronounced Gimbahanos).—The historians of the seventeenth century, under this title, designated a wild, heathen people, apparently of Malay origin, living in the interior of Sulu Island. Their name is derived from their war drum (guimba). Later writers are silent concerning them. In modern times the first mention of them is by P. A. de Pazos and by a Manila journal, from which accounts they are still at least in Carodon and in the valley of the Loo; it appears that a considerable portion of them, if not the entire people, have received Islam.

Variants: Guinbajanos, Guimbanos, Guimbas, Quimpanos.

Guinaanes (pronounced Ginaanes).—A Malay head-hunting people inhabiting the watershed of the Rio Abra and Rio Grande de Cagayan (Luzon), as well as the neighboring region of Isabela and Abra. They are heathen; their language possesses the letter *f*.

Variants: Guianes, Ginan, Quinaanes, Quinanes. [See A. B. Meyer,

with A. Schadenberg, Volume VIII, folio series, Royal Ethnographic Museum, Dresden, 1890.]

Gulanga, see *Guianga*.

Gulanganes, see *Dulanganes*.

Halaya †.—A Visaya dialect spoken in the interior of Panay.

Haraya.—A Visaya dialect spoken in the interior of the island of Panay, nearly identical with the foregoing.

Hiliguayna †.—A Visaya dialect spoken on the coast of the island of Panay. Variants: Hiligueyna, Hiligvoyna.

Hillunas, *Hilloonas*, see *Illanos*.

Ibalones †.—Ancient name of Bicols, especially those of Albay.

Ibanag.—Name of the language spoken by the Cagayanes. They possess the letter *f*.

Idan, *Idaan*.—The Idan, sought by non-Spanish authors on the islands of Palawan (Paragua) and Sulu, have not been found.

Ifugaos.—A dreaded Malay head-hunting people who inhabit the provinces of Nueva Vizcaya and Isabela and the lately formed comandancia of Quiangan. To them belong the Quianganes, Silipanos, etc. They are heathen. Their language possesses the sound of *f*.

Ifumangies.—According to Diaz Arenas, this name applies to a tribe of Igorrotes who were then (1848) in the province of Nueva Vizcaya. The *f* in their name leads to the suspicion that they are Ifugaos.

Ibilaos.—A Malay head-hunting people, having also apparently Negrito blood in their veins. They are heathen and inhabit the border lands of Nueva Vizcaya and Nueva Ecija.

Igorrotes.—With the name Ygolot the first chroniclers characterized the warlike heathen who now inhabit Benguet, therefore the pure Igorrotes. Later, the name extended to all the head-hunters of northern Luzon; still later it was made to cover the Philippine islanders collectively, and to-day the title is so comprehensive that the name Igorrote is synonymous with wild. According to Hans Meyer, the name applies only to the Igorrotes of Lepanto and Benguet, who speak the dialects Inibaloi, Cancanai, Cataoan, and a fourth (Suflin?), that of the Berpe Datá.

Variant: Ygolot, Ygulut.

[A Chinese-Japanese Tagala group. Brinton, Amer. Anthropologist, 1898, XI, p. 302. Consult A. B. Meyer, with A. Schadenberg, in Vol. VIII, folio series of the Royal Ethnographic Museum, in Dresden, 1890; and Die Igorroten von Pangasinan, F. Blumenbritt, by Mittheil. T. K. K. Geogr. Gesellschaft in Wien, 1900, hft 3 u. 4.]

Ilamut.—Name of an Igorrote tribe always mentioned together with that of Altsanes. If this tribe really exists, its home is in the Cordilleras which separate Benguet from Nueva Vizcaya, and is to be sought, indeed, in the last-named province, especially in Quiangan. They may be identical with the Alimut.

MORO CHIEFS.

From Hilder collection, Bureau of American Ethnology, 1900.

Ilanos, Illanos.—The Moros dwelling in the territory of Illano, Mindanao. Their name should be connected with Lanao, "lake," since their land incloses Lake Dagum, or Lanao. This conjecture is strengthened through the names Lanun, Lanaos, Malanaos, existing in the neighborhood. [Consult A. B. Meyer, 1899, p. 18, on the Hillunas, "Correcting Quatrefages and Harny, Crania Ethnica," 1882, p. 179, where they are called Negrito.—TRANSLATOR.]

Ileabanes.—According to Diaz Arenas there existed an Igorrote tribe of this name (1848) in the province of Nueva Vizcaya.

Ilocanos.—A Malay people, with language of their own. At the discovery they had their peculiar culture and an alphabet. They inhabit the provinces of Ilocos Norte, Ilocos Sur, Union, and form the civic population of Abra, whose Tinguian peasants they Ilocanise. Since they are fond of wandering, their settlements are scattered in other provinces of Luzon, as Benguet, Pampanga, Cagayan, Isabela de Luzon, Pangasinan, Zambales, and Nueva Ecija. They are to be found as far as the east coast of Luzon. They are Christians and civilized. [The Ilocanos of the northwest are markedly Chinese in appearance and speech. Brinton, Amer. Anthropologist, 1898, XI, p. 302. Consult A. B. Meyer, with A. Schadenberg, in Vol. VIII, folio series, of the Royal Ethnographic Museum in Dresden, 1890.]

Ilongotes.—A Malay people of apparent Mongoloid type, inhabiting the borders of Nueva Vizcaya, Isabela, and Principe, and known also in Nueva Ecija. They are bloodthirsty head-hunters. [In the eastern Cordillera, a rather pure but wild Tagala horde. Brinton, American Anthropologist, 1898, p. 302.]

Indios.—Under this title the Spanish understand the non-Mohammedanized natives of Malay descent, especially those Christianized and civilized.

Infieles.—Heathen, uncivilized peoples of Malay descent; were so named by the Spaniards.

Inibaloi.—Name of the dialect spoken by the Igorrotes Agnothales.

Insulares.—Spaniards born in the Philippine Archipelago.

Irapis.—After Mas, a subdivision of Igorrotes.

Irayas.—A Malay people mixed with Negrito blood, who dwell south of the Catalanganes and in the western declivities of the Cordillera of Palanan (Luzon). They speak the same language as the Catalanganes, and are likewise heathen. Their name seems to mean "dwellers on the plains," "owners of plains." To them the collective name Calinga is applied. [Consult A. B. Meyer, with A. Schadenberg, in Vol. VIII, folio series, of the Royal Ethnographic Museum in Dresden, 1890.]

Isinays (*Isinayas, Isinay*).—In the eighteenth century the heathen population of the then mission province of Ituy were so called, which includes the present communities of Aritao, Dupax, Banibang, Bayombong (Nueva Vizcaya, Luzon). It is not certain whether they are a separate people or are identical with Gaddanus, Italones, or Ifugaos.

Italones.—A head-hunting Malay people who inhabit the mountain wilds of Nueva Vizcaya (Luzon). They are heathen, only a small part of them having embraced Christianity.

Ita, see *Negritos.*

Itaas, see *Atás.*

Itanegas, *Itaneg*, *Itaveg.* See *Tinguianes.*

Itavés.—So used the language of the Calauas to be called; still there are authors who affirm that these two are different. Nothing certain is known concerning this name, which is also written Itaués, Itanes. From latest accounts, this is a dialect of Gaddan.

Itetapanes (*Itetapaanes*).—According to Buzeta and Bravo, a head-hunting Malay people mixed with Negrito blood, living on the western borders of Isabela de Luzon and perhaps also in Bontok.

Ituis.—According to Mas, a subdivision of Igorrotes. Nothing more is known. Compare Isinays.

Ivanha.—Form of Ibanag.

Joloanos.—The Moros of Sulu.

Jacanes, see *Yacanes.*

Kianganes, see *Quianganes.* [Meyer has Kingianes, 1899.]

Jumangi, see *Humangi.*

Humanchi.—Heathen people of central Luzon (?); written Jumangi.

Latan.—Another name for the Manguianes who inhabit the plains of Mangarin (Mindoro).

Lanaos, see *Illanos* and *Malanaos.*

Lanun, see *Illanos.*

Laút, see *Sámales-Laút.*

Lingotes, see *Ilongotes.*

Loacs.—Not a separate people, but the name of a very poor Tagacaolo tribe who dwell in the mountain forests of San Augustín Peninsula (Mindanao).

Lutangas.—A Mohammedan mixed race of Moros and Subanos, who inhabit the island of Olutanga and the adjacent coast of Mindanao.

Lutàos, *Lutayos.*—Moros of the district of Zamboango and frequently called Illanos. It appears to be the Hispanicized form of the Malay Orang-Laút.

Maguindanaos (*Mindanaos*).—Another of the Moros who inhabit the valley of the Rio Palangui or Rio Grande de Mindanao. To them belong also the Moros of Sarangani Islands and partly those of Dávao Bay. [See The Maguindanaos, by Blumentritt, Das Ausland, 1891, No. 45, pp. 886-892.]

Malanaos.—Common name of those Moros, specially of Ilanos, who inhabit the shores of Malanas Lake (Mindanao).

Malancos.—A tribe alleged to be settled in Mindanao, but the name is plainly an error for Malanaos.

Malauec.—In an anonymous author of "Apuntes interesantes sobre

NEGRITOS OF PROVINCE OF MARAVALES, LUZON.

From Hilder collection Bureau of American Ethnology 1900.

las islas Filipinas," (Madrid, 1870), and quoting V. Barrantes, the common language of commerce of Malaneg (province of Cagayan) is so called; but on the last named also (only) Ibanag is spoken. Other authors understand by this the language of the Nabayuganes or that of the Calaluas. The suspicion is also well founded that by Malauec is meant a lingua franca made up from various tongues. It is difficult to extract the truth from these conflicting accounts.

Mamánuas.—A Negrito people inhabiting the interior of Surigáo Peninsula (northeast Mindanao). Semper and others have called them a bastard race, but the Jesuit missionaries, who have turned a great number of them to Christianity, call them "los verdaderos negritos aborigines de Mindanao." [On the Mamánuas consult A. B. Meyer, Distribution of the Negritos, Dresden, 1899, p. 17—TRANSLATOR.]

Mananapes.—A heathen people alleged to dwell in the interior of Mindanao, possibly a tribe of Buquidnones or Manobos.

Mandaya.—In some authors this is the name of the Apayas language, which is somewhat doubtful.

Mandayas.—A bloodthirsty Malay and bright-colored head-hunting people in the comandancia of Bislig and the district of Dávao (Mindanao). They are heathen, partly converted to Christianity by the Jesuits.

Mancayaos.—Not a separate people, but merely the warriors among the Manobos, who carry lances.

Manguangao.—Under this name the Jesuits near Catel (comandancia Bislig, east Mindanao) characterized the heathen inhabitants. By the same authors the heathen living on the upper tributaries of the Rio Agusan, Rio Manat, and Rio Batutu are called Manguangas and Mangulangas (forest people). Père Pastells identifies Manguangao and Mangulangas and says that they inhabit the head waters of the Rio Salug (which does not agree with Montano's communications). From all which it results that Manguangas is a collective name and stands in connection with that of the Dulanganes and Guiangas. Perhaps all the folk named belong to one people. They are heathen and of the Malay race.

Manguianes.—The heathen, unaffiliated natives inhabiting the interior of Mindoro, Romblon, and Tablas. Manguian (forest people) is a collective name of different languages and races. According to R. Jordana, the Manguianes of Mindoro are divided into four branches, one of which, Bukil or Buquel, is a bastard race of Negritos, while a second in external appearance reminds one of Chinese Mestizos, and on that account it is to be regarded as a Mongoloid type. The other two are pure Malay. To the name Manguianes (which calls to mind Magulangas) specially belong only (1) those Manguianes who live in the mountains near Mangarin and (2) only those between Socol and Bulacao who dwell on the river banks. The remaining tribes bear different

names—Bangot, Buquil, Tadianan, Beribí, Durugmun, Buctulan, Tiron, and Lactan. The Manila journals speak of Manguianes of Paragua (Palawan). These have naught to do with those of Mindoro, since on Paragua this title in its meaning of "forest people" is applied to all wild natives of unknown origin.

Mangulangas, see *Manguangas*.

Manobos.—A Malay head-hunting people, sedentary, chiefly in the river valley of middle Rio Agusan (district of Swigao), as well as at various points in the district of Dávao (Mindanao). A considerable portion have been converted through Jesuit missionaries; the rest are heathens. The correct form of the name is Manuba, or, better, Man-Suba; that is, "river people." The name in earlier times was frequently extended to other heathen tribes of Mindanao. [On the relationship of Manobos with Indonesians, an allophyllic branch of the white race, see remark of Brinton on Quatrefages and Hamy in American Anthropologist, 1898, Vol. XI, p. 297.]

Mardicas.†—In the war between Spain and Holland (seventeenth century) the mercenaries from the Celebes, Macassars, and the Moluccas were so called.

Maritimos.—The Remontados, who inhabit the islands and rocks on the north coast of Camarines Norte. [The island of Alabat, on the east coast of Luzon, is peopled by Negrito half-breeds, called Dumagat and Maritimos.—A. B. MEYER.]

Mayoyaos.—A Malay head-hunting people, who inhabit the southwest corner of Isabela and the northwest angle of Nueva Vizcaya. The Mayoyaos belong, without doubt, to the Ifugao linguistic stock.

Mestizo.—Mixture. Mestizo Peninsulo, Mestizo Español, Mestizo Privilegiado, mixture of Spaniards and natives; Mestizo Chino, Mestizo Sangley, Mestizo Tributante, or mixture of Chinese with natives.

Mindanaos, see *Maguindanaos*.

Montaraz, *Montesinos.*—Collective name for heathen mountain peoples and also for Remontados.

Monteses.—(1) Collective name in the same sense as Montaraz; (2) Spanish name for Buquidnones and Buquitnon.

Moros.—Mohammedan Malays in the south of the archipelago, southern Palawan, Balabac, Sulu Islands, Basilan, western and partly the southern coast of Mindanao, as well as the territorio illano and the Rio Grande region and the Sarangani islands. Various subdivisions have been recognized: Maguindanaos, Illanos, Samales, Joloanos, etc.

[In the sixteenth century, 1521–1565, the Moros of Brunei (Borneo) propagated Islam among the brown race of the Philippines.]

Mundos.—Heathen tribes inhabiting the wilds of Panay and Cebu. Buzeta and Bravo regard them as Visaya Remontados gone wild. Baron

TAGAL WOMAN, MANILA.

From Hilder collection, Bureau of American Ethnology, 1900.

Hügel says that their customs resemble those of the Igorrotes. This is a contradiction, in which more stress is laid on the testimony of the two Augustinians, that Mundos is misused as a collective name, like Igorrotes, Maguianes, etc.

Nabayuganes.—A warlike, head-hunting people of Malay origin, dwelling westward from Malaneg or Malanec (province of Cagayán). They appear to be related to the Guinaanes.

Negrito.—(Native names: Aëta, Até (Palawan), Eta, Ita, Mamánua (northeast Mindanao), old Spanish name, Negrillos, Negros del País). The woolly-haired, dark-colored aborigines of the land who, in miserable condition, live scattered among the Malay population in various parts of Luzon, Mindoro (?), Tablas, Panay, Busuanga (?), Culion (?), Palawan, Negros, Cebu, and Mindanao. There are supposed to be 20,000 of them. They are also spoken of under the word Balugas. The Negrito idiom of the province of Cagayan is called Atta.

["It may be regarded as proved that Negritos are found in Luzon, Alabat, Corregidor, Panay, Tablas, Negros, Cebu, northeastern Mindanao, and Palawan. It is questionable whether they occur in Guimaias (island south of Panay), Mindoro."—A. B. Meyer, 1899, p. 19.

Upon the Negritos, consult A. B. Meyer: The Negritos of the Philippines, publications of the Royal Ethnographic Museum of Dresden, 1893, Vol. IX, 10 pl., folio; also, The Distribution of the Negritos, Dresden, 1899; Montano, Mission aux Philippines, 1885; Marche, Luçon et Palaouan, 1887.—TRANSLATOR.]

Palauanes.—Another name for Tagbanuas, perhaps their original name, from which the island of Paragua got the name Isla de los Palauanes. The *u* in these names equals the German *w* and the English *v*.

Pampangos.—A Malay language group who, at the arrival of the Spaniards, possessed a civilization and method of writing of its own. The people inhabit the province of Pampanga, Porac, and single locations in Nueva Ecija, Bataán, and Zambales. They are Christians.

Panayano.—Dialect of Visaya.

Pangasinanes.—A Malay language group which already at the time of the conquest had its own civilization and writing. The people inhabit the larger part of Pangasinan and various localities of Zambales, Nueva Ecija, Benguet, and Porac (?). They are Christians.

Panguianes, see *Punguianes.*

Panuipuyes (*Panipuyes*).—A tribe of so-called Igorrotes. Their dwellings were to be sought in the western portion of Nueva Vizcaya or Isabela de Luzon.

Peninsulares.—European Spaniards.

Pidatanos.—In the back country of Libúngan, therefore not far from the delta of the Rio Grande de Mindanao, dwell, as the Moros

report, a heathen mountain people bearing the name of Pidatanos. Probably they have not a separate language, but belong to one of the well-known families, perhaps the Manguangas.

Pintados, see *Visayas*.

Pungianes.—Tribe of Mayoyaos.

Quianganes.—(Pronounced Kianganes). A head-hunting people, settled in 1889 in the comandancia of Quiangan (Luzon), for that reason belonging to the Ifugao linguistic family. [See, Die Kianganes (Luzon), by Blumentritt, Das Ausland, Stuttgart, 1891, pp. 129–132.]

Quimpanos, see *Quimbazanos*.

Quinanes, see *Guinaanes*.

Remontados.—Name of civilized natives who have given up the civilized life and fled to the mountain forests.

Samales.—(1) A small Malay people living on the island of Samal in the Gulf of Dávao (Mindanao). They are heathen, but they are partly converted to Christianity. (2) Another name for the Moros who inhabit the islands lying between Basilan and Sulu.

Sámales-Laút.—The Moros who inhabit the coasts of Basilan. Compare Samales (2).

Sameacas.—Some authors speak of them as the aborigines of Basilan pushed back into the interior by the Moros. According to Claudio Montero y Gay, they are heathen.

Sangley.—A name borne in early times by Chinese settled in the Philippines. Going into disuse.

[It is thought that the Chinese were not numerous on the islands until the settlement of the Spaniards had established commerce with Acapulco, introducing Mexican silver, greatly coveted by the Celestials.—TRANSLATOR.]

Sanguiles.—(1) Until most recent times by this name was understood a people in the little-known southern part of the district of Dávao (Mindanao). The Jesuit missionaries have found no people bearing this name; it seems, therefore, that Sanguiles was a collective title for the Bilanes, Dulanganes, and Manobos, who occupied the most southern part of Mindanao, the peninsula of the volcano Sanguil or Saraganá. (2) Moros Sanguiles means those Moros who dwell in the part of the south coast of Mindanao (district of Dávao) lying between the Punto de Craan and the Punta Panguitan or Tinaka. They also appear to have received their name from the volcano of Sanguil.

Silipanes.—A heathen head-hunting people having its abode in the province of Nueva Vizcaya (and comandancia Quiangan). It belongs to the Ifugas linguistic family. [Consult A. B. Meyer, with A. Schadenberg, in Vol. VIII, folio series, Royal Ethnographic Museum in Dresden, 1890.]

Subanos.—(Properly Subánon, "river people.") A heathen people of Malay extraction, who occupy the entire peninsula of Sibuguey

TINGUIANES WARRIORS, LUZON.

From Hilder collection, Bureau of American Ethnology, 1900.

(west Mindanao), with exception of a single strip on the coast. [See Die Subanos (Mindanao), by Blumentritt, Das Ausland, Stuttgart, 1891, pp. 392–395.]

Suflín.—An Igorrotes dialect. The *f* in the name would hint at Guinaanes or Ifugaos. The official nomenclature in 1865 so characterizes a dialect spoken in Bontok.

Tabanus, see *Tagbanuas.*

Tadianan.—Another name for those Mongoloid Manguianes who live in the mountain vales of Pinamalayan (Mindoro).

Tagabaloyes.—In a chart of the Philippines for 1744, by P. Murillo Velardi, S. J., this name is to be seen west of Caraga and Bislig (Mindanao). English authors speak of the Tagabaloyes, Waitz mentions their clear color, and Mas calls them Igorrotes. Others add that they were Mestizos of Indians and Japanese, and more fables to the same effect. Their region has been well explored, but only Manobos and Mandayas have been found there. The last named are clear colored, so Tagabaloyes seems to be another name for Mandayas. The name sounds temptingly like Tagabelíes.

Variants: Tagbalvoys, Tagabaloyes, Tagobalooys, etc.

Tagabawas.—Dr. Montano reports that this is not a numerous people and that it is made up of a mixture of Manabos, Bagobos, and Tagacaolos. Their dwelling places are scattered on both sides of Dávao Bay (Mindanao), especially near Rio Hijo.

Tagabelíes.—A heathen people of Malay origin, living in the region between the Bay of Sarangani and Lake Buluan (Mindanao). Since they call themselves Taga-bulú (people of Bulu), it is suspected that they, like the Buluanes or Bilanes, derive their name from the lake mentioned.

Tagabotes.—A people of Mindanao mentioned in the Ilustracion Filipina (1860, No. 17).

Tagabulu, see *Tagabelíes*, also *Tagabuli.*

Tagacaolos.—A Malay, heathen people. Their settlements are scattered among those of other tribes on both sides of the Gulf of Dávao (Mindanao). Compare also Loac. Their name Taga-ca-olo would mean "dwellers on the river sources."

Variant: Tagalaogos.

Tagalos, *Tagalog* (elsewhere *Tagalas*).—A Malay people of ancient civilization, possessing already an alphabet in pre-Spanish times. They are Christians, and inhabit the provinces and territory of the following: Manila, Corregidor, Cavite, Bataán, Bulacán, Batangas, Infanta, Laguna, Mindoro; in less degree, Tayabas, Zambales, Nueva Ecija, Isabela, and Príncipe. They form, with the Visayas and Ilocanes, the greater part of the native population, as well by their numbers as by their grade of culture. Their language is called Tagalog. [See Brinton, American Anthropologist, 1898, XI, pp. 303–306.]

Tagbalvoys, see *Tagabaloyes*.

Tagbanuas.—A Malay people mixed with Negrito blood. They are heathen, with exception of the Calmianos, and appear to have formerly stood on a higher culture grade, for A. Marche found them in possession of an alphabet of their own. They inhabit the island of Palawan (Paragua) and the Calamianes. The Moros of Palawan are partly Tagbanuas. Variant: Tabanuas. [See Dean Worcester, Philippine Islands, 1898, p. 99.—TRANSLATOR.]

Tagobalooys, see *Tagabaloys*.

Talaos.—This newly christened name belongs to no Philippine people, but is the Spanish title of the inhabitants of the Dutch island Talaut. They come to southern Mindanao to purchase provisions.

Tandolanos.—Wild natives living on the west coast of Palawán, between Punta Diente and Punta Tularan. As they are also called Igorrotes they appear to belong to the Malay race.

Teduray, see *Tiruraye*s.

Tegurayes.—A variant form of Tirurayes.

Tinguianes.—A heathen people of Malay origin and peaceable disposition. Their home is the province of Abra and the bordering parts of Ilocos Norte and Ilocos Sur. They have also villages in Union (Luzon). The Tinguianes converted to Christianity are strongly Ilocanised. Variants: Itanega,† Itaneg,† Itaveg,† Tingues.† [See Brinton's note on the identification of Tinguianes with Indonesians, an allophyllic branch of the white race, by Quatrefages and Hamy. American Anthropologist, 1898, Vol. XI, p. 297. Consult A. B. Meyer, with A. Schadenberg, in Volume VIII, folio series, Royal Ethnographic Museum, in Dresden, 1890.]

Tinitianes.—A heathen people, probably of Malay origin. They inhabit a strip of land north of Bubayan Creek, Palawan. [A. B. Meyer, 1899, pp. 9, 19, quotes Blumentritt's The Natives of the Island of Palawan and of the Calamanian Group (Globus, Braunschweig, 1891, Vol. LIX, pp. 182, 183), to the effect that the Tinitianes are probably only Negrito half-breeds.—TRANSLATOR.]

Tinivayanes.—Moros (?) or heathen (?). Said to live along the Rio Grande de Mindanao.

Tino.—Name of the language of the Zambales.

Tiron.—Separate name of those Manguianes of Mindoro who inhabit the highest mountain regions in the surroundings of Naujan.

Tirones.†—The Moros pirates of the province of Tiron in Borneo and the islands near by are so called.

Tirurayes.—A peaceable heathen people of Malay origin. They live in the district of Cottabato, in the mountains west of the Rio Grande de Mindanao. The Christian Tirurayes live in Tamontaca. Variants: Teduray, Tirulay.

Vicol, see *Bicol*.—[Vicol is preferable.]

TINGUIANES OF PROVINCE OF ABRA, LUZON.

From Hilder collection, Bureau of American Ethnology, 1900.

Vilanes, see *Bilanes*.—[Vilanes is preferable.]

Visayas, see *Bisayas*.—[This spelling is preferable to Bisayas.]

Ygolot, see *Igorrotes*.

Ycanes.—According to P. P. Cavallería, S. J., the Moros dwelling in the interior of the island are so called. [Compare Jacanes, Sameacas, and Sámales-Láutes.]

Yrgades, see *Gaddanes*.

Zambales.—A civilized, Christianized people of Malay origin, living in the province of the same name. Those called by different writers Igorrotes de Zambales, Cimarrones de Zambales, are posterity of Remontados. Their language is Tino.

THE SCULPTURES OF SANTA LUCIA COZUMAHUALPA, GUATEMALA, IN THE HAMBURG ETHNOLOGICAL MUSEUM.[1]

By Herman Strebel.

The scientific committee of the Hamburg-America celebration, planned for 1892, had intended to hold an exhibition, and Director Bolau, Mr. L. Friedrichsen, Superintendent C. W. Lüders, Dr. Michau, and the author of the present paper were associated into a subcommittee for that purpose. As everybody knows, the cholera broke out and rendered this promising part of the programme impracticable. It thus became necessary to make some other disposition of so much of the material collected as had been either donated or purchased. It had all along been intended that our scientific institute should profit by such things, and so it happened, owing to the excellent financial management of the whole undertaking by the general committee, that our ethnological museum received the gift of a series of plaster casts whose originals are preserved in the Royal Ethnological Museum of Berlin.

Those originals came from Santa Lucia Cozumahualpa, which is a place in the province of Escuintla, in Guatemala, on the southern or Pacific slope of the Cordilleras, below the Volcano del Fuego. The locality seems to have been settled after 1850 by Cakchiquels from the high plateau, who commenced coffee plantations here. In 1860 the clearing of a piece of forest brought to light a number of sculptured blocks of stone. The commandant of the place, Mr. Pedro de Anda, considered the discovery of sufficient importance to be brought to the notice of the Guatemalan Government, and a commission of inspection was dispatched to the spot. Unfortunately, their thorough report was never published, and has since not been found in the archives. Two years later, in 1862, the Austrian traveler, Dr. Habel, in the course of his extended explorations, arrived at Santa Lucia, and made drawings and descriptions of the antiquities that had been found up to that date. These were first published at the instance of Prof. Ad. Bastian, director of the Royal Ethnological Museum of Berlin, in Vol. XXII of the Smithsonian Contributions to Knowledge, in 1878. Bastian had

[1] Translated from the Annual of the Hamburg Scientific Institute, Vol. XI, 1893.

been informed by Habel, when the latter passed through Berlin, of the existence of important ruins in Guatemala, and when, in 1876, during his own American travels, his attention was called, in Guatemala, to the discovery in Santa Lucia, and he had seen it himself, he recalled what had been told him by Habel, who had meantime disappeared. Bastian, however, did not rest until he had traced him to New York and had taken the necessary steps to have his report and drawings published by the Smithsonian Institution. Before leaving Santa Lucia, Bastian, promptly recognizing the importance of the discovery, had purchased of the owner of the land for the Berlin Museum all antiquities which had been or should thereafter be discovered, an act for which Americanistic investigation must be particularly grateful to him. The most difficult part of the task still remained, however, to be performed, namely, the transportation of the treasures to the port of San José for shipment. Bastian had the happy thought of securing the cooperation of Dr. Hermann Berendt, who had been settled for some years in Guatemala, and who, being well acquainted with the country and the people, and at the same time an eminent linguistic and archæological Americanist, was as much disposed to further the scientific research as he was competent to cope with its practical difficulties. (See my biographical notice of Berendt in the Globus, Vol. LIX, No. 22.) The matter was taken in hand with the aid of Engineers Napp and Au, but greater difficulties arose than had been anticipated, for it turned out that the majority of the blocks were too heavy to be drawn to the coast by oxen over the rough roads. However, since they were only sculptured on one face it was ultimately decided that the greater part of their thickness should be sawn away. For this apparatus had to be secured and labor performed, and it is not surprising that it was not until the end of the year 1880 that the material was ready for shipment and that it was only in August, 1881, that it reached the Berlin Museum in good condition. Berendt, unhappily, did not live to take satisfaction in this final result. In the year 1878 an old complaint of his was so aggravated by the exertions involved in many journeys between his home and Santa Lucia that he died in April of that year, and Americanist research thus lost one of its truest adherents. Bastian, in his paper on "The Guatemalan Sculptures" in the publications of the Royal Berlin Museum for 1882 has given extracts from Berendt's letters relating to this industrious period so trying to the patience of all parties. For further information concerning the entire archæological find in that neighborhood, of which only a portion, though no doubt the most important portion, has been transferred to Berlin, the reader may consult the above-named works of Bastian and of Habel, as well as papers by Gustav Eisen in the memoirs of the California Academy of Sciences, Vol. II, No. 2, and of Dr. Ed. Seler in the journal Il Centenario, No. 26, Madrid, 1892. In these works the ruins are consid-

ered collectively. Eisen's memoir treats of the materials still remaining at Santa Lucia and its environs, of which Berendt sent descriptions and drawings to Bastian, the publication of which is much to be desired. Seler gives remarkable explanations of the principal pieces found at Santa Lucia.

A part of these treasures adorning the Berlin Ethnological Museum have now become a gift to our own in the form of excellent plaster casts. They are to be found on the north side of the upper story of the museum of natural history in the prehistoric collection. The author of the present account of them has deemed it incumbent upon him to publish something concerning their significance, so that the public, by understanding them better, may be led to take more interest in them.

The discoveries at Santa Lucia are remains of an important settlement which must have been destroyed long before 1522, the date of the plundering of the country by Alvarado; for had it not been so, we should have had notices about it from the Spaniards. That this destruction must have been a forcible one is proved by the disorderly position of the remains hitherto discovered, especially such as plainly formed parts of buildings. This is shown by the plan of the site published by Bastian in the Berlin Zeitschrift für Ethnologie, Vol. VIII, p. 322. The rank tropical vegetation had covered these remains and given them over to oblivion, until, centuries later, chance brought them to light again, and thus gave us a glimpse into a civilization previously quite unknown.

To the question, what race produced these monuments, no certain answer can be given. Their type is a new one to us. Comparing them with those of the Maya civilization, we find that they present differences so fundamental that they must be of another origin. The anthropological type of the figures exhibited, at least of those which represent inhabitants of Santa Lucia, contrasts decidedly with those of the Maya sculptures, while the hieroglyphics characteristic of the latter are here wholly wanting. We are thus driven to seek their origin among the Nahoa peoples who formerly inhabited old Mexico, but of whom a part, as we learn from the remains, wandered south as far as the shores of the South Sea and into Central America, everywhere forming settlements of longer or shorter duration. Remains of these settlements have already been found. They are ascribable, with some degree of certainty, to the Nahoa civilization. It is, however, necessary to allow for the influence of the new conditions of life which the wanderers must have encountered, as well as for that of contact with foreign civilizations, in modifying original characteristics of their own and in introducing new elements—effects which must have become more marked the longer and the more undisturbedly those causes acted. Such must have been the case with the settlers of Santa Lucia, since

the magnificence of the remains discovered bespeaks of itself a long period of prosperous development. The main outlines of the Nahoa civilization are preserved; but new elements, some of them attributable to Maya civilization, have been introduced and have been worked over in a peculiar way, so that a new type has been evolved. Before entering into details we may as well glance at the age of the Santa Lucia monuments. For this estimation definite data are afforded by Maya remains which narrate incursions of foreign hordes. According to these records the settlement must have taken place six or seven centuries ago that is, in the thirteenth century of our era. It may have subsisted for a long time and may have been ultimately destroyed in contests with aboriginal Chakchiquels, Quichés, and other Maya races, which contests we likewise find recorded.[1]

Taken as a whole, the monuments of Santa Lucia exhibit, both in their technique and in their artistic conception and elaboration, a higher development than the corresponding productions of old Mexico, and approach the leading works of the Maya civilization, by which they may have been stimulated and aided. The proportions of the human body and the representation of its members are more correct than in the Mexican sculptures, and the bas-relief is executed with great taste. The pieces here considered generally represent priests engaged in performing rites of worship to different divinities; and the head of the divinity is so elaborated as to constitute the main object of the sculpture. The sex of the divinity is not indicated, at any rate not to our present means of discrimination. It may be that the mode of wearing the hair or the ornaments with which the divinities are loaded indicate their sex; but not having as yet found any figures of men and women with which they can be compared we are left in the dark upon this point. The particular function of each divinity is also difficult to determine, since attributes are employed with which we have no other acquaintance; so that it is only in some particular cases that analogy affords any clue to the nature of the god or goddess. Some details of the representations may now be considered in a general way.

We several times find close to the ornaments characterizing the divinity, and invariably before the mouths of the priests, as well as here and there among lifeless things, a sign in the form of a variously curved fillet, usually like an interrogation mark, with sundry double knot-like side excrescences. This sign must be equivalent to the tap-like sign with a bent end which is common in Nahuatlan represen-

[1] If the first settlement was in the thirteenth century, and if it endured long enough to develop an original type of civilization and to become rich enough to erect magnificent monuments, it could not well have been destroyed without a long struggle, and there would seem to be scant time for every vestige and memory of it to disappear before 1522.—TRANSLATOR.

tations; and this latter we know certainly stands for smoke, breath, speech, or song. Whether or not in the present case any differentiation of signification is attached to the different shapes of the curve, the number of binodes, or the differences between the persons or things to which the sign is attached can not be decided at the outset. Not so clear is a shape which, starting from the front of the thick, stiff girdle of the priest, runs upward to a point and mounts in waves toward the divinity. It looks like the appearances of flames which, for example, surround the disk of the sun, and are, no doubt, meant for flames, although these latter are much smaller. It can hardly be fluttering ribbons ornamenting the girdle. Yet what a mounting flame should signify in this position, plainly connected with the stiff girdle, is more than we can guess at the outset. We further find occasional simple disks which in Nahuatlan representations are undoubtedly numerals, each disk being a unit. Along with these, or alone, there are also larger disks, some of them with high rims, bearing various devices. These recall the way in which, in Nahuatlan representations generally, days or periods of time, as well as names, are sometimes presented, although in the details the resemblance ceases. In the costume of the priests the following details are noticeable: Most of the body is bare; for excepting the rich and varied ornaments for head, ears, and neck the common breech clout of all old American peoples is his principal article of clothing. It is in the form of a long band going around the waist and between the legs, and forming a girdle. The ends hang down before and behind, and have tassels, or fringes. In addition to this, the priest wears a broad girdle whose contour surpasses the line of the body, and is obviously of stiff material, probably wood, since it appears to be carved. Hanging from the girdle there is also a sort of skirt, sloping away in front on both sides, where it has laces and fringes, and closed behind. There are many like this in the Codex Vindobonensis. As a leg ornament, we find below the right knee a galloon, or thong, with something hanging from it, or else a multiple rope of pearls; and both wrists seem to be adorned with strings of pearls. Upon the feet there are sandals; frequently only the left foot has a sandal, while the right is bare. One hand of the priest is uniformly covered with something like the head of a man or of a beast. Seler takes this for a mask, not for a real head; and certainly the style of representation is in favor of this interpretation, for it is entirely unlike the realistic heads which the high priest and his assistants in fig. 1 carry in their arms. The latter must be the heads cut off of the sacrificed men. Whether these masks are also to be regarded as offerings is doubtful, although the arm which carries one of them is usually raised toward the divinity, and seems to hold out something to him. But why should the sacrifice itself be shown in the one case and in the other only its mask? It

may be that the mask is the distinctive mark of the priest, and h: some reference to the divinity whom he serves.

After these general explanations, short descriptions of the singl pieces must suffice. Among the originals of the Berlin Museum ther are eight blocks which have approximately equal dimensions. Habel who measured them in their original condition in situ, gives the heigh as 12 English feet, the breadth of the sculptured surface as 3, and th thickness of the stone as 2 feet; and he remarks that the face of eacl is plane in the lower 3 feet, so that the sculptured part is only 9 feet high. The blocks must have stood on end, and probably, with open interspaces, formed the façade of a temple or temples. For, had they been joined together, the sculpture would, in the ornamentation or somewhere, have been continuous from block to block; while, in fact, each is a separate representation, having, in some cases, a border of its own. There is no further connection between them than that of the subjects they represent, which are all religious performances, especially the worship of different divinities. This again agrees with the hypothesis that the blocks are remains of temples.

Of these eight blocks our museum possesses casts of the sculptured faces of three only, the first three of the following enumeration:

No. 1. Upon this plinth we see in the middle a priest, characterized by the sacrificial knife in his right hand and by the cut-off head of the sacrifice in his left. This kind of sacrifice belongs to the Mayas, not to the Nahoas, who, as is well known, offered the heart of the victim; and the whole composition must be interpreted in the light of Maya customs and conceptions. Land tells us that the high priest is the representative of the sun and that his four assistants answer to the four quarters of the horizon. The four assistants occupy here the four corners of the plinth; but in order to fix their orientation we must consult Nahuatlan ideas. The north is the place whither the dead go and where the god of death abides, which corresponds best with the assistant in the right lower corner, who appears as a skeleton. It is to be noticed that Death is not commonly represented by an entire carcass, but is incarnate only in arms and legs, or even only in hands and feet. This being settled, the assistant of the lower left-hand corner should be the east, that of the upper right-hand corner the west, and that of the upper left-hand corner the south. The last has also a death's head. Before the bridge of his nose there is a hooked object. The south was also regarded as the place of dearth and hunger; so that the reference to death is suitable to it. Like the high priest himself, each of the four assistants bears in his hands the head of a victim. These five heads differ from each other and from their bearers by the ornamentation and the anthropological type. We may reasonably infer that these heads represent races hostile to the inhabitants of Santa Lucia, and may correspond to the directions in which their

FIG. 1.—PRIEST SACRIFICING A VICTIM.

the sign of discourse, though not curved, but of a broken form. It very likely merely expresses the close connection of the symbolic figure with the priest. Whether the furrowed, pointed, elongated form which, starting from the priest's nose, arches backward, is equivalent to the flame-like shape which in other compositions shoots out from the girdle, must remain an open question. Over the head of the priest are two disks with raised rims, upon each of which is figured the head of a dog or some beast. According to the Mexican emblems, this would read "two dogs," which is a date, but may also be a name. Near these disks and over the skeleton we see a stand on which is placed the cut-off head of a victim, the type of which, except for a different earring, agrees exactly with that of the victim carried by the lower left-hand assistant in No. 1.

No. 3. This plinth is provided with a border. The divinity wears the hair bound up with snakes whose ends writhe upward. The necklace, too, is wound round with a snake. The bowed arms are surrounded with flames and the hands hold a peculiar object which bears a disk, from which something like feathers stick up, and from which hangs a three-cornered thing with a cross cut in it and with an excision at the end shaped like a stile between two fields. This hanging object is very much like certain feet of vessels which are frequently found upon the high plateau of Mexico; but this throws no light upon the object now considered. From the head of the divinity spring three boughs with leaves, flowers, and fruit, as well as other uninterpretable pendants, and two similar boughs proceed downward from the arms. We have here evidently an earth divinity, and, according to Seler, an early conception of such a divinity as causing drought, hunger, and earthquakes. The flames which surround the upper objects involve a reference to fires or to the sun, and justify Seler's view. In regard to the priest, it is to be noticed that the left hand is covered with a human mask. In what seems to be a mantle falling over the back, a death's head is introduced, precisely like that upon the fire basin that we shall notice below. The wooden girdle is also adorned with a death's head. The crooked incisions which we noticed on the left knee of No. 2 are here seen on both.

No. 4. The plinth has a smooth border. The divinity exceptionally wears a nose ornament in the shape of a clasp with enlarged ends. The hair appears to be intertwined with snakes, and from the ornaments of the head and neck proceed, above and below, branching boughs, exactly as in No. 3, except that here the upper part forms three teeth or rays, which lead Seler to suspect that this divinity is the goddess of night. Since other characteristics are wanting to that interpretation, such as are used in picture writings to signify the heavens of night, I can not entirely assent to it, and opine that there is only a reference to fruitfulness, and this time without the addition

FIG. 3.—GOD OF DROUGHT, THUNDER, AND EARTHQUAKES.

FIG. 4.—GODDESS OF FRUITFULNESS.

FIG. 5.—EARTH GODDESS, MOTHER OF ALL.

of flames, which show the evil qualities of the earth goddess upon No. 3. The headdress of the priest runs out into three teeth, from which proceed flames, and from his back there falls apparently the skin of a beast of prey, into whose belly a lance is sticking. Seler thinks that his interpretation of the divinity is confirmed by this feature, since the jaguar (if such it is intended to be) was, both with the Mayas and with the Nahoas, the emblem of the sun; and he is shown as stuck through with a spear, to indicate that the sun is robbed of his power. So, according to Seler, the night gains its cause. Yet it would be equally consequent to say that the destructive effect of the sun is overcome and a fruitful season, perhaps the rainy season, is brought in. Seler himself brings confirmation to this interpretation in recalling a Dresden design in which the wounded beast of prey, a puma or jaguar, lies at the feet of the rain god. The left arm of the priest is not stretched up, but the forearm is bent down, the hand being covered with the mask of a beast of prey. Another such animal is, according to Seler, depicted on the wooden girdle. If this be so, it exceptionally looks upward, as plainly appears in Habel's drawing.

No. 5. In this piece a rim surrounds the sculptured face. We have before us only an indistinct copy of a bad photograph, for Habel gives only a drawing of the lower half with the priest. The upper half was subsequently found. The whole is, however, less well executed than the other pieces. Seler, who had the originals at his disposition, says that the ornaments above the divinity agree with those which in old Mexico accompany the goddess of maize, named Seven Snakes. She is one of the forms of the older earth goddess—the mother of all that exists—having been differentiated in the course of time among different races; but in all her transformations the eagle continues to be a prominent accompaniment. The downward-shooting eagle seen below, near the right leg of the priest, can be referred to this fact, and so likewise can the eagle mask which covers the priest's left hand, as well as the eagle upon the wooden girdle. The divinity itself wears upon its head a braid of snakes; and from each arm there springs a branch directed upward which seems to correspond with the sign of discourse. In the headdress of the priest there is a human mask from which depends a long feather.

No. 6. About this block, of which only the upper half has been found, there is a plain border. The divinity is surrounded by the jaws of an alligator, which led Seler to suspect that it was the goddess of water. As a confirmation of this he mentions figures of a crab and of a fish which he says are to be found among the blossoms of the branches which shoot downward from the arms of the goddess. But no such objects can be perceived in the reproduction of a photograph given by Seler, and Habel's drawing and description show only the crab and that at the fracture of the stone where the headdress of the

priest should be. This crab is, therefore, similar to that in the headdress of the high priest of No. 1. It may be added that the priest of No. 6, unlike those of Nos. 2–5, faces the left, as is shown by the sign of discourse before his mouth and by his headdress.

No. 7. In this design and that of No. 8 the priest also faces the left, so that we may infer that the blocks were so arranged in the building that the two different directions in which the priests stood came opposite each other. [The writer seems to intend to leave it undetermined whether he thinks they were in pairs, or whether all the right-facing priests were at the left of the left-facing priests.] The divinity is upon this plinth very peculiarly represented. Upon his back he wears two crooked plates, which stand opposite each other, with their points almost in contact. The headdress is formed of two entangled rattlesnakes, and the ornament of neck and breast is composed of quadrangular plates or dice, as are the bracelets. From the middle of the breast ornament hangs a symbol such as the divinity of No. 3 wears. From either side of this symbol proceed boughs, each bearing leaves and flowers, while the entire boughs have the characters of the sign of discourse. We can come to no conclusion in regard to the function of this divinity. Habel's surmise that it is the goddess of the moon was suggested merely by the crescent-shaped plates and is unsupported by any old American examples. The priest has no other headdress than long, wavy hair. His right arm is raised; the left hangs down, and its hand is covered with a human mask, ornamented with a nose clasp. The wooden[1] girdle has represented upon it a fantastic beast's head, possibly a snake's head. In front of the priest there is a remarkable shape, like a package tied up at the ends, with a banner hanging down over the middle part of it, which banner has a cross on it and a style-shaped excision at the bottom exactly like the symbol that depends from the divinity's breast ornament. Upon the package is placed a human head with the sign of discourse before the mouth, and both there and behind it sheaves of feathers seem to stick up.

No. 8. The whole design is surrounded by a rim. The face of this divinity seems more masculine and aged than the others. It is surrounded with boughs bearing leaves, flowers, and fruit, but some of them have the characters of the sign of discourse. The priest has a helm in the form of a human head, and his left hand is covered with a mask which seems to represent the skull of an ape. Upon the wooden girdle a head may be seen, though it is indistinct, while upon his thigh there is a clear representation of a human head. This last wears a high cap and probably is intended for a trophy hanging from the girdle. Noteworthy are the flamelike shapes which from the back of

[1] The text has Halsgürtel, necklace. But I suppose this is a clerical error for Holzgürtel, wood girdle.—TRANSLATOR.

FIG. 6.—GODDESS OF WATER?

FIG. 7.—DEITY OF UNKNOWN FUNCTION.

11 wears a cap with dependent feathers and ribbons. The stag man has the hoof of the right fore leg covered with a mask; the left is raised and holds something, it is impossible to make out what. From his under jaw proceeds a flame.

No. 12. This great and finely worked piece was called an altar by Habel; but Seler more correctly designates it as a fire basin. The whole represents a crouching ape carrying on his back the basin wrapped round with a feather cloth (*Federtuch*), while he seems to hold Death between his fore paws. Such, at least, is the meaning of the symbolic low relief. It was chiefly this figure of Death which induced Habel to regard the whole as a sacrificial stone, and to assume that the blood of the victim was collected in the shallow basin, without making it clear how the victim was to be put to death. Other representations show that the head of the victim was cut off, in order to offer to the divinity the most important part of the man. But with such a mode of death there would scarcely be occasion for so colossal a basin. We know, however, that upon the platforms of the temples there stood great fire basins in which fire had to be kept up day and night. Now this piece would serve such a purpose very well; and consequently Seler's interpretation is to be preferred. The symbolic elements which in the form and ornamentation of this piece appear as an ape and as Death, bear no direct relation to such a purpose; but they may have had some ritual significance, indicating, for example, the divinity to which the particular temple was dedicated. The ape and Death, alike in the myths of the Mayas and of the Nahoas, are closely connected, representing perhaps the opposition of life and death, or that of motion and stillness. Among both peoples we find these among the twenty day signs.

No. 13. In conclusion, I will here notice a block whose sawed-off plate in the shipment at the port of San José, unfortunately fell into the sea and was irrecoverably lost. It is, therefore, doubly gratifying to meet among Habel's drawings the highly interesting representation of this block. An enlarged copy of the drawing is to be seen in the Museum. Habel gives its height as 9½ English feet and its sculptured width as 5 feet.

The design shows us the king of the vultures (*Sarcoramphus papa*) with outspread wings. The disk of the sun hangs at his neck, so that he is here the sun bird. The king of the vultures is also one of the day signs among both Mayas and Nahoas, and with the latter is further the bringer of worthy old age, but these functions seem to be foreign to the present representation.[1] The bird has half eaten a man, the upper part of whose body hangs down, and with his claw he grasps a

[1] The Bakairés of central Brazil, according to von den Steinen, consider the king of the vultures to be the creator of the sun, which would answer exactly to the design of No. 13.—AUTHOR.

FIG. 9.—CHIEF SEATED IN A CHAIR.

sphere, which may be an india rubber ball used in playing ball. The game of ball is, in the picture writings, the sign of Heaven, and the flying ball denotes the sun in its motions. The head of the bearded man agrees with that which the lower right-hand assistant of No. 1 carries. He would thus represent a race hostile to the inhabitants of Santa Lucia. From this representation, of which there is a replica, which, I believe, has been preserved, it seems to follow that there was a leading worship of the sun, and that human sacrifices were offered to it.

The material described is small in number, but in scientific interest it is an important enrichment to the still limited exhibit of our Museum in old American civilization. The sculptures of Santa Lucia are, at any rate, well adapted to bring wider recognition to a proposition long established in science that América, before it was plundered, was in part inhabited by peoples well entitled to be called civilized. It must be remembered that every people follows its own course of development, and that the forms of expression of the resulting civilization are not only the product of the peculiar genius of the race, but are also influenced in the most diverse ways by the conditions of life and the events of history. Science undertakes the task of exploring the conditions of these phenomena, so that, having arrived at a complete understanding of the nature of a civilization and its significance for the people studied, it may attain the only correct standard for its appreciation. In the present case science has not reached that degree of knowledge. Only here and there can it lift the veil which peculiar thoughts and ideas have woven around the productions of the Santa Lucia civilization. Yet even these few glimpses suffice to enable us to say that we have here to do with performances which rise far above the common level. Both conception and execution testify to extraordinary endowments, especially when we reflect that a material as hard as stone allows expression to the idea only after immense technical difficulties have been overcome. All those races, so far as we know, lacked the chief means that we possess, since the use of iron was unknown, and consequently the working of the stone in such fashion as we here see it worked must have been a fearfully wearisome and prolonged labor.

FIG. 11.—SICK MAN, A DOCTOR.

FIG. 12.—FIRE BASIN.

FIG. 13.—VULTURE DEVOURING MAN.

to the wind. The Lake of Constance is often swept by violent storms. Several times the housing broke loose from its moorings; but precautions have now been taken which will prevent a recurrence of the accident. The float has been anchored to a block of cement 4½ tons in weight, sunk at a depth of 72 feet, and held in place by steel hawsers 250 feet long, secured to two ship's anchors weighing over 5,000 pounds each. If despite these precautionary measures the housing should again break loose, two heavy anchors at the bow of the float can be lowered to prevent the float from being driven ashore.

The air ship now in the course of erection within this structure is 410 feet long. The supporting body is a cylinder 39 feet in diameter, the ends being tapered so as to offer the least possible resistance to the air. The skeleton frame of this cylinder is composed of aluminum. Sixteen rings separated from one another 26 feet hold the framework together. These rings are not circular, but form a twenty-four-sided polygon; their shape is determined by numerous strong aluminum wires radiating from a central circle like the spokes of a bicycle wheel. Horizontal bars are used to hold the rings together. The entire frame-
work will be surrounded by a
for its great toughness and ten
and on each side of the rings a

The sixteen rings divide the
as it were, each of which will c
these seventeen independent ba
intact and will still support t
resembles that of the water-tig
the system is far safer than that
doors or openings are used.

The balloons are made of a li
fabric covered with a gas-tigh
framework is still further prote
which serves chiefly to protect
sun and from rain. The ramie
ing the balloons from one anot

The balloons will have a cap
filled with hydrogen gas kept
each of which contains 175 cub
therefore, be required. The cy
will be towed to the housing
Since a cubic meter (1.308 cubi
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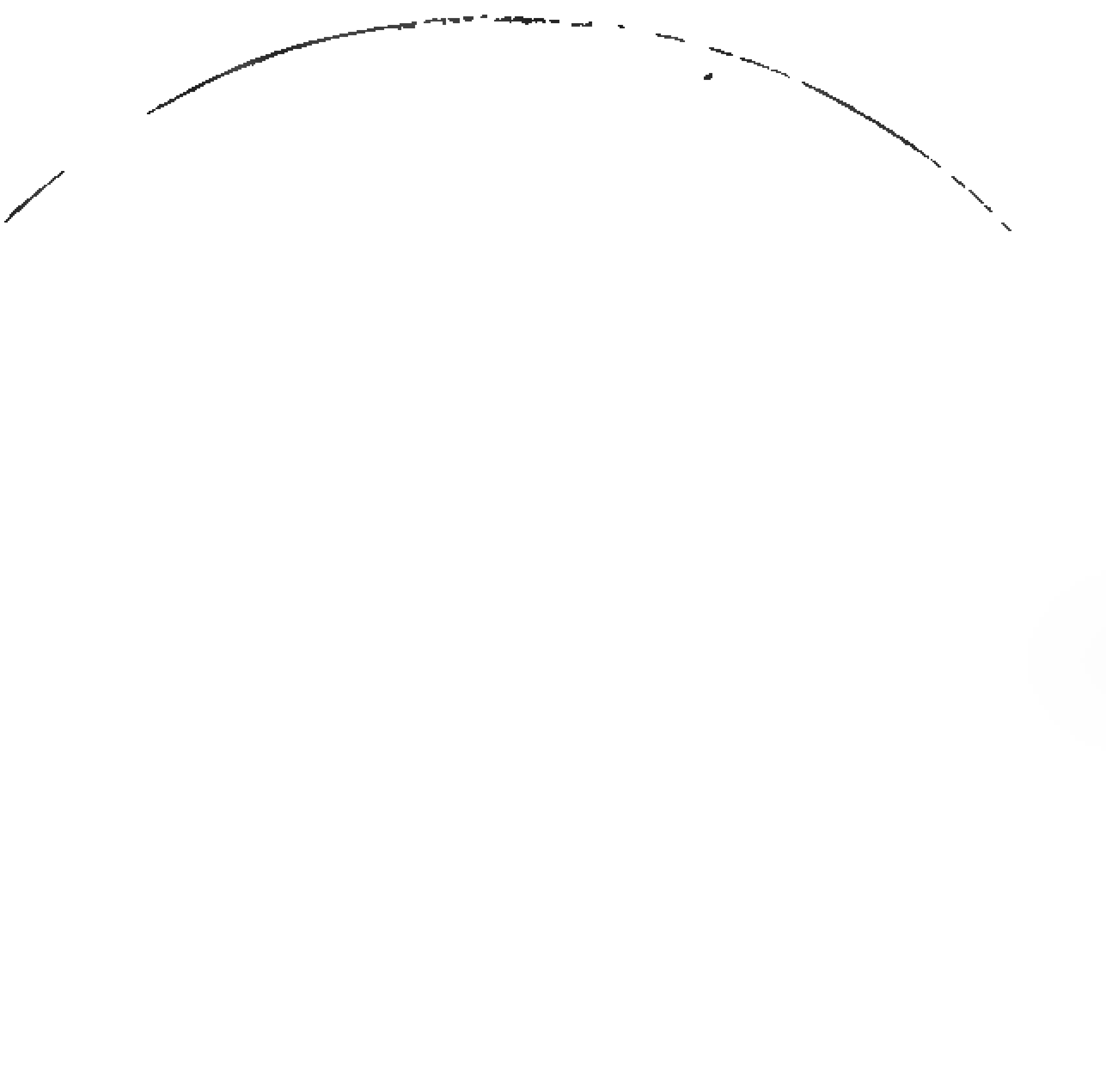

THE AIRSHIP HOUSING IN THE LAKE OF CONSTANCE.

This, in brief, is the general construction of the supporting part of the contrivance.

Every moving body, such as a ship or bicycle, can be steered. That it has hitherto been impossible to direct an air ship is due partly to the form adopted in the construction, partly to insufficient motive power, and inadequate steering appliances. Count von Zeppelin claims to have remedied all these faults. He will drive his air ship backward or forward by four aluminum propellers, a pair of which will be mounted at each end of the cylindrical body, somewhat below the central axis. The ship will be steered by rudders placed at the front and rear ends.

Rigidly connected with the balloon cylinder are two aluminum cars, each located beneath a pair of propellers. These cars are 21.32 feet long, 5.96 feet wide, 3.28 feet high, and taper from top to bottom. Beneath the bottom of each car are wheels provided with coiled springs, which deaden the shock when the air ship strikes the ground and set the wheels in motion. In each car is a benzine motor, developing from 12 to 15 indicated horsepower, by means of which the propellers are driven. The connection between the propellers and the motors consists of gearing and of driving shafts passing through Mannesmann seamless steel tubes. Variations in the position of the framework can be compensated for by means of two movable joint couplings.

Benzine is the most suitable motive power for aerial navigation. Electricity can not be used, for the necessary accumulators are far too heavy. Hydrocarbon vapors, to be sure, are highly inflammable, and their use in air ships provided with gas bags is therefore attended with much danger. But the benzine motors, in the present instance, have been so carefully constructed that there is no danger of fire. Moreover, the lower side of the balloon cylinder immediately above the cars has been covered with fireproof material. The cars are connected by a passage 2 feet wide, which rest on T rails and which are tied together with aluminum wire. The crew of five men can thus pass from one car to the other. Beneath the cars and connecting passage a cable is loosely suspended, to which a sliding weight is secured. By adjusting the position of the weight the ends of the ship can be raised or lowered. When the weight is shifted to the rear, the forward end of the air ship is raised, and the air pressing on the under surface, as in a kite, will force the vessel upward. When the weight is shifted to the front, the rear end is elevated, and the ship will descend, owing to the pressure of the air on its upper surface.

The first trials of the ship are soon to be made. The supporting cylindrical body is almost completed, and only the pointed ends are still to be placed in position. The cars, motors, propeilers, and accessory apparatus will be shipped to the housing ready to be mounted, an operation which will require but a few days.

THE PROGRESS IN STEAM NAVIGATION.[1]

By Sir WILLIAM H. WHITE,
Assistant Controller and Director of Naval Construction of the British Navy.

Steamship design, to be successful, must always be based on experiment and experience as well as on scientific principles and processes. It involves problems of endless variety and great complexity. The services to be performed by steamships differ in character, and demand the production of many distinct types of ships and propelling apparatus. In all these types, however, there is one common requirement—the attainment of a specified speed. And in all types there has been a continuous demand for higher speed.

Stated broadly, the task set before the naval architect in the design of any steamship is to fulfill certain conditions of speed in a ship, which shall not merely carry fuel sufficient to traverse a specified distance at that speed, but which shall carry a specified load on a limited draft of water. Speed, load, power, and fuel supply are all related, and the last two have to be determined in each case. In some instances other limiting conditions are imposed affecting length, breadth, or depth. In all cases there are three separate efficiencies to be considered—those of the ship, as influenced by her form; of the propelling apparatus, including the generation of steam in the boilers and its utilization in the engines, and of the propellers. Besides these considerations the designer has to take account of the materials and structural arrangements which will best secure the association of lightness with strength in the hull of the vessel. He must select those types of engines and boilers best adapted for the service proposed. Here the choice must be influenced by the length of the voyage, as well as the exposure it may involve to storm and stress.

Obviously the conditions to be fulfilled in an ocean-going passenger steamer of the highest speed, and in a cross-channel steamer designed to make short runs at high speed in comparatively sheltered waters, must be radically different. And so must be the conditions in a swift seagoing cruiser of large size and great coal endurance, from those

[1] Presideptial address before the mechanical section of the British Association for the Advancement of Science, 1899. Printed in Cassier's Magazine, November, 1899.

best adapted for a torpedo boat or destroyer. There is, in fact, no general rule applicable to all classes of steamships; each must be considered and dealt with independently, in the light of the latest experience and improvements. For merchant ships there is always the commercial consideration, Will it pay? For war ships there is the corresponding inquiry, Will the cost be justified by the power and efficiency of the proposed ship?

CHARACTERISTICS OF PROGRESS IN STEAM NAVIGATION.

Looking at the results so far attained, it may be said that progress in steam navigation has been marked by the following characteristics: (1) Growth in dimensions and weights of ships, and large increase in engine power as speeds have been raised. (2) Improvements in marine engineering, accompanying increase of steam pressure. Economy of fuel and reduction in the weight of propelling apparatus in proportion to the power developed. (3) Improvements in the materials used in shipbuilding; better structural arrangements; relatively lighter hulls and larger carrying power. (4) Improvements in form, leading to diminished resistance and economy of power expended in propulsion. These general statements represent well-known facts—so familiar indeed that their full significance is often overlooked. It would be easy to multiply illustrations, but only a few representative cases will be taken.

TRANS-ATLANTIC PASSENGER STEAMERS.

Trans-Atlantic service naturally comes first. It is a simple case, in that the distance to be covered has remained practically the same, and that for most of the swift passenger steamers cargo-carrying capacity is not a very important factor in the design. In 1840 the Cunard steamship *Britannia*, built of wood, propelled by paddle wheels, maintained a sea speed of about 8½ knots. Her steam pressure was 12 pounds per square inch. She was 207 feet long, about 2,000 tons in displacement, her engines developed about 750 horsepower, and her coal consumption was about 40 tons per day, nearly 5 pounds of coal per indicated horsepower per hour. She had a full spread of sail. In 1871 the White Star steamship *Oceanic*, first of that name, occupied a leading position. She was iron built, propelled by a screw, and maintained a sea speed of about 14½ knots. The steam pressure was 65 pounds per square inch, and the engines were on the compound principle. She was 420 feet long, about 7,200 tons in displacement, her engines developed 3,000 horsepower, and she burned about 65 tons of coal per day, or about 2 pounds per indicated horsepower per hour. She carried a considerable spread of sail. In 1889 the White Star steamer *Teutonic* appeared, propelled by twin screws, and practically with no sail power. She is steel built, and maintains a sea speed of

about 20 knots. The steam pressure is 180 pounds per square inch, and the engines are on the triple-expansion principle. She is about 565 feet long, 16,000 tons in displacement, 17,000 horsepower indicated, with a coal consumption of about 300 tons a day, or from 1.6 to 1.7 pounds per indicated horsepower per hour. In 1894 the Cunard steamship *Campania* began her service, with triple-expansion engines, twin screws, and no sail power. She is about 600 feet long, 20,000 tons displacement, develops about 28,000 horsepower at full speed of 22 knots, and burns about 500 tons of coal per day. The new *Oceanic*, of the White Star Line, is just beginning her work. She is of still larger dimensions, being 704 feet in length and over 25,000 tons displacement. From the authoritative statements made, it appears that she is not intended to exceed 22 knots in speed, and that the increase in size is to be largely utilized in additional carrying power. The latest German steamers for the trans-Atlantic service are also notable. A speed of 22½ knots has been maintained by the *Kaiser Wilhelm der Grosse*, which is 25 feet longer than the *Campania*. Two still larger steamers are now building. The *Deutschland* is 660 feet long and 23,000 tons displacement; her engines are to be of 33,000 horsepower, and it is estimated that she will average 23 knots. The other vessel is said to be 700 feet long, and her engines are to develop 36,000 horsepower, giving an estimated speed of 23½ knots. All these vessels have steel hulls and twin screws. It will be noted that to gain about 3 knots an hour nearly 50 per cent will have been added to the displacement of the *Teutonic*, the engine power and coal consumption will be doubled, and the cost increased proportionately.

Sixty years of continuous effort and strenuous competition on this great "ocean ferry" may be summarized in the following statement: Speed has been increased from 8½ to 22½ knots; the time on the voyage has been reduced to about 38 per cent of what it was in 1840. Ships have been more than trebled in length, about doubled in breadth, and increased tenfold in displacement. The engine power has been made forty times as great. The ratio of horsepower to the weight driven has been increased fourfold. The rate of coal consumption—measured per horsepower per hour—is now only about one-third what it was in 1840. To drive 2,000 tons weight across the Atlantic at a speed of 8½ knots per hour, about 550 tons of coal were then burned; now, to drive 20,000 tons across at 22 knots, about 3,000 tons of coal are burned.

With the low pressure of steam and heavy, slow-moving paddle engines of 1840, each ton weight of machinery, boilers, etc., produced only about 2 horsepower. With modern twin-screw engines and high steam pressure, each ton weight of propelling apparatus produces from 6 horsepower to 7 horsepower. Had the old rate of coal consumption continued, instead of 3,000 tons of coal, 9,000 tons would have been required for a voyage at 22 knots. Had the engines been

proportionately as heavy as those in use sixty years ago, they would have weighed about 14,000 tons. In other words, machinery, boilers, and coal would have exceeded in weight the total weight of the *Campania* as she floats to-day. There could not be a more striking illustration than this of the close relation between improvements in marine engineering and the development of steam navigation at high speeds.

Equally true is it that this development could not have been accom plished but for the use of improved materials and structural arrangements. Wood as the principal material for the hulls of high-powered swift steamers imposed limits upon dimensions, proportions, and powers which would have been a bar to progress. The use of iron first, and since of steel, removed those limits. The percentage of the total displacement devoted to hull in a modern Atlantic liner of the largest size is not much, if at all, greater than was the corresponding percentage in the wood-built *Britannia* of 1840, of one-third the length and one-tenth the total weight. Nor must it be overlooked that with increase in dimensions have come considerable improvements in form favoring economy in propulsion. This is distinct from the economy resulting from increase in size, which Brunel appreciated thoroughly half a century ago when he designed the *Great Britain* and the *Great Eastern*.

The importance of a due relation between the lengths of the "entrance and run" of steamships and their intended maximum speeds, and the advantages of greater length and fineness of form as speeds are increased, were strongly insisted upon by Scott Russell and Froude. Naval architects, as a matter of course, now act upon the principle, so far as other conditions permit. For it must never be forgotten that economy of propulsion is only one of many desiderata which must be kept in view in steamship design. Structural weight and strength, seaworthiness, and stability all claim attention and may necessitate modifications in dimensions and form which do not favor the maximum economy of propulsion. Increase in length and weight have largely assisted the marvelous regularity of service now attained on the longest passages by swift steamships. Even the largest vessels at times have to yield to the forces of nature displayed in wind and sea; but these conditions are more rarely reached in the longer and heavier ships.

SWIFT PASSENGER STEAMERS FOR LONG VOYAGES.

Changes similar to those described for the trans-Atlantic service have been in progress on all the great lines of ocean traffic. In many instances increase in size has been due not only to increase in speed, but to enlarged carrying power and the extension of the lengths of voyages. No distance is now found too great for the successful working of steamships, and the sailing fleet is rapidly diminishing in

importance. So far as long-distance steaming is concerned, the most potent factor has undoubtedly been the marvelous economy of fuel that has resulted from higher steam pressures and greater expansion. In all cases, however, advances have been made possible not merely by economy of fuel, but by improvements in form, structure, and propelling apparatus and by increased dimensions. This might be illustrated by many interesting facts drawn from the records of the great steamship companies which perform the services to the Far East, Australia, South America, and the Pacific. I must be content, however, with the statement of a few facts regarding the development of the fleet of the Peninsular and Oriental Company. The paddle steamer *William Fawcett*, of 1829, was about 75 feet long, 200 tons displacement, of 60 nominal horsepower—probably about 120 indicated horsepower—and in favorable weather steamed at a speed of 8 knots. Her hull was of wood, and, like all the steamers of that date, she had considerable sail power.

In 1853 the *Himalaya*, iron-built screw steamer of this line, was described as "of larger dimensions than any then afloat and of extraordinary speed." She was about 340 feet long, over 4,000 tons load displacement, 2,000 indicated horsepower on trial, with an average sea speed of about 12 knots. The steam pressure was 14 pounds per square inch and the daily coal consumption about 70 tons. This vessel was transferred to the royal navy, and did good service as a troopship for forty years. In 1893 another *Himalaya* was added to the company's fleet. She was steel built, nearly 470 feet long and 12,000 tons load displacement, with over 8,000 indicated horsepower and a capability to sustain 17 to 18 knots at sea on a daily consumption of about 140 tons of coal. The steam pressure is 160 pounds per square inch, and the engines are of the triple-expansion type. Comparing the two *Himalayas*, it will be seen that in forty years the length has been increased about 40 per cent, displacement trebled, horsepower quadrupled, and speed increased 50 per cent. The proportion of horsepower to displacement has only been increased as 3 to 4, enlarged dimensions having secured relative economy in propulsion. The rate of coal consumption has been probably reduced to about one-third of that in the earlier ship. The latest steamers of the line are of still larger dimensions, being 500 feet long and of proportionately greater displacement. It is stated that the *Himalaya* of 1853 cost £132,000 complete for sea; the corresponding outlay on her successors is not published, but it is probably twice as great.

On the service to the Cape similar developments have taken place. Forty years ago vessels less than 200 feet long and of about 7 knots performed the service, whereas the latest additions to the fleets exceed 500 feet in length and can, if required, be driven at 17 to 18 knots, ranking in size and power next to the great trans-Atlantic liners. Com-

mercial considerations necessarily regulate what is undertaken in the construction of merchant steamers, including the swift vessels employed in the conveyance of passengers and mails. The investment of £600,000 to £700,000 in a single vessel like a great trans-Atlantic liner is obviously a serious matter for private owners; and even the investment of half that amount in a steamer of less dimensions and speed is not to be lightly undertaken. It is a significant fact that whereas fifteen years ago nearly all the largest and swiftest ocean steamers were British built and owned, at the present time there is serious competition in this class by German, American, and French companies. It is alleged that this change has resulted from the relatively large subsidies paid by foreign governments to the owners of swift steamers and that British owners, being handicapped in this way, can not continue the competition in size and speed on equal terms unless similarly assisted. This is not the place to enter into any discussion of such matters; but they obviously involve greater considerations than the profit of shipowners and have a bearing on the naval defense of the Empire. In 1887 the Government recognized this fact and made arrangements for the subvention and armament of a number of the best mercantile steamships for use as auxiliary cruisers. Since then other nations have adopted the policy and given such encouragement to their shipowners that the numbers of swift steamers suitable for employment as cruisers have been largely increased. Not long since the First Lord of the Admiralty announced to Parliament that the whole subject was again under consideration.

CARGO AND PASSENGER STEAMERS.

Cargo steamers, no less than passenger steamers, have been affected by the improvements mentioned. Remarkable developments have occurred recently, not merely in the pure cargo carrier, but in the construction of vessels of large size and good speed, carrying very great weights of cargo and considerable numbers of passengers. The much-decried "ocean tramp" of the present day exceeds in speed the passenger and mail steamer of fifty years ago. Within ten years vessels in which cargo carrying is the chief element of commercial success have been increased in length from 300 feet or 400 feet to 500 feet or 600 feet; in gross register tonnage from 5,000 to over 13,000 tons, and in speed from 10 or 12 knots to 15 or 16 knots. Vessels are now building for the Atlantic service which can carry 12,000 to 13,000 tons dead weight, in addition to passengers, while possessing a sea speed as high as that of the swiftest mail steamers afloat in 1880. Other vessels of large carrying power and good speed are running on much longer voyages, such as to the Cape and Australia.

In order to work these ships successfully, very complete organization is necessary for the collection, embarkation, and discharge of

cargo. The enterprise and skill of shipowners have proved equal to this new departure, as they have in all other developments of steamships. How much further progress will be made in the sizes and speeds of these mixed cargo and passenger steamers can not be foreseen. The limits will be fixed by commercial considerations and not by the capability of the shipbuilder. In passing, it may be noted that while the lengths and breadths of steamships have been greatly increased, there has been but a moderate increase in draft. Draft of water is, of course, practically determined by the depths available in the ports and docks frequented, or in the Suez Canal for vessels trading to the East. From the naval architect's point of view, increase in draft is most desirable as favoring increase of carrying power and economy of propulsion. This fact has been strongly represented by shipowners and ship designers, and not without result. The responsible authorities of many of the principal ports and of the Suez Canal have taken action toward giving greater depth. Other changes have become necessary on the part of dock and port authorities in consequence of the progress made in shipbuilding. Docks and dock entrances have had to be increased in size, more powerful lifting appliances provided, and large expenditure incurred. There is no escape from these changes if the trade of a port is to be maintained. The chief lesson to be learned from past experience is that when works of this character are planned, it is wise to provide a large margin beyond the requirements of existing ships.

CROSS-CHANNEL STEAMERS.

The conditions to be fulfilled in vessels designed to steam at high speed for limited periods obviously differ essentially from those holding good in ocean-going steamers. None the less interest attaches, however, to cross-channel steamers, and in no class has more notable progress been made. So far as I am informed, the first steamer placed on the route between Dover and the Continent in 1821 was of 90 tons burden, 30-horsepower nominal, and maintained a speed of 7 to 8 knots. She was built by Denny, of Dumbarton, engined by Robert Napier, and named the *Rob Roy*. It is interesting to note that the lineal successors of the builder of this pioneer vessel have produced some of the most recent and swiftest additions to the cross-channel service. In 1861–62 a notable advance was made by the building of vessels which were then remarkable for structure and speed, although small and slow when compared with vessels now running. Their designers realized that lightness of hull was of supreme importance, and with great trouble and expense obtained steel of suitable quality. The machinery was of special design and relatively light for the power developed. A small weight of coal and cargo had to be carried, and the draft of water was kept to about 7 feet. Under then existing

conditions it was a veritable triumph to attain speeds of 15 to 16 knots in vessels only 190 feet long, less than 25 feet broad, and under 350 tons in displacement. To raise the trial speed to 21 or 22 knots in later vessels, whose design includes the improvements of a quarter of a century, it has been found necessary to adopt lengths exceeding 320 feet, and breadths of about 35 feet, with engines developing 4,500 to 6,000 indicated horsepower, and with very great increase in coal consumption and cost.

Another interesting contrast is to be found in the comparison of the steamers running between Holyhead and Kingstown in 1860 and at the present time. The *Leinster* of 1860 was 328 feet long, 35 feet broad, and rather less than 13 feet draft. Her trial displacement was under 2,000 tons, and with 4,750 horsepower she made 17¾ knots. She had a steam pressure of 25 pounds per square inch, and was propelled by paddle wheels driven by slow-moving engines of long stroke. Her successor of 1896 is about 30 feet greater length, 6½ feet greater breadth, and about 10 per cent greater displacement. The steam pressure is 160 pounds per square inch. Forced draft is used in the stoke hold. Twin screws are adopted, driven by quick-running vertical engines of the triple-expansion type. Very great economy of coal consumption is thus secured, as compared with the earlier vessel, and much lighter propelling apparatus in proportion to the power, which is from 8,000 horsepower to 9,000 horsepower at the full speed of 23 knots. The hull is built of steel, and is proportionately lighter.

This is a typical case, and illustrates the effect of improvements in shipbuilding and engineering in thirty-five years. The later ship probably requires to carry no greater load of coal than, if so great as, her predecessor, although her engine power is nearly double. The weight devoted to propelling machinery and boilers is probably not so great. Thanks to the use of steel instead of iron and to improved structural arrangements, the weight of hull is reduced in comparison with dimensions, and a longer ship is produced, better adapted to the higher speed. Messrs. Laird, of Birkenhead, who built three of the *Leinster* class forty years ago and have built all the new vessels, are to be congratulated on their complete success. Between such vessels designed for short runs at high speed, and requiring, therefore, to carry little coal, while the load carried, exclusive of coal, is trifling, and an ocean-going steamer of the same average speed designed to make passages of 3,000 miles, there can obviously be little in common. But equal technical skill is required to secure the efficient performance of both services. In the cross-channel vessel, running from port to port and under constant observation, conditions of working in engine and boiler rooms, as well as relative lightness in scantlings of hull, can be accepted which would be impossible of application in the seagoing

ship. These circumstances, in association with the small load carried, explain the apparent gain in speed of the smaller vessel in relation to her dimensions.

INCREASE IN SIZE AND SPEED OF WAR SHIPS.

Turning from seagoing ships of the mercantile marine to war ships, one finds equally notable facts in regard to increase in speed, associated with enlargement in dimensions and advance in propelling apparatus, materials of construction, structural arrangements, and from. Up to 1860 a measured-mile speed of 12 to 13 knots was considered sufficient for battle ships and the largest classes of cruisers. All these vessels possessed good sail power and used it freely as an auxiliary to steam or as an alternative when cruising or making passages. When armored battle ships were built, 1859, the speeds on measured-mile trials were raised to 14 or 14½ knots, and so remained for about twenty years. Since 1880 the speeds of battle ships have been gradually increased, and in the latest types the measured-mile speed required is 19 knots. Up to 1870 the corresponding speeds in cruisers ranged from 15 to 16 knots. Ten years later the maximum speeds were 18 to 18½ knots in a few vessels. Since then trial speeds of 20 to 23 knots have been attained, or are contemplated. There is, of course, a radical distinction between these measured-mile performances of war ships and the average sea speeds of merchant steamers above described. But for purposes of comparison between war ships of different dates measured-mile trials may fairly be taken as the standard. For long-distance steaming the power developed would necessarily be much below that obtained for short periods, and with everything at its best. This is frankly recognized by all who are conversant with war-ship design, and fully allowed for in estimates of sea speeds.

On the other hand, it is possible to point to sea trials made with recent types where relatively high speeds have been maintained for long periods. For example, the battle ship *Royal Sovereign* has maintained an average speed of 15 knots from Plymouth to Gibraltar and the *Renown* has maintained an equal speed from Bermuda to Spithead. As instances of good steaming by cruisers, reference may be made to 60-hour trials with the *Terrible*, when she averaged over 20 knots, and to the run home from Gibraltar to the Nore by the *Diadem*, when she exceeded 19 knots. Vessels of the *Pelorus* class, of only 2,100 tons displacement, have made long runs at sea averaging over 17 knots. Results such as these represent a substantial advance in speed of Her Majesty's ships in recent years.

Similar progress has been made in foreign war ships built abroad as well as in this country. It is not proposed to give any facts for these vessels or to compare them with results obtained by similar classes of ships in the royal navy. Apart from full knowledge of the conditions

under which speed trials are made a mere statement of speeds attained is of no service. One requires to be informed accurately respecting the duration of the trial, the manner in which engines and boilers are worked, the extent to which boilers are "forced," or the proportion of heating surface to power indicated, the care taken to eliminate the influence of tide or current, the mode in which the observations of speed are made, and other details, before any fair or exact comparison is possible between ships. For present purposes, therefore, it is preferable to confine the illustrations of increase in speed in war ships to results obtained under Admirality conditions, and which are fairly comparable.

A great increase in size has accompanied this increase in speed, but it has resulted from other changes in modern types as well as from the rise in speed. Modern battle ships are of 13,000 to 15,000 tons and modern cruisers of 10,000 to 14,000 tons, not merely because they are faster than their predecessors, but because they have greater powers of offense and defense and possess greater coal endurance. Only a detailed analysis, which can not now be attempted, could show what is the actual influence of these several changes upon size and cost and how greatly the improvements made in marine engineering and shipbuilding have tended to keep down the growth in dimensions consequent on increase in load carried, speed attained, and distance traversed. It will be noted also that, large as are the dimensions of many classes of modern war ships they are all smaller in length and displacement than the largest mercantile steamers above described. There is, no doubt, a popular belief that the contrary is true, and that war ships exceed merchant ships in tonnage. This arises from the fact that merchant ships are ordinarily described, not by their displacement tonnage, but by their register tonnage, which is far less than their displacement.

As a matter of fact, the largest battle ships are only of about two-thirds the displacement of the largest passenger steamers. and from 200 feet to 300 feet shorter. The largest cruisers are from 100 feet to 200 feet shorter than the largest passenger steamers, and about 60 per cent of their displacement. In breadth the war ships exceed the largest merchant steamers by from 5 feet to 10 feet. This difference in form and proportions is the result of radical differences in the vertical distribution of the weights carried, and is essential to the proper stability of the war ships. Here we find an illustration of the general principle underlying all ship designing. In selecting the forms and proportions of a new ship considerations of economical propulsion can not stand alone. They must be associated with other considerations, such as stability, protection, and maneuvering power, and in the final result economy of propulsion may have to be sacrificed to some extent in order to secure other essential qualities.

ADVANTAGES OF INCREASED DIMENSIONS.

Before passing on it may be interesting to illustrate the gain in economy of propulsion resulting from increase in dimensions by means of the following table, which gives particulars of a number of typical cruisers, all of comparatively recent design:

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Length ....feet..	280	300	360	435	500
Breadth ....do...	35	43	60	69	71
Mean draft ....do...	13	16¼	23¾	24¼	26¼
Displacement ....tons..	1,800	3,400	7,400	11,000	14,200
Indicated horsepower for 20 knots	6,000	9,000	11,000	14,000	15,500
Indicated horsepower per ton of displacement	3.3	2.65	1.48	1.27	1.09

The figures given are the results of actual trials, and embody, therefore, the efficiencies of propelling machinery, propellers, and forms of the individual ships. Even so they are instructive. Comparing the first and last, for example, it will be seen that while the displacement is increased nearly eightfold the power for 20 knots is only increased about 2.6 times. If the same types of engines and boilers had been adopted in these two vessels—which was not the case, of course—the weights of propelling apparatus and coal for a given distance would have been proportional to the respective powers; that is to say, the larger vessel would have been equipped with only 2.6 times the weight carried by the smaller. On the other hand, roughly speaking, the disposable weights, after providing for hulls and fittings in these two vessels, might be considered to be proportional to their displacements. As a matter of fact, this assumption is distinctly in favor of the smaller ship. Adopting it, the larger vessel would have about 8 times the disposable weight of the smaller, while the demand for propelling apparatus and fuel would be only 2.6 times that of the smaller vessel. There would, therefore, be an enormous margin of carrying power in comparison with displacement in the larger vessel. This might be devoted, and, in fact, was devoted, partly to the attainment of a speed considerably exceeding 20 knots—which was a maximum for the smaller vessel—partly to increased coal endurance, and partly to protection and armament.

Another interesting comparison may be made between vessels Nos. 4 and 5 in the preceding table by tracing the growth in power necessary to drive the vessels at speeds ranging from 10 knots up to 22 knots.

It will be noted from the table that follows that up to the speed of 18 knots there is a fairly constant ratio between the powers required to drive the two ships. As the speeds are increased the larger ship gains, and at 22 knots the same power is required in both ships. The

smaller vessel, as a matter of fact, was designed for a maximum speed of 20½ knots, and the larger for 22 knots. Unless other qualities had been sacrificed neither space nor weight could have been found in the smaller vessel for machinery and coals corresponding to 22 knots. The figures are interesting, however, as illustrations of the principle that economy of propulsion is favored by increase in dimensions as speeds are raised.

Knots.	Horsepower.	
	No. 4.	No. 5.
10	1,500	1,800
12	2,500	3,100
14	4,000	5,000
16	6,000	7,500
18	9,000	11,000
20	14,000	15,500
22	23,000	23,000

Going a step farther, it may be assumed that in unsheathed cruisers of this class about 40 per cent of the displacement will be required for the hull and fittings, so that the balance, or "disposable weight," would be about 60 per cent, say, 6,600 tons for the smaller vessel and 8,500 tons for the larger, a gain of nearly 2,000 tons for the latter. If the speed of 22 knots were secured in both ships, with machinery and boilers of the same type, the larger ship would, therefore, have about 2,000 tons greater weight available for coals, armament, armor, and equipment. These illustrations of well-known principles have been given simply for the assistance of those not familiar with the subject, and they need not be carried farther. More general treatment of the subject, based on experimental and theoretical investigation, will be found in text-books of naval architecture.

SWIFT TORPEDO VESSELS.

Torpedo flotillas are comparatively recent additions to war fleets. The first torpedo boat was built by Mr. Thornycroft for the Norwegian navy in 1873, and the same gentleman built the first torpedo boat for the royal navy in 1877. The construction of the larger class, known as "torpedo-boat destroyers," dates from 1893. These various classes furnish some of the most notable examples extant of the attainment of extraordinarily high speeds for short periods, and in smooth water, by vessels of small dimensions. Their qualities of performances, therefore, merit examination. Mr. Thornycroft may justly be considered the pioneer in this class of work. Greatly impressed by the combination of lightness and power embodied in railway locomotives, Mr. Thornycroft applied similar principles to the propulsion of small boats, and obtained remarkably high speeds. His work became more widely known when the results were published of a

series of trials, conducted in 1872 by Sir Frederick Bramwell, on a small vessel named the *Miranda*. She was only 45 feet long and weighed 4 tons, yet she exceeded 16 knots on trial. The Norwegian torpedo boat built in 1873 was 57 feet long, 7½ tons, and of 15 knots. The first English torpedo boat of 1877 was 81 feet long, 29 tons, and attained 18½ knots.

Mr. Yarrow also undertook the construction of small, swift vessels at a very early date, and has greatly distinguished himself throughout the development of the torpedo flotilla. Messrs. White, of Cowes, previously well known as builders of steamboats for use on board ships, extended their operations to the construction of torpedo boats. These three firms for a considerable time practically monopolized this special class of work in this country. Abroad they had able competitors in Normand in France, Schichau in Germany, and Herreshoff in the United States. Keen competion led to successive improvements and rapid rise in speed.

During the last six years the demand for a fleet of about 100 destroyers, to be built in the shortest possible time, involved the necessity for increasing the sources of supply. At the invitation of the Admiralty a considerable number of the leading shipbuilding and engineering firms have undertaken, and successfully carried through, the construction of destroyers varying from 26 to 33 knots in speed, although the work was necessarily of a novel character, involving many difficulties. As the speeds of torpedo vessels have risen, so have their dimensions increased. Within the class, the law shown to hold good in larger vessels applies equally. In 1877 a first-class torpedo boat was 81 feet long, under 30 tons weight, developed 400 horsepower, and steamed 18½ knots. Ten years later the corresponding class of boat was 135 feet long, 125 tons weight, developed 1,500 horsepower, and steamed 23 knots. In 1897 it had grown to 150 feet in length, 140 to 150 tons, 2,000 horsepower, and 26 knots. Destroyers are not yet of seven years' standing, but they come under the rule. The first examples (1893) were 180 feet long, 240 tons, 4,000 horsepower, and 26 to 27 knots. They were followed by 30-knot vessels, 200 to 210 feet long, 280 to 300 tons, 5,500 to 6,000 horsepower. Vessels now in construction are to attain 32 to 33 knots, their length being about 230 feet, displacements 360 to 380 tons, and engine power 8,000 to 10,000 horsepower.

Cost has gone up with size and power, and the limit of progress in this direction will probably be fixed by financial considerations rather than by constructive difficulties, great as these are as speeds rise. It may be interesting to summarize the distinctive features of torpedo vessel design:

(1) The propelling apparatus is excessively light in proportion to the maximum power developed. Water-tube boilers are now universally adopted, and on speed trials they are "forced" to a considerable

extent. High steam pressures are used. The engines are run at a high rate of revolution—often at 400 revolutions per minute. Great care is taken in every detail to economize weight. Speed trials at maximum power extend over only three hours. On such trials in a destroyer each ton weight of propelling apparatus produces about 45 indicated horsepower. Some idea of the relative lightness of the destroyer's machinery and boilers will be obtained when it is stated that in a large modern cruiser with water-tube boilers, high steam pressure, and quick-running engines, the maximum power obtained on an eight hours' trial corresponds to about 12 indicated horsepower per ton of engines, boilers, etc. That is to say, the proportion of power to weight of propelling apparatus is from three and a half to four times as great in the destroyer as it is in the cruiser.

(2) A very large percentage of the total weight, or displacement, of a torpedo vessel is assigned to propelling apparatus. In a destroyer of 30 knots trial speed, nearly one-half the total weight is devoted to machinery, boilers, etc. In the swiftest cruisers of large size, the corresponding allocation of weight is less than 20 per cent of the displacement, and in the largest and fastest mail steamers it is about 20 to 25 per cent.

(3) The torpedo vessel carries a relatively small load of fuel, equipment, etc. Taking a 30-knot destroyer, for example, the speed trials are made with a load not exceeding 12 to 14 per cent of the displacement. In a swift cruiser the corresponding load would be from 40 to 45 per cent, or proportionately more than three times as great. What this difference means may be illustrated by two statements. If the load were trebled, and the vessels correspondingly increased in draft and weight, the speed obtained with the same maximum power would be about 3 knots less. If, on the other hand, the vessel were designed to attain 30 knots on trial with the heavier load, her displacement would probably be increased about 70 to 80 per cent.

(4) The hull and fittings of the torpedo vessel are exceedingly light in relation to the dimensions and engine power. For many parts of the structure steel of a high tensile strength is used. Throughout the utmost care is taken to economize weight. In small vessels, for special service, many conditions can be accepted which would be inadmissible in larger seagoing vessels. The result of all this care is the production of hull structures having ample general strength, but very little local strength; but notwithstanding all the accidents of navigation and collisions that have occurred in this class of vessels, and they have not been few, not one has yet foundered at sea.

These conditions are essential to the attainment of very high speeds for short periods. They resemble the conditions ruling the design of cross-channel steamers, so far as relative lightness of propelling apparatus, small load, and light scantlings are concerned. The essential

differences lie in the requirements for passenger accommodation as compared with the requirements for armament of the torpedo vessel. No one has yet proposed to extend the torpedo vessel system to seagoing ships of large dimensions. Very similar conditions for the propelling apparatus have been accepted in a few cruisers of considerable dimensions, wherein high speeds for short periods were required. It is, however, unquestionable that in many ways, and particularly in regard to machinery design, the construction of torpedo vessels has greatly influenced that of larger ships.

One important consideration must not be overlooked. For short-distance steaming at high speeds economy in coal consumption is of little practical importance, and it is all important to secure lightness of propelling apparatus in relation to power. For long-distance steaming, on the contrary, economy in coal consumption is of primary importance, and savings in weight of propelling apparatus, even of considerable amount, may be undesirable if they involve increased coal consumption. Differences of opinion prevail as to the real economy of fuel obtainable with boilers and engines such as are fitted to torpedo vessels. Claims are made for some vessels which represent remarkable economy. Only enlarged experience can settle these questions. Endurance is also an important quality in seagoing ships of large size, not merely in structure, but in propelling apparatus. The extreme lightness essential in torpedo vessels obviously does not favor endurance, if high powers are frequently or continuously required. Still it can not be denied that the results obtained in torpedo vessels show such a wide departure from those usual in seagoing ships as to suggest the possibility of some intermediate type of propelling apparatus applicable to large seagoing ships, and securing sufficient durability and economy of fuel in association with further savings of weight.

THE PARSONS TURBO MOTOR.

The steam turbo motor, introduced by Mr. Charles Parsons, with its very high rate of revolution, reduces the weights of machinery, shafting, and propellers greatly below the weight required in the quickest running engines of the reciprocating type. This reduction in the proportion of weight to power carries with it, of course, the possibility of higher speed in a vessel of given dimensions; and when large powers are employed the absolute gain is very great. An illustration of this has been given by Mr. Parsons in the *Turbinia*. That remarkable vessel is 100 feet long and of 44½ tons displacement, but she has attained 33 to 34 knots in short runs. There are three shafts, each carrying three screw propellers, each shaft driven by a steam turbine making over 2,000 revolutions at full speed, when an aggregate of more than 2,000 horsepower is developed. A water-tube boiler of

special design supplies steam of 175 pounds pressure, and is exceptionally light for the steam produced, being highly forced.

The whole weight of machinery and boilers is 22 tons. In other words, about 100 horsepower indicated is produced for each ton weight of propelling apparatus. This is rather more than twice the proportion of power to weight, as compared with the lightest machinery and boilers fitted in torpedo boats and destroyers. It will be noted that in the *Turbinia*, as in the destroyers, about half the total weight is devoted to propelling apparatus, and in both instances the load carried is relatively small. The secret of the extraordinary speed is to be found in the extreme lightness of propelling apparatus and small load. No doubt in the *Turbinia* lightness has been pushed further than it would be in vessels of larger size and greater power. In such vessels a lower rate of revolution would probably be accepted, additional motors would be fitted for maneuvering and going astern, boilers of relatively greater weight would be adopted, and other changes made. But after making ample allowance for all such increases in weight, it is unquestionable that considerable economies must be possible with rotary engines. Two other vessels of the destroyer type with turbo motors—one for the royal navy—are now approaching completion. Their trials will be of great interest, as they will furnish a direct comparison with vessels of similar size and form fitted with similar boilers and driven by reciprocating engines.

On the side of coal consumption Mr. Parsons claims at least equality with the best triple-expansion engines. Into the other advantages attending the use of rotary engines it is not necessary now to enter. Reference must be made, however, to one matter in which Mr. Parsons has done valuable and original work. In torpedo vessels of high speed the choice of the most efficient propellers has always been a matter of difficulty, and the solution of the problem has in many instances involved extensive experimental trials. By means of alterations in propellers alone very large increases in speed have been effected; and, even now, there are difficulties to be faced. When Mr. Parsons adopted the extraordinary speed of revolution just named for the *Turbinia* he went far beyond all experience and precedent and had to face unknown conditions. He has found the solution after much patient and original investigation in the use of multiple screws of small diameter. His results in this direction are of general interest to all who have to deal with screw propulsion. Such radical changes in propelling machinery as are involved in the adoption of turbo motors must necessarily be subjected to thorough test before they will be widely adopted. The experiment which the Admiralty are making is not on a small scale as regards power. Although it is made in a destroyer, about 10,000 horsepower will probably be developed, and a correspondingly high speed attained. It may well happen that from this

experiment very far-reaching effects may follow. Mr. Parsons himself has prepared many designs illustrating various applications of the system to seagoing, cross-channel, and special-service vessels. Where shallowness of draft is unavoidable, the small diameter of the screws possible with the quick-running turbines is clearly an important matter.

COMPARISONS BETWEEN LARGE AND SMALL VESSELS.

It has been shown that the attainment of very high speeds by vessels of small size involves many conditions not applicable to large seagoing steamships. But it is equally true that in many ways the trials of small, swift vessels constitute model experiments, from which interesting information may be obtained as to what would be involved in driving ships of large size at speeds much exceeding any of which we have experience. When the progressive steam trials of such small vessels can be studied side by side with experiments made on models to determine their resistance to various speeds, then the fullest information is obtained and the best guide to progress secured. This advantage, as has been said, we owe to William Froude. His contributions to the reports of the British Association are classics in the literature of the resistance and propulsion of ships. In 1874 he practically exhausted the subject of frictional resistance so far as it is known, and his presidential address in 1875 dealt fully and lucidly with the modern or stream-line theory of resistance. No doubt there would be advantage in extending Froude's experiments on frictional resistance to greater lengths and to ship-shaped forms. It is probable also that dynamometric determinations of the resistance experienced by ships of modern forms and considerable size when towed at various speeds would be of value if they could be conducted.

These extensions of what Froude accomplished are not easily carried out, and in this country the pressure of work on shipbuilding for the royal navy has for many years past taxed to the utmost limits the capacity of the Admiralty experimental establishment, so ably superintended by Mr. R. E. Froude, allowing little scope for purely scientific investigations, and making it difficult to deal with the numerous experiments incidental to the designs of actual ships. Now that Holland, Russia, Italy, and the United States have equipped experimental establishments, while Germany and France are taking steps in that direction, we may hope for extensions of purely scientific work and additions to our knowledge. In this direction, however, I am bound to say that much might be done if experimental establishments capable of dealing with questions of a general nature relating to resistance and propulsion were added to the equipment of some of our universities and colleges. Engineering laboratories have been multiplied, but, there is as yet no example of a model experimental tank devoted to instruction and research.

It is impossible here to attempt any account of Froude's "scale of comparison" between ships and models at "corresponding speeds." But it may be of interest to give a few illustrations of the working of this method, in the form of a contrast between a destroyer of 300 tons, 212 feet long, capable of steaming 30 knots an hour, and a vessel of similar form enlarged to 765 feet in length and 14,100 tons. The ratio of dimensions is here about 3.61:1, the ratio of displacements is 47:1, and the ratio of corresponding speeds is 1.9:1. To 12 knots in the small vessel would correspond 22.8 knots in the large vessel, and the resistance experienced by the large vessel at 22.8 knots—neglecting a correction for friction—should be forty-seven times that of the small vessel at 12 knots. By experiment this resistance for the small vessel was found to be 1.8 ton. Hence, for the large vessel at 22.8 knots the resistance should be 84.6 tons. This would correspond to an "effective horsepower" of over 13,000, or to about 26,000 indicated horsepower. The frictional correction would reduce this to about 25,000 horsepower, or about 1.8 horsepower per ton. Now, turning to the destroyer, it is found experimentally that at 22.8 knots she experiences a resistance of about 11 tons, corresponding to an effective horsepower of over 1,700 horsepower and an indicated horsepower of about 3,000 horsepower; say, 10 horsepower per ton, or nearly five and a half times the power per ton required in the larger vessel. This illustrates the economy of propulsion arising from increased dimensions.

Applying the same process to a speed of 30 knots in the large ship, the corresponding speed in the small ship is 15.8 knots. Her resistance at that speed is experimentally determined to be 3.5 tons, and the resistance of the large ship at 30 knots, neglecting frictional correction, is about 165 tons. The effective horsepower of the large ship at 30 knots is, therefore, about 34,000 horsepower, corresponding to 68,000 horsepower indicated. Allowing for the frictional correction, this would drop to about 62,000 horsepower, or 4.4 horsepower per ton. For the destroyer at 30 knots the resistance is about 17½ tons; the effective horsepower is 3,600 horsepower, and the indicated horsepower about 6,000 horsepower, or 20 horsepower per ton—nearly five times as great as the corresponding power for the large ship. But while the destroyer under her trial conditions actually reaches 30 knots, it is certain that in the large ship neither weight nor space could be found for machinery and boilers of the power required for 30 knots, and of the types usually adopted in large cruisers, in association with an adequate supply of fuel. The explanation of the methods by which the high speed is reached in the destroyer has already been given. Her propelling apparatus is about one-fourth as heavy in relation to its maximum power, and her load is only about one-third as great in relation to the displacement, when compared with the corresponding features in the cruiser.

The earlier theories of resistance assumed that the resistance experienced by ships varied as the square of the speed. We now know that the frictional resistances of clean-painted surfaces of considerable length vary as the 1.83 power of the speed. This seems a small difference, but it is sensible in its effects, causing a reduction of 32 per cent at 10 knots, nearly 40 per cent at 20 knots, and 42 per cent at 25 knots. On the other hand, it is now known that the laws of variation of the residual or wave-making resistance may depart very widely from the law of the square of the speed, and it may be interesting to trace for the typical destroyers how the resistance actually varies. Take, first, the total resistance. Up to 11 knots it varies nearly as the square of the speed; at 16 knots it has reached the cube; from 18 to 20 knots it varies as the 3.3 power. Then the index begins to diminish; at 22 knots it is 2.7; at 25 knots it has fallen to the square; and from there to 30 knots it varies practically as does the frictional resistance. The residual resistance varies as the square of the speed up to 11 knots; as the cube, at 12½ to 13 knots; as the fourth power, about 14½ knots; and at a higher rate than the fifth power at 18 knots. Then the index begins to fall, reaching the square at 24 knots, and falling still lower at higher speeds. It will be seen, therefore, that when this small vessel has been driven up to 24 or 25 knots by a large relative expenditure of power further increments of speed are obtained with less proportionate additions to the power.

Passing from the destroyer to the cruiser of similar form but of 14,100 tons, and once more applying the scale of comparison, it will be seen that to 25 knots in the destroyer corresponds a speed of 47½ knots in the large vessel. In other words, the cruiser would not reach the condition where further increments of speed are obtained with comparatively moderate additions of power until she exceeded 47 knots, which is an impossible speed for such a vessel under existing conditions. The highest speeds that could be reached by the cruiser with propelling apparatus of the lightest type yet fitted in large seagoing ships would correspond to speeds in the destroyer, for which the resistance is varying as the highest power of the speed.

These are suggestive facts. Frictional resistance, as is well known, is a most important matter in all classes of ships and at all speeds. Even in the typical destroyer this is so. At 12 knots the friction, with clean-painted bottom, represents 80 per cent of the total resistance; at 16 knots, 70 per cent; at 20 knots, a little less than 50 per cent; and at 30 knots, 45 per cent. If the coefficient of friction were doubled, and the maximum power developed with equal efficiency, a loss of speed of fully 4 knots would result. In the cruiser of similar form the friction represents 90 per cent at 12 knots, 85 per cent at 16 knots, nearly 80 per cent at 20 knots, and over 70 per cent at 23 knots. If the coefficient of friction were doubled at 23 knots, and the correspond-

ing power developed with equal efficiency, the loss of speed would approximate to 4 knots. These illustrations only confirm general experience that clean bottoms are essential to economical propulsion and the maintenance of speed, and that frequent docking is necessary in vessels with bare iron or steel skins, which foul in a comparatively short time.

POSSIBILITIES OF FURTHER INCREASE IN SPEED.

From the facts above mentioned it is obvious that the increase in speed which has been effected is the result of many improvements, and has been accompanied by large additions to size, engine power, and cost. These facts do not discourage the inventor, who finds a favorite field of operation in schemes for attaining speeds of 50 to 60 knots at sea in vessels of moderate size. Sometimes the key to this remarkable advance is found in devices for reducing surface friction by the use of wonderful lubricants to be applied to the wetted surfaces of ships, or by interposing a layer of air between the skins of ships and the surrounding water, or other departures from ordinary practice. If these gentlemen would "condescend to figures," their estimates or guesses would be less sanguine. In many cases the proposals made would fail to produce any sensible reduction in resistance; in others it would increase resistance. Other proposals rest upon the idea that resistance may be largely reduced by adopting novel forms, departing widely from ordinary ship shapes. Very often small-scale experiments, made in an unscientific and inaccurate manner, are adduced as proofs of the advantages claimed. In other instances mere assertion is thought sufficient. Ordinarily no regard is had to other considerations, such as internal capacity, structural weight and strength, stability, and sea worthiness. Most of these proposals do not merit serious consideration. Any which seem worth investigation can be dealt with simply and effectively by the method of model experiments. A striking example of this method will be found in the usual form of a parliamentary paper—No. 313, of 1873—containing a report made by Mr. William Froude to the Admiralty. Those interested in the subject will find therein much matter of special interest in connection with the conditions attending abnormally high speeds. It must suffice now to say that ship-shaped forms are not likely to be superseded at present.

The most prolific inventions are those connected with supposed improvements in propellers. One constantly meets with schemes guaranteed by the proposers to give largely increased efficiency and corresponding additions to speed. Variations in the numbers and forms of screws or paddles, the use of jets of water or air expelled by special apparatus through suitable openings, the employment of explosives, imitations of the fins of fishes, and numberless other departures from established practice are constantly being proposed. As a

rule, the "inventors" have no intimate knowledge of the subject they treat, which is confessedly one of great difficulty. When experiments are adduced in support of proposals they are almost always found to be inconclusive and inaccurate. More or less mathematical demonstrations find favor with other inventors, but they are not more satisfactory than the experiments. An air of great precision commonly pervades the statements made as to possible increase in efficiency or speed. I have known cases where probable speeds with novel propellers have been estimated—or guessed—to the third place of decimals.

In one instance a trial was made with the new propeller with the result that, instead of a gain in efficiency, there was a serious loss of speed. Very few of the proposals made have merit enough to be subjected to trial. None of them can possibly give the benefits claimed. It need hardly be added that, in speaking thus of so-called "inventors," there is no suggestion that improvement has reached its limit, or that further discovery is not to be made. On the contrary, in regard to the forms of ships and propellers, continuous investigation is proceeding, and successive advances are being made. From the nature of the case, however, the difficulties to be surmounted increase as speeds rise, and a thorough mastery of the past history and present condition of the problems of steamship design and propulsion is required as a preparation for fruitful work in the nature of further advance.

It would be idle to attempt any predictions as to the characteristic features of ocean navigation sixty years hence. Radical changes may well be made within that period. Confining attention to the immediate future, it seems probable that the lines of advance which I have endeavored to indicate will remain in use. Further reductions may be anticipated in the weight of propelling apparatus and fuel in proportion to the power developed; further savings in the weight of the hulls, arising from the use of stronger materials and improved structural arrangements, improvements in form, and enlargement in dimensions. If greater drafts of water can be made possible, so much the better for carrying power and speed. For merchant vessels commercial considerations must govern the final decisions; for war ships the needs of naval warfare will prevail.

It is certain that scientific methods of procedure and the use of model experiments on ships and propellers will become of increased importance. Already avenues for further progress are being opened. For example, the use of water-tube boilers in recent cruisers and battle ships of the royal navy has resulted in saving one-third of the weight necessary with cylindrical boilers of the ordinary type to obtain the same power, with natural draft in the stoke holds. Differences of opinion prevail as to the policy of adopting particular types of water-tube boilers; but the weight of opinion is distinctly in favor of some

type of water-tube boiler in association with the high steam pressures now in use. Greater safety, quicker steam raising, and other advantages, as well as economy of weight, can thus be secured. Some types of water-tube boilers would give greater saving in weight than the particular type used in the foregoing comparison with cylindrical boilers. Differences of opinion prevail also as to the upper limit of steam pressure which can with advantage be used, taking into account all the conditions in both engines and boilers. From the nature of the case, increases in pressure beyond the 160 pounds to 180 pounds per square inch commonly reached with cylindrical boilers can not have anything like the same effect upon economy of fuel as the corresponding increases have had, starting from a lower pressure. Some authorities do not favor any excess above 250 pounds per square inch on the boilers. Others would go as high as 300 pounds, and some still higher.

Passing to the engine rooms, the use of higher steam pressures and greater rates of revolution may, and probably will, produce reductions in weight compared with power. The use of stronger materials, improved designs, better balance of the moving parts, and close attention to details have tended in the same direction without sacrifice of strength. Necessarily there must be a sufficient margin to secure both strength and endurance in the motive power of steamships. Existing arrangements are the outgrowth of large experience, and new departures must be carefully scrutinized. The use of rotary engines, of which Mr. Parsons's turbo-motor is the leading example at present, gives the prospect of still further economies of weight. Mr. Parsons is disposed to think that he could about halve the weights now required for the engines, shafting, and propellers of an Atlantic liner, while securing proper strength and durability. If this could be done in association with the use of water-tube boilers it would effect a revolution in the design of this class of vessel, permitting higher speeds to be reached without exceeding the dimensions of existing ships. It does not appear probable that, with coal as the fuel, water-tube boilers will surpass in economy the cylindrical boilers now in use; and skilled stoking seems essential if water-tube boilers are to be equal to the other type in rate of coal consumption. The general principle holds good that as more perfect mechanical appliances are introduced, so more skilled and disciplined management is required in order that the full benefits may be obtained. In all steamship performance the "human factor" is of great importance, but its importance increases as the appliances become more complex. In engine rooms the fact has been recognized and the want met. There is no reason why it should not be similarly dealt with in the boiler rooms.

Liquid fuel is already substituted for coal in many steamships. When sufficient quantities can be obtained, it has many obvious advan-

tages over coal, reducing greatly manual labor in embarking supplies, conveying it to the boilers, and using it as fuel. Possibly its advocates have claimed for it greater economical advantages over coal than can be supported by the results of extended experiment. Even if the saving in weight for equal evaporation is put as low as 30 per cent of the corresponding weight of coal, it would amount to 1,000 tons on a first-class Atlantic liner. This saving might be utilized in greater power and higher speed or in increased load. There would be a substantial saving on the stoke-hold staff. At present it does not appear that adequate supplies of liquid fuel are available. Competent authorities here and abroad are giving attention to this question and to the development of supplies. If the want can be met at prices justifying the use of liquid fuel, there will undoubtedly be a movement in that direction.

Stronger materials for the construction of hulls are already available. They are, however, as yet but little used, except for special classes of vessels. Mild steel has taken the place of iron, and effected considerable savings of weight. Alloys of steel with nickel and other metals are now made, which gives strength and rigidity much superior to mild steel, in association with ample ductility. For destroyers and torpedo boats this stronger material is now largely used. It has also been adopted for certain important parts of the structures of recent ships in the royal navy. Of course, the stronger material is more costly, but its use enables sensible economies of weight to be made. It has been estimated, for example, that in an Atlantic liner of 20 knots average speed about 1,000 tons could be saved by using nickel steel instead of mild steel. This saving would suffice to raise the average speed more than a knot, without varying the dimensions of the ship. Alloys of aluminium have also been used for the hulls or portions of the hulls of yachts, torpedo boats, and small vessels. Considerable savings in weight have thus been effected. On the other hand, these alloys have been seriously corroded when exposed to the action of sea water, and on that account are not likely to be extensively used. Other alloys will probably be found which will be free from this defect, and yet unite lightness with strength to a remarkable degree. Other examples might be given of the fact that the metallurgist has by no means exhausted his resources, and that the shipbuilder may look to him for continued help in the struggle to reduce the weights of floating structures.

It is unnecessary to amplify what has already been said as to possible increase in the efficiency and types of propellers. With limited draft, as speeds increase and great powers have to be utilized, multiple propellers will probably come into use. Mr. Parsons has shown how such problems may be dealt with; and other investigators have done valuable work in the same direction. In view of what has

happened, and is still happening, it is practically certain that the dimensions of steamships have not yet attained a maximum. Thanks to mechanical appliances, the largest ships built or to be built, can be readily steered and worked. In this particular difficulties have diminished in recent years notwithstanding the great growth in dimensions. Increase in length and weight favor the better maintenance of speed at sea. The tendency, therefore, will be to even greater regularity of service than at present. Quicker passages will to some extent diminish risks, and the chance of breakdown will be lessened if multiple propellers are used. Even now, with twin screws, the risk of total breakdown is extremely small.

Whatever may be the size and power of steamships there must come times at sea when they must slow down and wait for better weather. But the larger and longer the vessel, the fewer will be the occasions when this precaution need be exercised. It must never be forgotten that as ships grow in size, speed, and cost, so the responsibilities of those in charge increase. The captain of a modern steamship needs remarkable qualities to perform his multifarious duties efficiently. The chief engineer must have great powers of organization, as well as good technical knowledge, to control and utilize most advantageously the men and machinery in his charge. Apart from the ceaseless care, watchfulness, and skill of officers and men, the finest ships and most perfect machinery are of little avail.

The "human factor" is often forgotten, but it is all-important. Let us hope that in the future, as in the past, as responsibilities increase so will the men be found to bear them.

A CENTURY'S PROGRESS OF THE STEAM ENGINE.[1]

By Dr. R. H. Thurston.

Twenty years ago, reviewing the progress of the steam engine to date, and seeking the reasons of the steady gain observable in the economy of its operation, the writer, in his History of the Growth of the Steam Engine, remarked:

The direction of improvement has been marked by a continual increase of steam pressure, greater expansion, provision for obtaining dry steam, higher piston speed, careful protection against loss by radiation and conduction, and in marine engines by surface condensation.

This statement and the extended discussion of the details of method and manner of steady improvement during the time since Watt, which were then and there given, apply as well to-day, and require absolutely no qualification, and the summary holds good for the century. The salient points of this progress are three: (1) Increased steam pressure; (2) proportional increase of the "total" ratio of expansion; (3) continual rise in speeds of piston and of rotation.

Of these methods, the first and second, which are in fact properly means of attaining a single object, the widening of the temperature range of the engine cycle, give increased thermodynamic efficiency, and the third produces lessened wastes of heat and work by permitting a larger amount of work to be done by a smaller machine. Roughly estimated, the gain by these methods is proportional to the increase of the square root of the total range of temperature worked through, and to that of the reciprocal of the time occupied by a stroke of piston or by a revolution of the engine, i. e., to the increase of engine speed. It will be interesting and useful to note what have been the magnitudes of these quantities since, at the beginning of the century, the steam engine assumed its modern form and commenced its great work of producing our modern civilization. The following statements and the accompanying diagrams show, approximately, perhaps with considerable and sufficient accuracy, what have been the engine speeds, the expansion ratios, and the steam pressures since the now expiring century was born; what have been the rates of advance, and what the amount of the gains effected.

[1] Reprinted from Cassier's Magazine, Vol. XVII, 1899-1900, pp. 191-199.

The problem of the engineer engaged in the perfection of the steam engine may perhaps be accurately and concisely stated thus:

The conditions of the case as affecting the ideal, purely thermodynamic machine being known and exactly specified, to produce a real engine of similar cycle, free, to the greatest extent practicable, from the defects of cycle and from the extra thermodynamic wastes which characterize all real engines in higher or lower degree.

The ideal engine would be a purely thermodynamic machine, in the sense that its only wastes would be such as would occur in a steam cylinder constructed of a perfectly nonconducting material; it would not waste heat by conduction or radiation or by transformation into useless work. The solution of the problem thus obviously involves simply the adjustment of a valve-gear in such manner as to secure the proper form of cycle, as a geometric figure, and the provision of either a nonconducting cylinder, of a nonconducting working fluid, or both; or, in case neither of these equivalents can be secured, such approximation to these ideal conditions through such other expedients as will insure the best possible approximation to the ideal. Superheating, compounding, and the employment of high-speed engines are simply such expedients, while the increasing of steam pressures and ratios of expansion, and the adoption of condensation and of other plans for reduction of back pressure, are expedients for increasing the ideal efficiency of the engine.

It will be interesting to look back over the century just closing and to observe to what extent the adoption of now familiar plans for improving the performance of the steam engine during the period of its existence—practically coincident in its working life with the nineteenth century—have had the desired result, and how far efficiencies, duties, and thermodynamic operations have been approximated to the figures for the ideal, thermodynamic, machine. The principal directions of general progress have been toward higher engine speed, toward higher steam pressures and correspondingly increased ratios of total expansion, decreased back pressures, superheating and compounding, and the use of improved forms of valves and valve gearing.

Increasing engine speed secures greater immunity from losses by conduction and radiation, within and without, by simply securing a larger amount of work and the use of more steam in the unit of time with a given cylinder volume, thus reducing the waste per unit of weight of steam and of useful work to a lower magnitude. Doubling the speed of engine approximately reduces the waste percentages in proportion to the difference in the squares of the two speeds, while it gives, other things being equal, double the power, thus also reducing costs of construction for stated powers of engine. In the engines of Watt, the steam pumping engine—or the Cornish engine, as it came to be called—was more economical than the same size of rotative engine

of the same builder, because of the fact that, having no flywheel to steady its speed during the piston stroke, it took steam in such a manner as to cause the piston to start with a jump and to traverse the cylinder so rapidly as to give comparatively little time for waste by the condensation of the steam upon the cool surfaces of the metallic walls. It maintained its superiority in this respect until other forms of engine approximated a piston speed approaching that of the older machine, or were provided with arrangements for attaining the same result in reduced wastes by other means.

Modern engines, other things being equal, improve in efficiency and give increased duty as they increase in speed. Fig. 1 shows what has been the extent and rate of progress in this direction in the case of the marine engine, taken as an example of a type, since the beginning of its work and to date; this means, practically, during the nineteenth century, since the work at earlier dates of Fitch and other inventors brought forth no practical results.

FIG. 1.—MARINE ENGINE PISTON SPEEDS, 1800–1900

John Stevens, in 1804, and Robert Fulton, in 1807, were the pioneers in practical employment of the steam engine in marine work, though it should not be forgotten that John Fitch, in the United States, actually transported passengers for a regular fee on a regularly settled route, employing several steamers of small size and very moderate speed, between Philadelphia and Bordentown and Trenton, on the Delaware River, several years earlier, between 1787 and 1791.

In the figure the lowest curve on the diagram represents the progress made in the conservative practice of Watt and his successors and their imitators; the next higher curve shows the advances effected by rivals and more radical constructors from the year 1820 onward; the next in order shows the higher speeds, considered, when Corliss and his contemporaries introduced them, as dangerously high; while the upper curve exhibits the limit of radical practice, the danger line, as it was thought, of the last forty years of the century. Thus it is seen that piston speeds have risen from 200 to 500 feet per minute in marine practice of a conservative kind during the nineteenth century; that what may be to-day called moderate practice has advanced from 300 to 600 feet per minute, while high speeds for their dates have increased from 400 feet at about the middle of the century to 900 feet at its close, and, in radical practice, from 500 to 1,200 feet. In exceptional

instances, or in the effort to accomplish a special tour de force, speeds of considerably greater magnitude have been for a time maintained. The figures here given, however, represent settled practice in the business of certain builders, or in certain classes of constructions. Thus torpedo-boat builders adopt the radical practice, while constructors of small and short-route craft keep speeds down to what they regard as economical and permanently safe rates.

Speeds of engine may be measured either by speeds of piston, as above, or by speeds of rotation, and it is obvious that the latter and the length of stroke of engine piston together determine the speed of the piston. With some engines, as those with detachable valve gear, the speed of rotation is limited to that at which disengagement of the valve may be positively assured; and this with, for example, the Corliss engine is at present not far from 100 revolutions per minute, although instances of much higher speeds are known, and, in

FIG. 2.—SPEEDS OF REVOLUTION (MARINE) 1800-1900

one case at least, a speed of 160 revolutions per minute was maintained for years together.

The method of progress of rotative speeds is shown in fig. 2, and, naturally, follows closely the direction observed in the preceding case. Here the lowest curve in the diagram is that for heavy engines and a very conservative practice; the intermediate line gives the speeds for common good practice at the respective dates; and the higher curve shows the limit of what is considered safe, and a radical practice, where, as in practically all marine engines, no limit to speed of rotation is set, as in so many stationary engines, by the character of the system of steam distribution. Each curve has been given its appropriate scale, and the latter is suitably designated on the margin of the diagram.

Torpedo-boat practice illustrates the highest case, and the work of the average good marine-engine builder the middle case. The lowest

line has risen from 40 revolutions in 1840 to about 100 or 110 in 1890 to 1899, and promises to become, in the best moderate practice, 120 revolutions at the end of the century. The very largest marine engines, with their diameters ranging up to 3 and 4 feet in their high-pressure cylinders, and in low-pressure cylinders 6 and 8 feet, and with their stroke of 5 or 6 feet, are now driven up to 90 and 100 revolutions per minute with apparent safety, and unquestionably gain in economy and in reduced weight and volume. Medium powers and sizes have similarly ranged from 100 to 200 revolutions, and "positive motion valve gears" and the small high-speed engines of torpedo boats have carried radical practice in recent years up to speeds of rotation formerly incredible, now ranging all the way from 400 to 600 revolutions. The steam turbine meantime has set a pace which even the most radical torpedo-boat constructors can never hope even to approach with small engines—5,000 to 10,000 revolutions per minute—

FIG. 3.—PISTON SPEEDS OF LOCOMOTIVES

their largest sizes probably seldom falling much below a speed of 1,000 to 2,000 revolutions.

Fig. 3 exhibits the speeds of piston of the locomotive from the earlier days of its introduction to the present time; in this case, also, the progress, practically, of the century. The lower line represents what seems to have been considered standard practice from the time when there was such a practice; the middle line shows the advances of the century in good common practice, and the upper line is that illustrating a high-speed practice. These deductions, however, are not to be taken as either exact or controlling. The speed of the locomotive is necessarily very variable, the character of its service varies greatly, and builders are controlled by these varying conditions far more than by any considerations of fuel saving.

Ordinary practice became established about 1850, after nearly a half century of experimentation and of variation of type and method of

construction. The standard was set up, it may be fairly asserted, by George and Robert Stephenson about 1830. There is, however, far less variation in the practice of reputable builders in this department of steam-engine construction than in marine practice. It should also be stated that in those earlier days there were occasions on which the engines of the time were forced up to a speed which rivaled that of similarly operated engines of our own day, as when George and Robert Stephenson, in September, 1830, pushed the *Rocket* up to 36 miles an hour, carrying the wounded statesman Huskisson to his home, 15 miles, in twenty-five minutes. That engine was, in 1837, driven up to a speed of 4 miles in four and one-half minutes on the Midgeholme Railway, near Carlisle, a speed of nearly 55 miles an hour.

Common practice during the last half century or more has ranged from the figures of Stephenson and his followers, as above, from 500 or 600 feet per minute piston speed, to about 1,000 at the close of the century, while radically high speeds may be taken as about 30 or even 50 per cent higher in cases of maximum speeds on special occasions.

Steam pressures have been constantly rising since the time of Watt, although, curiously enough, some of the experimental work of the inventors of the marine engine, as well as those of the locomotive, has been done with pressures of considerable magnitude, while the stationary engines of Jacob Perkins were operated at pressures of from 1,000 to 1,500 pounds per square inch, and that inventor, about 1836, proposed pressures of 2,000 pounds. Dr. Albans a little later also adopted pressures of 600 to 800 pounds and worked small engines with, for a time, great economy and without any apparent difficulty. Standard marine practice, however, like the steam pumping engine practice of the early part of the century, involved the employment of steam of little more than atmospheric pressure and permitted but very tardy increase for many years.

Fig. 4 exhibits the general trend of this change at sea, from the early part of the century, in vessels operated on regular routes. For a long time the rise was extremely slow, but at about the middle of the century the introduction of the surface condenser, by permitting the use of fresh water in the boilers, or at least the avoidance of the introduction of sea water, and by thus enabling the engineer to evade the difficulties arising from constant precipitation of solid matter on the heating surfaces of the boiler, caused the adoption of steadily increasing steam pressures and allowed the designer to provide for the utilization of the wider range of working temperatures which accompanied and gave reason for rise of pressure and larger thermodynamic efficiencies.

From that time the rise has been increasingly rapid, and the law of increase with time is shown, with a fair approximation to the mean, by the curve of the diagram. The increased pressure, in turn, made it

necessary to adopt, first, the compound, the double-cylinder engine; then the triple, and finally the quadruple-expansion machine. The compound came in about 1854, the triple in 1874, and the quadruple during the closing years of the century. The demand for increased pressures also compelled a gradual modification of the standard constructions of steam boilers, and finally forced the adoption of the now familiar water-tube boiler, with its externally heated surfaces, a form of boiler original with the earliest inventors of the steam engine of modern type.

The increase in the ratio of expansion adopted from the first has been in a manner fairly constant in its relation to the pressure, and may be roughly taken as, for common practice in condensing engines, the "absolute" steam pressure at the boiler divided by 10 pounds. The terminal pressure, in good practice, has been about 10 pounds, falling, in the engines of highest efficiency and giving maximum duty

FIG. 4.—STEAM PRESSURES IN MARINE ENGINES

for their time, to 8 and occasionally to 7 or even 6 pounds absolute. The precise relation of the ratio of steam pressure to back pressure to the ratio of "total" expansion in all classes of engine has necessarily been affected very appreciably by the degree of approximation secured to truly ideal thermodynamic, adiabatic expansion. Initial condensation and, later, reevaporation have a marked effect upon this relation, and this, in turn, is determined in amount by the character of the construction and the "quality" of the working fluid.

The final improvement of the steam engine, marking the best practice of the century, and particularly of its later years, is that which reduces that variation from the thermodynamic ideal which is consequent upon internal waste due to exchange of heat between the steam and the metal of the working cylinder. Rankine's ideal "cycle of the nonconducting cylinder" can be secured either by actually making the cylinder nonconducting or by giving the steam so nearly gaseous a

quality as to reduce appreciably, if not entirely, this heat exchange. Either fluid or cylinder wall being nonconducting, heat exchange is impossible.

Steam drying and superheating has come to be recognized as an essential process in the economical operation of the steam engine. Separators at or near the engine cylinder are now made very efficient in the removal of all particles of water from the steam entering the engine, and thus superheating is very effectively facilitated; but superheating itself is a problem in construction and in operation which is not even yet completely solved. Nevertheless, all engines exhibiting maximum economy to-day employ steam effectively dried and more or less superheated. These processes are not only practiced in the passage of the steam into the engine, but they are also often employed between cylinders where the engine is of the multiple-cylinder type. Here separation is always practicable and easily made effective; but superheating, even where it is provided for, is seldom accomplished in "reheaters." One heat unit employed in superheating the steam preliminarily to its introduction into the cylinder, whether high-pressure, intermediate, or low, is worth several employed in evaporating additional steam; yet such are the practical difficulties that even the best of modern engines are rarely supplied with steam superheated more than 50° F., and effective superheating between cylinders is very seldom accomplished. Where it is successfully introduced the effect is probably always to very considerably improve the action of the machine and reduce its expenditure of steam and of fuel. The highest modern records are held by engines in which the ideal thermodynamic conditions are most closely approximated in this respect. The usual variation of efficiency with variation of engine speed is not, in this case, so observable, and is far less important.

The fundamental deductions from experience, as well as from scientific examination of the case, and the principles controlling the construction and operation of the steam engine in which high efficiency is sought, are the following:

(1) Make the steam pressure adopted as high as, under existing conditions, is safe.

(2) Adopt the lowest practicable back pressure.

(3) Expand through the widest range of temperature and pressure found commercially satisfactory.

(4) Adopt as high engine speed as is safe.

(5) Employ dry and, if practicable, moderately superheated steam in all cylinders.

(6) So design the machine that friction and external wastes of heat shall be reduced to the lowest practicable amounts.

(7) In the application of any expedient for promoting efficiency, seek that limit at which further gain is compensated by the additional

costs of its production. In choosing an engine type for any application, seek that which returns in useful power the largest amount of value for each unit expended in its procurement.

Progress in the improvement of the steam engine is measured by the gain in "duty" secured by improvement in its construction and operation. This gain is exhibited in fig. 5, in which are presented the curves of mean efficiency of the steam engine of the best types from the time of Smeaton and the Newcomen engine to the end of the nineteenth century.

A duty curve measures the gain in amount of useful work performed by the unit of fuel consumed; the curve of heat and steam and fuel consumption exhibits the quantity consumed per horsepower and per hour. It may be also noted that the internal wastes of the engine, at first constituting 95 per cent of all the heat and steam and fuel supplied, have become extinguished to such an extent that 80 per cent or more of the steam has become available for use in the engine cylinder.

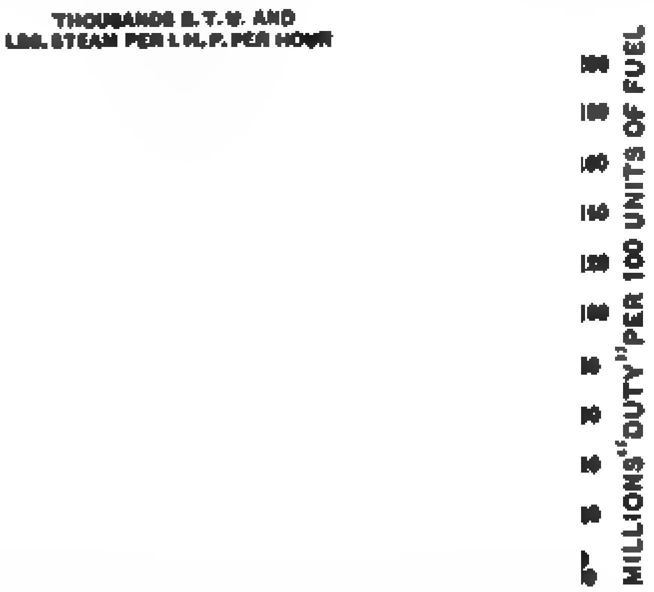

FIG. 5.—PROGRESS OF STEAM ENGINE EFFICIENCY, 1750–1900

The curve of heat, steam, and fuel consumption is, perhaps, the most familiar measure of the growth of the engine efficiency during the century just elapsing. The scale is one of thousands of British thermal units per indicated horsepower per hour and of pounds of steam for similar units, it being assumed that each pound stores 1,000 heat units between feed water and steam temperatures. It is also a scale of tenths of a pound per horsepower per hour, assuming the most efficient of steam boilers– with an evaporation of 10 pounds of steam per pound of fuel—to be employed. It will be seen that the gain has recently approximated 20,000,000 foot-pounds per 100 pounds of fuel on the duty scale, 1 pound of steam and one-tenth pound of fuel, per decade, on the scale of heat expenditure, and that the decrease in magnitude of internal wastes has been, and is at present, about 1 per cent per decade. These rates of gain may be taken as those of

our own time, and slightly lesser gains, with a progressively decreasing rate of gain, are likely to continue for the immediate future, precisely as the rates of increase of steam pressure, of expansion ratios, and of engine speeds may be expected to extend the curves, through the next decade or more, along the same directions as hitherto observable in the diagrams, provided no unexpected change, due to invention or the approach of the curve to an as yet unknown critical point, shall compel a change in the law of progress. No such change affecting our prophecy, we have a safe, a scientific, and an instructive and availably useful prediction. "Science here reads an oracle."

The limit for the immediate future would seem to be about 10 pounds of steam, 1 pound of fuel, and something inside 200,000,000 foot-pounds duty, beyond which figure it would be rash to expect further progress, except under conditions still beyond the view of the engineer of this time.

Individual engines have excelled in efficiency the records here indicated as the best general results of the progressive improvement of the century. It may prove interesting to gauge both the approximation of the averages already presented and of the individual machine to the ideally perfect steam engine. Were it practicable to produce an absolutely perfect thermodynamic machine, whether steam engine or any other form of heat engine, and whether operated with gas, vapor, liquid, or even solid working substance, its maximum efficiency would not be unity, but that fraction which is measured by the ratio of the working range of temperature to the absolute temperature of its maximum limit, the Carnot efficiency. This is therefore what must be accepted as the standard with which to compare any given case. Numerically it is a variable quantity, obviously increasing with the elevation of the steam pressure in the case of the steam engine. It is known to be proportional very closely to the logarithm of that pressure where the back pressure is a practical minimum. Its value is sufficiently accurately given for present purposes by the expression, measuring costs in steam, heat, and fuel,

$$Q = a \div \log p',$$

in which for heat units per horsepower per hour a may be assumed to be about 15,000; for steam in pounds per horsepower per hour, a may be taken at 15; and for fuel take a at 1.5. For the measure of efficiency, unity as the standard, we will employ the expression,

$$E = 12.5 \log p',$$

which will serve within the customary range of steam pressures.

Employing these several expressions, it is seen that the efficiency of the Carnot engine, under the usual conditions of pressure range, may be taken at 25 per cent for 100 pounds steam pressure, and that the

rise of the pressure to 1,000 pounds would give approximately 37.5 per cent efficiency. Meantime the expenditure in heat units would be 7,500 per horsepower per hour; that of steam, at 1,000 units per pound. would be 7.5; and that of fuel about 0.75 pound at the lower pressure; while at the higher these figures would become 5,000 B. T. U., 5 pounds of steam, and 0.5 pound of good fuel burned in a boiler of high efficiency.

The Rankine cycle, defective in its lack of that compression which is an essential characteristic of the Carnot cycle, gives constants in our equations about 20 per cent above those of the latter, measuring heat, steam, and fuel consumption, and proportionally lower in measures of efficiency. Where heat wastes occur, as in the real case, to the extent

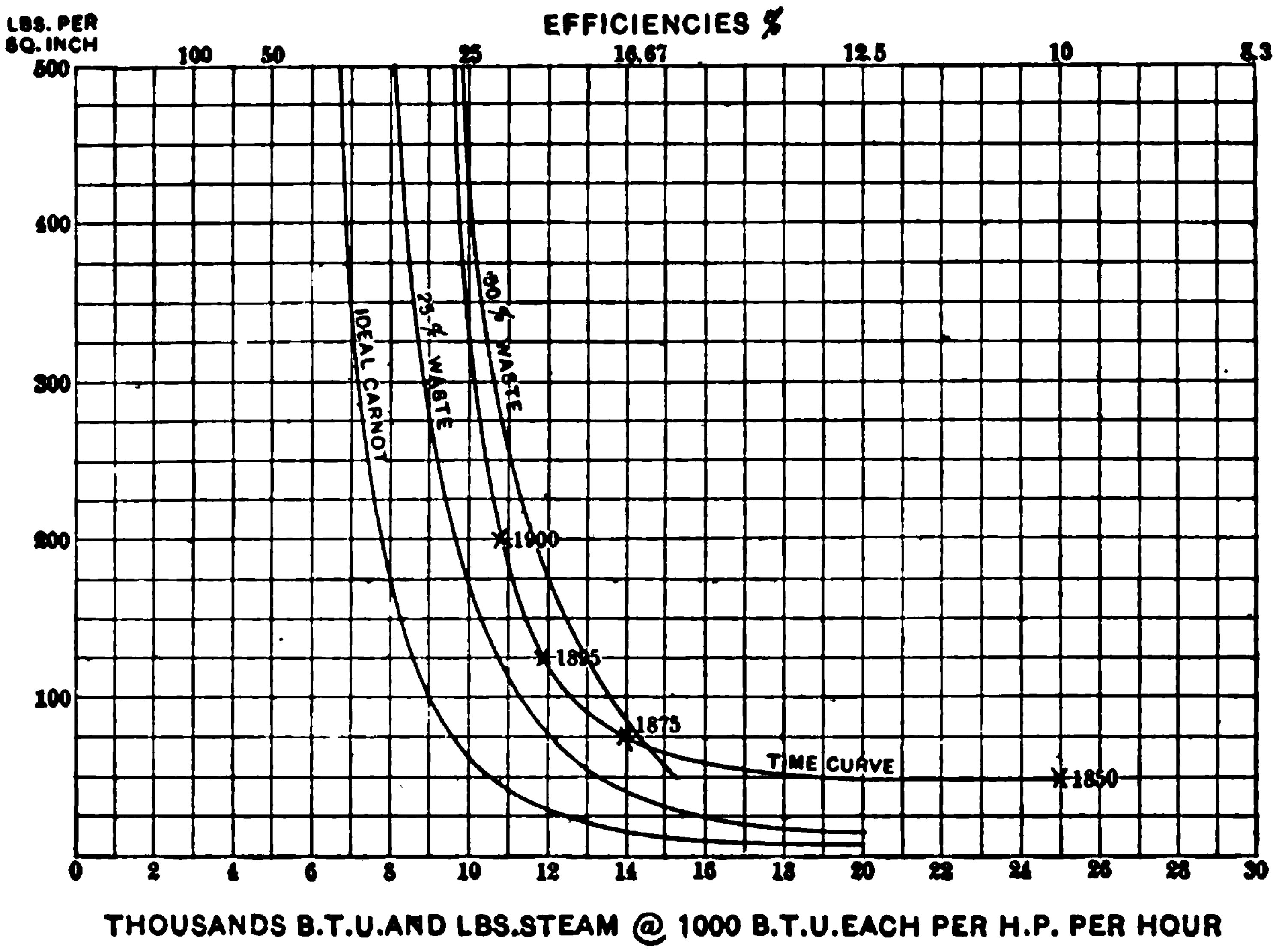

FIG. 6.—GAIN IN EFFICIENCY

of 20 per cent or more, these variations from the ideal case become proportionately increased. The expenditures of the best engines will average, in this case, probably 20 per cent internal waste and the constants become about 18,000 and 22,000 for the ideal and the real case, respectively, as measuring heat expenditure, and 15 and 18 for the constant in the measure of efficiency.

Fig. 6 shows, on this basis, the ideal limit of the Carnot cycle, measuring efficiency by expenditure of heat in thousands B. T. U., costs in steam and in fuel being computed on the assumption of 1,000 B. T. U. per pound, and 10 pounds evaporation per pound of best fuel in the best steam boilers.

The scale and diagrams are constructed for a pressure limit of 500 pounds per square inch.

The curve at the left of this diagram represents the ideal case of Carnot and its increasing efficiency as the pressure employed rises from the low figures of the middle of the century and earlier to the maximum for the advanced practice of leading engineers of to-day. The costs of the horsepower range from between 12 and 13 pounds of steam per hour at the minimum pressure to approximately 9 pounds at 100 pounds pressure, 8 and 7, respectively, at 200 and 300 pounds pressure, and about 6¾ at 500. With 1,000 pounds boiler pressure the figure should drop to about 6 pounds of steam per horsepower per hour.

It seems entirely practicable, so far as experience to date goes, to secure quite as close an approximation to the ideal case in the real engine at high as at low pressures. A waste of from 25 to 50 per cent may be taken as a common range of efficiency loss in steam engines by reputable builders, multiple-cylinder engines being employed for all pressures exceeding 100 pounds boiler pressure, and the steam jacket and moderate superheating being adopted for the most efficient machine, especially when, as is usual with pumping engines, having a low piston speed. Curves are inscribed on the diagram with these amounts of waste, and the area bounded by them may be taken as that occupied by modern good practice up to the present limit of good and common practice. The facts that the trend of existing and earlier practice so closely follows these lines and that the only experiments scientifically conducted and recorded to date for the maximum limit show the accuracy of the preceding conclusion, give us good basis for these general deductions.

The weight of steam per indicated horsepower per hour is here given as $W = 18 \div \log p$ for the ideal case, while experience gives about $W = 25 \div \log p$ for good practice up to the highest limits yet accepted as standard. On the diagram, fig 6, are inscribed, also, the dates at which the noted efficiencies were attained by good builders generally and the approximate record for the close of the century. It will be found that these chronological observations fall into a fairly smooth curve, and the deduction is as inevitable that not only will steam pressures and expansion ratios continue to increase in the immediate future, but also that improvement may be expected to continue in this direction, slowly with respect to rising efficiencies, rapidly in increasing pressures, as improved forms of steam boilers make it safe to employ such pressures, and as users and builders gradually yield their long-existing prejudices against high pressures.

We may expect in a very few years more to see steam pressures for engines of high efficiency range from 500 to 1,000 pounds per square inch, the quality of the steam being maintained at a high fraction by preliminary superheating on at least a moderate scale, with reheater superheating between cylinders in series, and with jackets on heads of low-speed pumping engines, as now practiced. At the rate at which

"safety boilers" are being improved and introduced at the close of the century, we may confidently anticipate that standard pressures will rise very rapidly until this revolution in boiler construction is completely effected. The twentieth century opens with the record for costs of power reduced to 10 pounds of steam, nearly, per horsepower per hour, and the next century will undoubtedly see the approximation to the ideal case made much closer, while the ideal costs will be as certainly reduced from 10,000 B. T. U. per hour, nearly, to decidedly lower figures.

Gain must, however, be expected to be comparatively slow in the coming century, both because of the fact that the great wastes of the beginning of the nineteenth century have already been largely reduced, leaving comparatively little opportunity to effect improvement in that respect, and also because, under any circumstances, the progress of improvement must always be at a constantly decreasing rate. If the coming century sees the costs of the indicated horsepower reduced to as little as 8,000 B. T. U. per hour, or to 8 pounds of steam, or to three-quarters of a pound of the best fuel, burned in the best boilers, it will be probably quite as great an advance as can fairly be anticipated for the first century of the next millenium.

BUNSEN MEMORIAL LECTURE.[1]

By Sir Henry Roscoe, B. A., Ph. D., D. C. L., LL. D., D. Sc., F. R. S., Hon. M. D. (Heidelberg).

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[Delivered on March 29, 1900.]

The death of Bunsen at Heidelberg, on August 16, 1899, severs the last link connecting the chemists of our time with the great men of the earlier part of the century. With Berzelius, of whom Bunsen writes as "my truest friend and counsellor who, during the whole of my scientific life, has stood to me in intimate personal relationship;"[2] with Gay-Lussac, in whose laboratory in Paris he worked in the year 1833; with Dumas, whose acquaintance and friendship he enjoyed when they both were young; with Liebig and Wöhler, who were more nearly his contemporaries, and for whom throughout their lives he entertained the warmest feelings of affectionate regard; with the Berlin chemists, Mitscherlich and the two Roses, as well as with the older physicists, Dove, Wilhelm Weber, and Magnus, all of whom he counted among his personal friends.

Moreover, living to the ripe age of 88, he was destined to witness the deaths as well as the scientific births of many distinguished colleagues and pupils: Of Kirchhoff, Helmholtz, Kopp, and Hofmann; of Strecker, Kolbe, Kekulé, Pebal, Lothar Meyer; and, lastly, of his successor in the chair of chemistry at Heidelberg, Victor Meyer. So that in his later years Bunsen stood alone in his glory, like some strong oak in the forest which still holds firm root unmoved by the tempests which have smitten down both old and young around it.

Nearly twenty years ago I gave, in the columns of Nature, a sketch of the scientific work of him whose memory we are here assembled to

[1] From Transactions of The Chemical Society, London, vol. 77, pp. 513–554.

[2] Berzelius, on his side, fully appreciated Bunsen's character and ability. In 1844 he writes to Schönbein apropos of the latter's ozone experiments: "You must devote all your time to this so important investigation, you must follow it up with the true perseverance of a Bunsen, and if possible not abandon it until we are perfectly clear about it." (Kahlbaum Briefwechsel, Berzelius-Schönbein, 1898, p. 60.)

honor, as being not merely one of the most distinguished of the great chemists of the century, but one of the truest and noblest of men. In introducing the subject to the readers of that journal I used the following words, which I make no apology for quoting, as I can not find more appropriate expressions wherewith to commence the more detailed account of the life and labors of Bunsen in the memorial lecture, with the preparation of which the council of the chemical society has honored me.

"The value of a life devoted to original scientific work is measured by the new paths and new fields which such work opens out. In this respect the labors of Robert Wilhelm Bunsen stand second to those of no chemist of his time. Outwardly, the existence of such a man, attached, as Bunsen had been from the first, exclusively to his science, seems to glide silently on without causes for excitement or stirring incident. His inward life however is, on the contrary, full of interests and of incidents of even a striking and exciting kind. The discovery of a fact which overthrows or remodels our ideas on a whole branch of science; the experimental proof of a general law hitherto unrecognized; the employment of a new and happy combination of known facts to effect an invention of general applicability and utility; these are the peaceful victories of the man of science which may well be thought to outweigh the high-sounding achievements of the more public professions."

Owing to the fact, not common in the annals of scientific intercourse, that I have enjoyed the privilege for nearly half a century of counting Bunsen among my most intimate friends, that we have stood in the position first of pupil and master, and afterwards of colleagues and coworkers, I am in the fortunate position of being able to present to you on this occasion something more than a description of the scientific work which he accomplished, the record of which anyone who cares to do so can gather up from his published memoirs. From my personal recollections I propose to lay before you a picture, doubtless imperfectly, but so far as my abilities go, truly drawn, of the man working in his laboratory, lecturing to his students, and enjoying simple but refined social intercourse with his friends. I shall hope to give you an idea what manner of man he was, what was his moral, as well as his scientific character, to point out why he was not only venerated as a great leader and teacher, but why he inspired all with whom he came in contact with feelings of deep attachment and regard.

But first let me shortly mention some few particulars of his life, and give you a summary of his most important investigations. Bunsen was born at Göttingen on March 31, 1811. His father, Christian Bunsen,[1] occupied the position of chief university librarian and professor of modern philology. After passing through the usual course in the

[1] Born at Frankfort on April 1, 1770; died March 24, 1837.

gymnasium at Holzminden in Hanover, Bunsen entered the University in 1828, studied chemistry under Stromeyer (the discoverer of cadmium), obtained his degree in 1830, presenting for this purpose a thesis having the title "Enumeratio et descriptio hygrometrorum." He then visited Paris; arriving there at the latter end of September, 1832, he remained until the spring of 1833, meeting Reiset, Regnault, Pelouze, and Despretz. The latter proposed to Bunsen to work in common at some problem in physical chemistry. Subsequently having visited Berlin and Vienna, continuing his studies and making acquaintance with the scientific men of those cities, Bunsen returned to Göttingen, where, in 1834, he was admitted by the university as privatdozent in chemistry. In this position he lectured for three semesters, and after Stroymeyer's death in 1835, Bunsen temporarily took up his work and lectured six days a week on theoretical and practical chemistry. In January, 1836, he was appointed teacher of chemistry in the Polytechnic School of Cassel as Wöhler's successor. In October, 1839, he became professor extraordinarius of chemistry in the University of Marburg, and in 1842 was advanced to the position of professor ordinarius. Remaining here until 1851, he went for a short period to Breslau, and in 1852 he accepted the chair at Heidelberg vacated by Gmelin, a post which he occupied until his retirement in 1889. In these several positions Bunsen labored incessantly and devotedly for fifty-six years in the furtherance of chemical science, with the result that his name will be handed down to posterity as one whose work has earned for him the very first rank among chemists of the nineteenth century.

On the present occasion it is not possible to do more than to indicate the nature and extent of Bunsen's work, so numerous are his published investigations and so wide and far-reaching their scope. To bring before you the general effect of the work, and to give you by some examples the special characteristics of that work, is all that can be now attempted. And for these objects I propose to treat the matter rather by classifying his work under separate heads of subjects than by taking it in the chronological order of publication.

But before commencing a review of some of his most important researches it may be well briefly to refer to the early work by which he won his scientific spurs. The first paper was one of general interest, as recording his discovery that freshly precipitated hydrated ferric oxide acts as a powerful antidote to arsenical poisoning by rendering the arsenic insoluble both in water and in the secretions of the body. This result of the withdrawal of the whole of the arsenic from solution by this means forms a striking lecture experiment (Journ. de Pharm., 1834 (20), 567; 1838 (24), 93).

His next communication shows the interest which, in those early days, Bunsen took in mineralogy and chemical geology, subjects in

which he in after life became a distinguished exponent. It consisted of an exact analysis and a detailed description of a specimen of allophane from a lignite bed near Bonn (Pogg. Ann., 1834 (31), 53).

A more specially chemical subject next engaged his attention, namely, an investigation of a new series of double cyanides, in which he not only determined their composition with exactitude, but showed the relationship existing between these and other well-known members of the same class of bodies. He measured their crystalline form and proved that ammonium ferrocyanide is isomorphous with the corresponding potassium salt (Pogg. Ann., 1835 (34), 131; 1835 (36), 404; 1836 (38), 208).

All this work was, however, merely of the nature of what he was in the habit of calling "ein kleiner Vorversuch" when he was indicating the manner in which a pupil should commence an investigation.

The first research in which Bunsen showed his power was the classical one on the cacodyl compounds—begun in Cassel in 1837 and continued in Marburg for no less than six years. The publication of this work placed Bunsen at once in the front rank of experimentalists.

To assist in forming an estimate of the scientific value of these researches (Pogg. Ann., 1837 (40), 219; 1837 (42), 145; Annalen, 1841, (37), 1; 1842 (42), 14; 1843 (46), 1), it may be well to summon to our aid the opinion of a contemporary whom Bunsen himself, as we have seen, considered as a master, both from a philosophical and from an experimental point of view, the great Swedish chemist Berzelius.

Those who have studied his celebrated Jahresbericht will know that Berzelius was unsparing in his criticism of inaccurate work and of illogical conclusions. The more valuable, therefore, and reliable are his remarks when favorable to the subject under review.

In 1839 (Jahresber., 1839 (18), 487), Berzelius writes:

"An extremely important discovery has been made by Bunsen, in the investigation of the well-known fuming, self-inflammable liquid (Cadet's fuming arsenical liquid) obtained when anhydrous acetate of potash is distilled with arsenious acid. From this body Bunsen has prepared several substances whose properties resemble those of an organic compound, in which, however, arsenic enters as an elementary constituent."

Of the importance of this research as affecting chemical theory, Berzelius reports (Jahresber., 1841 (20), 526):

In the last German edition of my handbook, I gave what I considered the probable theoretical views regarding this substance, namely, that it contains the compound radical $C_4H_{12}As_2$, similar to the radicals contained in the organic bodies, for which I have suggested to Bunsen the name 'kakodyl' in consequence of the nauseous smell of its compounds. With regard to this name Bunsen writes me as follows: 'The view of the existence of a ternary radical $Kd = C_4H_{12}As_2$ agrees so per-

fectly with the behavior of the whole alkarsin group that it would be scarcely possible to find a more striking example of a compound radical. Alkarsin is kakodyl oxide, Kd; it can be directly oxidized and deoxidized. Alkargen is kakodylic acid, Kd.'"

In a further notice (Jahresber., 1842 (21), 503) Berzelius writes:

"By this investigation Bunsen has made his name memorable. Chemical science is bound to acknowledge its debt to him for the investigation of a subject at once so important and so dangerous—an investigation of which it may well be said that it leaves little to be desired."

Again he reports (Jahresber., 1845 (24), 640):

"Bunsen has now concluded his investigation on kakodyl. Through the private communications with which the author has favored me, I have been able each year to give an account of the experiments as they progressed. The research is a foundation stone of the theory of compound radicals of which kakodyl is the only one the properties of which in every particular correspond with those of the simple radicals."

And he concludes his criticism with a paragraph referring once more to the importance of this tedious and difficult research.

To quote another opinion, that of one of the leaders of modern chemical science, to place side by side with that of the great Swede, I would refer to that expressed by Adolf von Baeyer in his editorial remarks in the reprint of Bunsen's work in Ostwald's collection of scientific classics:

"These researches have long been considered classical, and they deserve such praise, particularly as pieces of model investigation demonstrating how the most difficult problems of experimental chemistry can be solved by a master's hand."

Among the many remarkable new facts which these researches contain is that of the nonpoisonous properties of cacodylic acid, although it contains no less than 54 per cent of soluble arsenic.

"A solution of 8 grains of cacodylic acid injected into the vena jugularis of a rabbit produced no deleterious result on the health of the animal."

It is also of interest to read Bunsen's description of the properties of cacodyl cyanide, by the explosion of which he lost the sight of his right eye, was nearly poisoned, lying for days between life and death, but the investigation of which he nevertheless brought to a satisfactory conclusion.

"It is obtained when cacodyl oxide is distilled with mercury cyanide, when it sublimes to a camphor-like solid; it melts at 32.5° to an oily liquid. The smell of this body produces instantaneous tingling of the hands and feet, and even giddiness and insensibility. The cacodyl compounds appear to exert a specific action on the nervous system. It is remarkable that when one is exposed to the smell of these com-

pounds the tongue becomes covered with a black coating, even when no further evil effects are noticeable."

Respecting the constitution of the radical of the cacodyl compounds, various theories have from time to time been put forward. Bunsen himself did not give any opinion on the point, and it was Kolbe who first suggested the view that it was arsine-dimethyl, $As(CH_3)_2$, while the experiments of Frankland, and subsequently those of Cahours and Riche, rendered this probable. It is, however, to the researches of Adolf von Baeyer (Annalen, 1853 (107), 257) on the arsenmonomethyl compounds that we owe the full explanation of the relation which these various bodies bear to one another.

The cacodyl research claims our interest not only because, as we have seen, it furnishes us with the first example of an isolable radical, but also because it assisted Frankland and Kekulé in more exactly illustrating the term "chemical valency." For it is not too much to say that the subsequent researches of Frankland on the organo-metallic bodies and on the so-called alcohol radicals, as well as those of the French chemists, and, I may add, those of Baeyer, received their first impulse from the cacodyl investigation. This indebtedness was acknowledged by our late lamented Fellow in the graceful and modest words which appear in the dedication of the volume of his collected researches:

"To my friend and teacher, Robert William Bunsen, whose researches on cacodyl, on the gases of the iron furnaces, and on the volcanic phenomena of Iceland I have always regarded as models of investigation in pure, applied, and physical chemistry, I dedicate these pages, both as a testimony of my regard and in gratitude for the teaching whereby he imbued me with the necessity for thoroughness and accuracy in all scientific work. Would that they were more worthy of such a high standard."

Thus it is seen that, although this remarkable research is the only one of any importance which was carried out by Bunsen in the domain of organic chemistry, it was destined to exert such an influence on the later developments of that branch of the science that he may with truth be regarded as one of the pioneers of modern organic chemistry.

I now pass to an investigation of a different type, but one not less important or interesting than the last.

Up to the year 1838, when Bunsen began his investigation of the composition of the gases of the iron furnaces, the mode of measuring gaseous volumes and the methods adopted for the separation of the several gases were faulty and inaccurate in the extreme. But during the period elapsing between the above year and 1845 Bunsen had not only elaborated and perfected his well-known gasometric methods, but had applied these methods with signal success to the investigation of

the chemical changes which occur in the processes of a most important industry, that of the production of cast iron in the blast furnace.

The first detailed description of Bunsen's gasometric methods was published in pamphlet form by Kolbe, who was at the time one of Bunsen's assistants. To the English public these methods became known by a communication made to the meeting of the British Association at Cambridge in 1845 by R. W. Bunsen and Lyon Playfair, entitled "On the gases evolved from iron furnaces with reference to the smelting of iron." Before entering upon the technical side of the question, the authors give experimental proofs concerning the accuracy and reliability of the methods employed for the measurement and the separation of the blast-furnace gases. One of these consisted in the analyses of a large number of samples of air. These were collected and analyzed in Marburg, and gave analytical results upon which Bunsen reports as follows:

"The close agreement of these experiments with one another, and with the result obtained by the careful experimental determination of the composition of air by Dumas, proves that the eudiometric examination of gases admits of a degree of exactness which is certainly not surpassed by the most minute chemical methods, and they further show that the presence of nitrogen does not exert any disturbing influence on the estimation of explosive mixtures of gases."

The report, printed in full in the British Association volume for 1845, next proceeds to discuss the experiments made by Bunsen on the composition of the gases evolved in the process of iron smelting in furnaces fed with charcoal and using cold blast at Vickerhagen, in Germany. From these, it appeared that in such furnaces nearly half the heat of the fuel consumed was evolved in the escaping gases.

The importance of these investigations, as being the first attempt to introduce accurate scientific inquiry into so widespread an industry as that of iron smelting, was at once appreciated by Lyon Playfair, who had made Bunsen's acquaintance at Marburg. In consequence, at Playfair's suggestion, Bunsen consented to visit England, and undertook to carry out a similar set of experiments for the English furnaces fed with coke and coal, and worked both by hot and by cold blast, to those which he had previously made in Germany. Thus was initiated a research which may be truly said to be a model of the application of the methods of scientific investigation to the elucidation of industrial problems. For not only did it clearly reveal the nature of the chemical changes which take place throughout the furnace, but pointed out the direction in which economies to an undreamt-of extent might be effected in the processes as then carried on. Thus it proved that while about half the fuel was lost as escaping gas in the German furnaces, no less than 81.5 per cent was lost in English ones, and, what was important from the industrial point of view, it pointed out that the

whole of the heat thus allowed to escape might without difficulty be utilized for the various purposes of the works. These suggestions were only slowly adopted by the ironmasters; six years elapsed before any steps in the direction indicated were taken, but gradually the importance of the proposal was appreciated, and now and for many years past the whole of the hitherto wasted heat has been utilized and economies effected of which the value may be reckoned by millions rather than by thousands of pounds.

Not only is it the lost heat which has been recovered, but also valuable by-products, the existence of which had been up to that time entirely ignored. The report points out the loss of combined nitrogen, both as ammonia and cyanogen, which the process as then carried out evolves, the upper part of the furnace being, in the words of the report, "a region of distillation and not of combustion." The amount of loss of these valuable materials was ascertained by accurate analysis, and a method for recovering them suggested, "without increasing the cost of manufacture or in the slightest degree affecting the process of smelting." Apropos of the determination of the escaping cyanogen compounds, the occurrence of a singular accident to Bunsen, as related by Playfair, is found in the admirable life lately written by Wemyss Reid: "Bunsen was engaged below," at the blast furnaces at Alfreton, in Derbyshire, "and I above, passing the gases through water to collect any soluble products, when I was alarmed by being told that my friend had become suddenly ill. I ran down and saw white fumes coming out of a lateral tube, and Bunsen apparently recovering from a fainting condition. I applied my nose to the orifice and smelt the vapor of cyanide of potassium, which gave an entirely new light to the processes of the furnace."

In 1857 Bunsen collected in a volume—the only book he ever published—the whole of his gasometric researches, and of this a second and greatly enlarged edition appeared in 1877 (Gasometric Methods, by R. W. Bunsen, translated by H. E. Roscoe, 1857). No better or more complete method of learning what Bunsen's work is like can be taken than that of reading this volume. For originality of conception, for success in overcoming difficulties, for ingenuity in the construction of apparatus, and for accurate work, I believe the book, as a record of experiment, to be unequaled.

The first part contains a description of his various processes for collecting, preserving, and measuring gases, different methods being employed for the first of these according to the source from which the gases are obtained, whether, as has been described, from blast furnaces or from fumeroles, from volcanic vents or when freely rising from mineral springs, or whether the gases are contained in solution in river or spring water. In the second part we find a full description of the

methods of eudiometric analysis, giving details of manipulation, with a discussion in each case of the probable sources of error and of the means of their limitation. As a model of accurate work (his oxygen determinations in air showed differences of 0.1 per cent on the oxygen) Bunsen's eudiometric methods will always remain as the standard. More expeditious and simpler methods have been introduced of late years, but none of these equals the original processes in exactitude.

The third portion of the volume consists of a description of two new methods for determining the specific gravity of gases. The first of these, which also applies to the case of vapors, consists in weighing a tared vessel, filled first with the gas or vapor under examination and then with air, all variations due to change of temperature and pressure being eliminated by a simple and ingenious compensating arrangement. Perhaps the most interesting portion of this section is a description of a new thermostat, by means of which perfectly constant temperatures up to a high point can be obtained. This served Bunsen for ascertaining the specific gravity of aqueous vapor at different temperatures, and closely accordant numbers were obtained, although the weight of vapor amounted to only 80 milligrams. The second method, applicable only to gases, depends on the determination of the rates of diffusion of the gases into air. Here, too, the volume of gas operated upon need not exceed 50 to 60 cubic centimeters, and yet the results obtained are extremely accurate. On this point Bunsen remarks that for technical purposes—as, for example, for the determination of the density of coal gas—the above simple method will probably be found preferable to all other processes.

The fourth part contains a series of investigations on the absorptiometric phenomena of gases in water and alcohol, the experiments having been chiefly undertaken with a view of determining the limits to which the well-known laws of pressure hold good. First he describes his absorptiometer, a new instrument by means of which it is possible to obtain accurate numbers with relatively small volumes of the gases. The absorption coefficients of no fewer than twenty-seven different gases in water and alcohol were determined by methods varying according to the nature of the case, partly carried out by himself and partly by many of his pupils, the result being that certain gases, generally those least soluble in water, are found to be in accord with Dalton's law of pressures and Dalton and Henry's law of partial pressures, whereas the more soluble gases are not always in accord with them. In the former class it is possible, from an experimental determination of the coefficient of absorption, to calculate the composition of the original gas, the composition derived from an absorptiometric analysis being found to agree exactly with that obtained by direct eudiometric measurements. It is also possible to ascertain whether a given gas consists of a single substance or is a mixture of

several. Thus, while eudiometric analysis can not decide whether the gas evolved by the action of caustic alkali on an acetate is methane or a mixture of equal volumes of ethane and hydrogen, this can readily be accomplished by absorptiometric methods.

In the fifth part of the volume he discusses the phenomena of gaseous diffusion, and, although admitting the truth of Graham's law for cases of pure diffusion, he obtained results, when a stucco diaphragm of considerable thickness is used, which are not in accord with this law, the conclusion being that the pores of gypsum act upon gases, not as a series of fine openings, but rather as a series of capillary tubes, the phenomena being thus modified by those of transpiration. At the end of this chapter he describes the details of a method for ascertaining, by diffusion, whether a given gas is a mixture or not.

The sixth and last section relates to the combustion phenomena of gases. The temperature of combustion—that is, the temperature of the interior of a mixture of burning gases—can be calculated from the heat of combustion of the gaseous mixture and the specific heats of the products of combustion under the assumption that the combustion at this high temperature is perfect. This condition, however, is not fulfilled, and Bunsen therefore endeavored to determine this temperature by another means, namely, by measuring the pressure produced at the moment of explosion of an inclosed gaseous mixture.

For this purpose he constructed a wonderfully simple apparatus, by means of which he ascertained that the maximum temperature of combustion of carbon monoxide and of hydrogen with the theoretical volume of oxygen was, respectively, 3,033° and 2,844°. He likewise attempted to determine the rate at which the explosion is propagated, and came to the conclusion that for hydrogen and oxygen this was 34 meters per second. Subsequent experiments, especially those of Dixon (Phil. Trans., 1893 (184), 97), have shown that this rate referred only to the initial period of the combination before the explosion wave had attained its maximum velocity, this latter amounting in the case of hydrogen and oxygen to the high number of nearly 3,000 meters per second, the rate in other gases being of the same order in magnitude, and the ignition appearing to be propagated in somewhat the same manner as a sound wave.

One of the best known of Bunsen's discoveries is that of the carbon-zinc battery, which bears his name.

The construction of this battery in 1841 (Annalen, 1841 (38), 311) marks an era in the economic production of electricity. By the replacement of carbon for the platinum plates of Grove, Bunsen not only greatly reduced the initial cost, but increased the length of time during which the current can be maintained at its maximum. The success of the invention depends upon a method he devised for overcoming the disintegrating action on the carbon of concentrated

nitric acid. This he effected by strongly igniting the cylinders, thus foreshadowing the process adopted on a large scale for graphitizing the carbon poles now so generally used for electro-industrial purposes by ignition in the electric furnace. It is interesting to remember that it was Bunsen who, so early as 1843, pointed out that the electric current could be made use of as a means of illumination. He describes how, by using a battery of 44 of his elements, a light equal in illuminating power to 1,171.3 candles can be obtained for an expenditure of 1 pound of zinc per hour, and giving a light "the brilliancy of which the eye can scarcely support." He adds that by inclosing the carbon poles in a globe of glass the wear of carbons by oxidation might be minimized. In short, he describes the first step toward the modern system of arc lighting rendered generally applicable on the large scale by the discovery of the dynamo. In his first communication respecting this battery Bunsen gave a careful estimate of the work it can accomplish. He showed that three cells will, in thirty minutes, decompose 0.6775 gram of water, yielding 1,137 cubic centimeters of mixed gas, measured at 0° and 760 millimeters. The corresponding loss of zinc in each cell was then determined, the result showing that the same weight was dissolved in each, and that the weights thus found correspond closely with the zinc equivalent for the above amount of water decomposed. A few years later, in 1848, he determined the electro-chemical equivalents for zinc and water. For the first of these he obtained the value 0.033, and for the latter 0.00927; in other words, in order to decompose 1 milligram of water per second a current of the absolute intensity of 106.33 is necessary. These experiments confirm Faraday's law, showing that the quantity of water decomposed is proportional to the quantity of circulating electricity, and that the nature of the poles, as well as the conducting power of the liquids decomposed, exerts no influence on the result.

We owe to Wilhelm Weber the first determination of the scientific units for electrical measurements, and in 1840 he obtained the number 0.009376 for the electro-chemical equivalent of water with his unit current. The difficulties which surround the subject are: (1) The measurement of the current, and (2) the absorption of the decomposed gases by water and electrodes, and (3) the production of ozone. Bunsen improved the voltameter by evolving the mixed gases from hot acidified water, by which the second and third of these difficulties were overcome. At present voltameters depositing copper or silver are employed, and the ampere, which is now our practical unit, is one-tenth of that used by Weber and Bunsen, so that the electro-chemical equivalent of water is 0.0009315 gram, meaning that 1 ampere decomposes that amount of water in one second.

This, however, was only the beginning of the work which the Bunsen battery was destined to perform. It was not until 1852, when in

Breslau, that Bunsen turned his attention to using the battery for electrolytic preparation of metals, some of which had not been obtained in a coherent condition, and others had only been prepared in such minute quantities that their physical and chemical properties could not be properly studied. The first of these metals he attacked was magnesium (Annalen, 1852 (32), 137), the reduction of which had vainly been attempted by Davy, and only with very partial success by Bussy in 1830. The difficulty which had hitherto stood in the way was the fact that the globules of molten magnesium are lighter than the fused magnesium chloride used as the electrolyte, and that on their formation they rise to the surface and burn. To avoid this Bunsen adopted the ingenious plan of cutting the carbon pole, on which the metal forms, into pockets, inside which the magnesium is deposited, and from which the molten globule can not escape. By means of the tangent galvanometer Bunsen measured the absolute units the electricity employed, finding that the quantity of magnesium reduced is 2.45 grams, while the theoretical yield of metal is 4.096 grams. Having obtained the metal in some quantity, he determined its physical and chemical properties, showed how it could be pressed out into wire, and measured the luminous intensity of the burning metal. This he found to be 500 times that of a candle flame.

Some seven years later, he and I measured the actinic value of the light emitted by burning magnesium, and showed that it could be used for photographic purposes. We found that a burning surface of magnesium wire, which, seen from a point of the sea's level, has an apparent magnitude equal to that of the sun, effects at that point the same chemical action as the sun would do if shining from a cloudless sky at the height of 9° 53′ above the horizon. On comparing the visible brightness of these two sources of light, it was found that the brightness of the sun's disk, as measured by the eye, is 524.7 times as great as that of burning magnesium when the sun's zenith distance is 67° 22′, while at the same zenith distance the sun's chemical brightness is only 36.6 times as great. Hence the value of this light as a source of chemically active rays for photographic purposes is at once apparent. The application of magnesium as a source of light has become of technical importance. A burning magnesium wire of the thickness of 0.297 millimeter evolves as much light as 74 stearin candles of which five go to the pound. If this light lasted one minute, 0.987 meter of wire, weighing 0.120 gram, would be burnt. In order to produce a light equal to 74 candles burning for ten hours, whereby 20 pounds of stearin is consumed, 72.2 grams or 2½ ounces of magnesium would be needed. The light from burning magnesium has been employed for signaling, and for military and naval purposes, and it is especially used in pyrotechny.

Perhaps the most interesting of these applications of the battery is that of the preparation of the metals of the alkal[illegible] (Journ.

de Pharm., 1854 (26), 311; 1855 (28), 155), the isolation of which had hitherto eluded pursuit, and this work he handed over to our countryman, Augustus Matthiessen, who, under Bunsen's guidance, brought the investigation to a successful issue. The conditions most favorable to a reduction were carefully worked out. It had already been pointed out by Bunsen "that the density of the current (that is, the current per unit cross section) is the chief condition under which the electricity is able to overcome chemical affinities." This condition was fulfilled by using for the negative pole a very short length of thin harpsichord wire, upon which the reduced metal hangs in the form of molten beads, from which they can be quickly detached and plunged into petroleum. Another necessary condition is that the melting point of the electrolyte should be as low as possible, and this was attained by using a mixture of the chlorides of calcium and strontium, and by the addition of some sal ammoniac to the mass as the electrolysis proceeds. This subject was again further elaborated, in 1875, in the Heidelberg laboratory, by Hillebrand and Norton (Pogg. Ann., 1875 (156), 466), who prepared considerable quantities of cerium, lanthanum, and didymium in the coherent metallic state.

But the reduction of the metals was not the only important work which Bunsen got out of his battery, for quite early in its history it made its mark in organic chemistry. If by the electrolysis of caustic soda we obtain oxygen and a metallic radical, might not the electrolysis of an organic substance yield the corresponding organic radical? Doubtless a question of this kind presented itself to Bunsen's mind when he set his assistant, Kolbe, to work on the electrolysis of acetic and valeric acids (Annalen, 1847 (64), 339; 1849 (69), 257). The results of investigations thus commenced and carried out, both by Kolbe alone and in collaboration with Frankland, and the still more prolific researches of the latter chemist, are matters of scientific history; it is, however, not so generally recognized that they owe their origin to the Bunsen battery.

In addition to the zinc-carbon-nitric acid battery, Bunsen also constructed a powerful thermopile of copper pyrites and copper (Pogg. Ann., 1864 (123), 505); and in later years a constant zinc-carbon-chromic acid battery (Pogg. Ann., 1875 (155), 232) so arranged that the zinc and carbon plates could be lowered readily into the exciting liquid, and thus the battery was easily put in and out of action. This he used for obtaining the spark spectra of the rare earth metals.

For the purpose of measuring the intensity of the light given off by carbon poles connected with his battery, Bunsen, in 1844, constructed his well-known photometer (Berzelius Jahresber., 1845 (24), 13). The essential feature of this apparatus is the "disk" of paper having a greased spot in the center, or having a greased circumference with an untouched spot in the middle. With regard to this, Elster

relates the story that when he showed this arrangment to the late Emperor Frederick, then Crown Prince, the Prince remarked: "For the first time in my life, I now know the value of a spot of grease." In the original Bunsen photometer a small flame burning in a closed box fixed on a pivot in the center of a long board illuminated the back of the disk, the relative luminous intensity of the two sources of light under examination being ascertained by moving them alternately backward and forward on each side of the disk until, in each case, the spot disappeared. This form of the photometer was afterwards modified by other observers, not, according to Bunsen, to its advantage, by omitting the small flame and box and simply moving the disk backward and forward between the two fixed sources of light. Recently, Preece has proposed to reintroduce the principle of the original Bunsen arrangement of ascertaining the relative luminosity by always exposing the same side of the disk, and therefore eliminating the error arising from its translucency. In one form or other, the Bunsen photometer has, however, for many years been in general use, but recently it has been partially replaced by the shadow photometer.

In this connection mention must be made of two important researches of a physical rather than of a purely chemical nature, and characteristic of the manipulative as well as of the intellectual power of the author. They refer to the ice and the vapor calorimeters.

As by means of his battery it was possible for Bunsen to prepare small quantities of the rare metals, so by help of his ice calorimeter (Pogg. Ann., 1870 (141), 1) he was able to ascertain one of their most important physical properties. It was constructed in order to be able to determine exactly the specific heats of substances which could only be obtained in small quantities, and to which the usual calorimetric methods were therefore inapplicable. Thus it became of the greatest theoretical importance to ascertain the specific heat of indium, of cerium, lanthanum, didymium, and germanium, and other metals which are only obtainable in small quantities. The principles of construction and mode of action of the ice calorimeter are so well known that a description of the instrument and its use is here superfluous. They were published in 1870, and by its means the atomic weight of indium and the formulæ of its compounds were rectified, while the doubts arising as to the formulæ of the compounds of other metals were eliminated.

"Thus Bunsen largely contributed to the confirmation and to the acceptance of the system of atomic weights now in use, and thereby to the rational classification of the elements depending on that system."[1]

[1] Stanislao Cannizzaro, Commemorazione del Soci W. Bunsen, Rendiconti d. R. Acad. dei Lincei, 8 December, 1899.

In 1887, when 76 years of age, Bunsen published the description of a new vapor calorimeter (Ann. Phys. Chem., 1887 (31), 1) upon which he had for some time been engaged. It depends on the same principle as the one previously constructed by Joly (Proc. Roy. Soc., 1886 (41), 352). The body whose specific heat has to be determined is hung by a fine platinum wire to the beam of a balance, then brought into saturated aqueous vapor at 100°, and the amount of water deposited on the body while it is being heated is weighed in the vapor, this amount being directly proportional to the specific heat. This method gives very accurate results, and differs in some essential respects from that proposed by Joly. In this way Bunsen determined the specific heat of platinum at different temperatures, that of glass, and of water inclosed in glass. This latter he found to be 0.9992 (Joly obtained as a mean result 1.0062). The originality of this idea, arrived at quite independently from Joly, and the degree of accuracy with which the whole research is worked out, must indeed be considered as a wonderful achievement of a man close upon 80 years of age.

In addition to the work which Bunsen did alone, I am bound to refer to the long and difficult series of researches on the measurement of the chemical action of light, in all of which I was associated with him. (Pogg., 1855 (96), 373; Phil. Trans., 1857 (147), 355, 381, 601; 1859 (149), 879; 1863 (153), 139.) For this reason I feel difficulty in criticising it. This difficulty is, however, somewhat removed if for this lecture I simply quote the opinion of Richard Meyer as found in his Nachruf of Bunsen, with an extract from Ostwald's Classiker, with which he closes the notice, and as an illustration of Bunsen's literary style add a few sentences of the introduction which he wrote to the fourth part of our photochemical researches.

"The year 1855 was rendered especially memorable, as in that year the first communication appeared of the photochemical investigations which Bunsen carried out together with H. E. Roscoe. These researches are considered by Ostwald simply as 'the classical example for all further researches in physical chemistry.'

"The investigation is founded on the discovery by Gay-Lussac and Thénard of the action of light on a mixture of equal volumes of chlorine and hydrogen, in which an intense illumination produces an explosive combination, while with a less intense one the combination proceeds more slowly. So early as 1843 Draper had made use of this property for the construction of an actinometer, to which he gave the name of tithonometer. This, however, first became a reliable instrument in the hands of Bunsen and Roscoe. Equipped with this instrument, they have determined the most important laws of the chemical action of light after overcoming extraordinary experimental difficulties. Subsequently they replaced this apparatus, in consequence of the difficulties attending its manipulation, by the much more convenient chloride of silver actinometer.

"The first point determined was that the chemically active rays are

reflected and absorbed according to the same laws as the visible rays, and that their intensity diminishes as the square of the distance. The question as to 'whether energy is expended in the act of photochemical combination for which an equivalent amount of light disappears, or whether the action, like that of the liberation of a spring, is brought about by the chemical rays without any appreciable loss of light,' is decided in favor of the first view. The phenomenon is termed by the authors photochemical extinction.

"A second very remarkable phenomenon, first pointed out by the authors, is that of chemical induction. This refers to the fact that the action of light on the sensitive mixture of chlorine and hydrogen does not begin in its full intensity, but that it slowly increases, until after the lapse of a certian time it attains its regular and maximum rate. A satisfactory explanation, much less a theory, of induction is as yet wanting. Lastly, it was proved that photochemical action depends solely upon the quantity of the incident light, and is altogether independent of the time during which the insolation takes place.

"The great and important influence which photochemical action exerts in organic nature, especially in plant assimilation, renders the application of photochemical measurements to meterological and climatic phenomena of special interest. But the difficulties which surrounded such an application were enormous. In the first place, it was necessary to find a unit of absolute measurement for the chemically active rays. A flame of carbonic oxide which emits chemically active rays of great intensity, burning in air under carefully specified conditions, satisfied the requirement. It was found that whilst the variation of the chemical action of the light reflected from a clouded sky was subject to no recognisable law, that obtaining when the sky was cloudless and when direct sunlight was employed at once exhibited distinct relations. The curves of daily intensity thus obtained before and after noon were seen to be symmetrical throughout the day. In direct sunshine these curves, of course, rise much higher than is the case in diffuse daylight; moreover, the considerable variation due to change of latitude was precisely calculated.

"The dependence of the chemical action on the wave length of the incident light was carefully studied, the result being that the most intense action was exerted by the rays between the lines G and H of Fraunhofer; the curve falls sharply toward the red end of the spectrum, whilst it extends in the more refrangible portion far into the ultra violet. Strictly speaking, this only applies to the mixture of chlorine and hydrogen, still experiment has shown that the same thing is to some degree true of many other sensitive substances, although the distribution of the chemical activity in the spectrum is a different one.

"This short account of the photochemical researches is far from doing them justice. 'In no other research in this domain of science do we find exhibited such an amount of chemical, physical, and mathematical dexterity, of ability in devising experiments, of patience and perseverance in carrying them out, of attention given to the minutest detail, or of breadth of view as applied to the grander meteorological and cosmical phenomena of nature'.—OSTWALD."

And now comes Bunsen's introduction:

"The measureless store of energy which nature has amassed in the sun's body flows in an unceasing current as solar rays throughout the universe.

"The labor expended on the earth's surface in the maintenance of the animal and vegetable creation, and in the production of geological change, is derived almost exclusively from this source.

"Those of the sun's rays which vibrate most slowly and form the red portion of the solar spectrum, including the rays visible and invisible which surround them, give rise by their absorption more especially to the thermic actions observed on the surface of the earth and in both the fluid zones which as ocean and atmosphere encircle the solid crust of our planet. These rays constitute the sources of heat which in those grand processes of distillation and atmospheric deposit have effected these vast transformations of the earth's crust, by the study of which we obtain some idea of the immensity of the sun's action exerted during geological ages upon our globe.

"Of a totally different kind, on a scale less magnificent but not less important, are the effects mainly produced by the more highly refrangible and more rapidly vibrating portions of the solar rays. These rays exert the most marked influence upon the chemical changes on which the vegetable world depends, and are therefore of the greatest importance as regards the character and geographical distribution of organic nature.

"Although the atmospheric phenomena regulating the amount and distribution of the chemical action of light on the earth's surface have not as yet been systematized to the same extent as the thermic, electrical, and magnetic phenomena of meteorology, the reason is not so much that their importance has been overlooked, as that the difficulties which surround an exact investigation of the subject have up to the present time proved insurmountable. * * * The light which the sun radiates into space during each minute of time represents a chemical energy, by means of which more than twenty-five and a half billions of cubic miles of chlorine and hydrogen may be combined to form hydrochloric acid." (Phil. Trans., 1859 (149), 879.)

Of all Bunsen's researches, the one which will undoubtedly stand out preeminent as time rolls on is that on spectrum analysis.

The most important discovery made by Bunsen during the short duration of his residence in Breslau was the discovery of Kirchhoff, who was then professor of physics in that university, and whose great ability the elder man at once recognized. No sooner had Jolly removed to Munich in 1854 than Bunsen took care that Kirchhoff should be his successor in the Heidelberg chair of physics. And thus came about that great twin research which has made the names of these men known through the wide world. To dilate upon the importance of the discovery is unnecessary; to follow out the growth of this branch of science in its height and depth and breadth is here impossible. All that can be now done is to indicate briefly the origin of the discovery and to refer to a few points in Bunsen's work which are of special interest to chemists. To begin with, let me give you in Bunsen's own words the account of Kirchhoff's great discovery—namely, the full explanation of Fraunhofer's lines in the solar spectrum, pointing the way to a knowledge of the chemical composition of the sun and fixed

stars, and then of his own application of the principles of spectrum analysis to the examination of terrestrial matter.

In a letter to myself (facsimile, Pls. II–VII), dated November 15, 1859, he writes:

"At the moment I am engaged in a research with Kirchhoff which gives us sleepless nights. Kirchhoff has made a most beautiful and most unexpected discovery; he has found out the cause of the dark lines in the solar spectrum, and has been able both to strengthen these lines artificially in the solar spectrum and to cause their appearance in a continuous spectrum of a flame, their positions being identical with those of the Fraunhofer lines. Thus the way is pointed out by which the material composition of the sun and fixed stars can be ascertained with the same degree of certainty as we can ascertain by means of our reagents the presence of SO_3 and Cl. By this method, too, the composition of terrestrial matter can be ascertained and the component parts distinguished with as great ease and delicacy as is the case with the matter contained in the sun. Thus I have been able to detect lithium in 20 g[illegible]ms of sea water. For the detection of many substances this meth[illegible] is to be preferred to any of our previously known processes. Thus, if you have a mixture of Li, Ka, Na, Ba, Sr, Ca, all you need to do is to bring a milligram of the mixture in our apparatus in order to be ble to ascertain the presence of all the above substances by mere obs rvation. Some of these reactions are wonderfully delicate. Thus it is possible to detect five one-thousandths of a milligram of lithium with the greatest ease and certainty, and I have discovered the presence of this metal in almost every sample of potashes."

The following letter contains the first announcement of his discovery of cæsium. It was not until one month later (May 10, 1860) that the fact of the discovery was communicated to the Berlin Academy of Sciences:

"HEIDELBERG, *April 10, 1860.*

"MY DEAR FRIEND: Weltzien went to Paris a week ago and pressed me to accompany him, but unfortunately I was unable to free myself from work which I had postponed until the vacation, and so I have been obliged to forego the pleasure of seeing you in Paris and to tell you how much I have been pleased with your investigation. Do not be annoyed with me, dear Roscoe, that I have done nothing with our light investigation. I have left everythi g untouched, because I have obtained full certainty, by means of spectrum analysis, that besides Ka, Na, and Li, a fourth alkali metal must exist, and all my time has been occupied in endeavoring to isolate some compounds of the new substance. Where the presence of this body is indicated it occurs in such minute quantity that I almost give up hope of isolating it unless, indeed, I am fortunate enough to find a material which contains it in larger amount."

On November 6, 1860, Bunsen describes to me his further work on the new metal as follows:

"I have been very fortunate with my new metal. I have got 5 grams of the nearly chemically pure chloro-platinic compound. is true that this 50 grams has been obtained from no less than 40 to of the mineral water, from which 2.5 pounds of lithium carbona

1

Facsimile.

Heidelberg d. 15 Nov 1857

Meinen besten Dank, lieber Roscoe, für Ihren letzten Brief. Ihre Radiationsversuche erscheinen mir sehr wichtig und interessant. Daß bei unserer rotierenden Scheibe wirklich bedeutende Radiation stattgefunden hat, halte

2

und meinen letzten Versuchen
war aber die Dauer der Insolation
gegen die der Verdunkelung
~~[illegible]~~ nicht selten [illegible] wie $\frac{1}{500}$ Secunde
zu 20 Secunden. Nimmt man
nun an, dass die Induction
bei dem Chlorsilberpapier
<u>sehr schnell</u> überwunden
wird, so können dieselben
bei Ihren Versuchen nicht
mehr sichtbar werden,
während sie bei den
rotirenden Scheiben sicht-
bar werden mussten.
Mit dem, neuen Apparat,
von dem ich Ihnen schrieb

FACSIMILE OF LETTER OF R. W. BUNSEN, 1859.

3

und uns dieß Alles leicht
ermitteln lassen. Leider
ist das Wetter der Art, daß
ich alle Versuche genöthigt
bis auf beßere Zeit habe
aufsparen müssen. In den
Weihnachts- oder Osterferien
denke ich mit dem Apparat
alles erreicht zu haben, was
wir so lange vergeblich an-
strebt haben. Im Augen-
blick bin ich und Kirchhoff
mit einer gemeinschaftlichen
Arbeit beschäftigt, die uns
nicht schlafen läßt. Kirchhoff
hat nämlich eine wunder-
schöne

FACSIMILE OF LETTER OF R. W. BUNSEN, 1859.

ganz unerwartete Entdeckung
gemacht, indem er die
Ursache der dunkelen
Linien im Sonnenspectrum
aufgefunden und diese
Linien künstlich im
Sonnenspectrum verstärkt
und in linienlosen
Flammenspectren hervor-
gebracht hat und zwar
der Lage nach mit
den Fraunhoferschen
identische Linien.
Dadurch ist der Weg gegeben,
die stoffliche Zusammen-
setzung der Sonne und

FACSIMILE OF LETTER OF R. W. BUNSEN, 1859.

5.

der Fixsterne mit derselben
Sicherheit nachzuweisen, wie
wir SO_3 Cl etc. durch unsere
Reagenzien bestimmen. Auf
der Erde lassen sich die Stoffe
nach dieser Methode mit derselben
Schärfe unterscheiden und nach-
weisen, wie auf der Sonne
so dass ich z. B. in 20 Grm.
Meerwasser noch einen Lithion-
gehalt habe nachweisen
können. Zur Erkennung
mancher Stoffe ist diese
Methode allen bisher be-
kannten vorzuziehen.
Haben Sie ein Gemenge von
Li Ka Na Ba Sr Ca, so brauchen
Sie nur ein Milligramm davon

FACSIMILE OF LETTER OF R. W. BUNSEN, 1859.

in unseren Apparaten zu
bringen, um davon
unmittelbar durch ein
Fernrohr alle diese Ge-
mengtheile durch
bloße Beobachtung
abzulesen. Einzelne
dieser Reactionen sind
unerhört scharf. So
kann man noch $\frac{5}{1000}$
Milligramm Lithium
mit der größten Leichtig-
keit nachweisen. Ich
habe diesen Stoff in fast
allen Pottaschen aufge-
funden.
Kirchhoff und
alle Freunde lassen bestens
grüßen. Von Herzen
Ihr
R W Bunsen

FACSIMILE OF LETTER OF R. W. BUNSEN, 1859.

have been prepared by a simple process as a bye-product. I am calling the new metal 'cæsium,' from 'cæsius' blue, on account of the splendid blue line in its spectrum. Next Sunday I hope to find time to make the first determination of the atomic weight."

The rare combination of mental and manual dexterity characteristic of Bunsen is nowhere more strikingly shown than in the investigation of the cæsium compounds. From these 17 grams of cæsium chloride, obtained as above described, he not only succeeded in preparing and analyzing all the more important compounds, but in crystallizing the salts in such a form that he was able to determine their crystallographic constants and then to supply all the necessary data for fixing the position of this new element and its compounds in relation to its well-known relatives, potassium and sodium.

All the world knows that shortly after his discovery of cæsium the birth of another new alkali metal, rubidium[1] (Berlin Monatsh., 1861, (6), 273), was announced by Bunsen, and the application of spectrum analysis led, in other hands, to the isolation of thallium in 1861, indium in 1863, germanium in 1886, gallium in 1875, and scandium in 1879, but alongside of these came announcements of the discovery of other new metals whose existence was more than doubtful. Concerning these he writes to myself:

"The frivolous way in which new metals are now discovered by dozens and sent forth into the world duly christened is certainly no gain to science; only later inquirers will be able to decide what remains new and serviceable out of this chaos of material."

I may here remind you that cæsium is not only interesting as being the first metal to have been discovered by spectrum analysis, but because, even before Bunsen's discovery, chemists had worked with cæsium salts which they had mistaken for potassium compounds, so closely do the properties of the two metals correspond. Plattner, in 1846, analyzing a mineral from Elba termed "pollux," could not bring his analysis to add up to 100 parts and was unable to explain the anomaly. After Bunsen had established the existence of cæsium, Pisani, in 1864, took up the reexamination of the mineral and showed that the alkali metal was cæsium, with an atomic weight of 131.9, and not potassium, with one of 38.85, thus accounting for the missing percentage.

In the Christmas vacation of 1863 an extraordinary accident, illustrating in a painful manner the close analogy which exists between the properties of the potassium compounds and those of rubidium, occurred in Bunsen's laboratory. It is thus described in a letter from Bunsen to myself:

"For a week I have been in a very depressed and sad state of mind owing to a fearful misfortune which has taken place in the laboratory.

[1] Aulus gellius noctes Atticæ II, 26. "Rubidius autem est rufus atrior et nigrore multo inustus."

During my absence from Heidelberg in the Christmas holidays a man employed there in cutting wood, in spite of previous warnings, inexcusably took his little son with him into the laboratory and allowed him to run about without proper supervision. The child seems to have put into his mouth an iron tube which had been used for the reduction of metallic rubidium by heating the carbonate with charcoal, and in which the explosive compound carbonic oxide-rubidium had been formed. The result was that an explosion occurred, and although no mechanical wounding took place the child's throat and roof of its mouth were fearfully burned, so much so that it died within twelve hours. You can imagine how much I have been affected by this accident, although, heaven be thanked, no blame for want of caution can be attributed to me."

In 1875 (Pogg. Ann., 1875 (155), 230, 366) Bunsen published a long investigation upon the spark spectra of the rare earths. He had constructed, and describes there, a new and convenient form of carbon-zinc chromic acid battery which was sufficiently powerful to give a small arc light or to work a large induction coil, and could be put in and out of action, so that it was made ready for instant use by lowering the carbons into the exciting liquid. By the help of this battery Bunsen mapped the spark spectra of the rare earths, the separation of which has proved to be a tedious and very laborious piece of work. An accident, almost pathetic in its incidents and somewhat similar to the well-known accident which happened to Newton's manuscript, occurred to Bunsen. He had just completed the above-named research, and the finished manuscript lay upon his writing table. On his return from dinner one day he found the whole reduced to ashes. It seems that a spherical water bottle stood on his desk, and this, acting as a lens in the sunlight, was the cause of the disaster. Writing to me on June 3, 1874, he says:

"You have good cause to be very angry with me for not having answered your sympathetic letter before this; but I have not allowed myself lately to think of anything which would remind me of the loss of my burned research. * * * I had finished the editing of a memoir on a subject which had occupied me for three years, and was about to forward it to Poggendorff for publication, when on returning home the other day I found all these papers, which had caught fire during my absence, reduced to ashes. The photographs of the apparatus, the drawings of the spark spectra of the metals of the rare earths, to separate and map which had cost me untold trouble, all are burned."

With regard to this accident, Kirchhoff writes to me on May 22:

"The disaster of which you read in the papers really happened. The manuscript of a research at which he had labored for years, with maps of spectra, has been burned. He was, to begin with, much depressed, but his wonderful elasticity of mind enabled him to overcome his dejection, and he has already begun to replace what was lost."

This he continued to do, never drawing rein until the memoir was again ready for press.

The original views of Bunsen and Kirchhoff concerning the nature of the spectra of the alkali and alkaline-earth metals as examined in the flame of the Bunsen burner has since their time undergone considerable modification. We now know that while the spectrum of potassium, sodium, cæsium, rubidium, and lithium, produced when any compound of these elements is brought into the flame, is that of the metal, it is quite otherwise with the similar spectra of the alkaline earths, for if a bead of calcium, strontium, or barium salt be brought into the flame bright lines and bands are seen, characteristic indeed of the individual substance, but differing altogether from the spectra obtained from the above compounds at the high temperature of the electric spark. In the first case we are dealing with the spectra of a compound, whereas in the latter instance we obtain the line spectrum of the metal itself. Nor must it be forgotten that Bunsen was the first to point out that which has only in recent years been fully recognized, namely, that change of physical condition under which a spectrum is observed may give rise to fundamental changes in the character of the spectrum itself. It was in his research on the absorption spectrum of didymium (Pogg. Ann., 1866 (128), 100), carried out with minute care, that this point was made clear. In this he proved that, examined under a high dispersive and magnifying power, a crystal of didymium sulphate gives an absorption spectrum in which the dark bands vary in position and in breadth according to the position of the crystal in regard to its axes through which the light passes; that is, whether the polarized ray is ordinary or extraordinary. These changes, somewhat similar to those since shown to be effected by change of pressure under magnetic influence, or from change of temperature, have yet to receive a satisfactory explanation. To enlarge upon these matters is, however, beyond the province of the present address, suffice it to say that Bunsen's original investigation has opened out an unbounded field for research, the cul tivation of which has already yielded great results and will in future yield still greater ones.

Next let us turn to his celebrated researches on chemical geology, especially those concerning the volcanic phenomena of Iceland.

The only relaxation from his scientific labors which Bunsen throughout life allowed himself was traveling, and this he thoroughly enjoyed. During many autumn vacations I had the pleasure of accompanying him in rambles throughout Switzerland and the Tyrol. He walked well and had a keen appreciation of natural beauty, especially of mountain and woodland scenery, while he took great interest in the geology and physical characteristics of the districts through which he passed, and this it was that led him to turn his mind to chemico-geological studies. So early as 1844, in company with Pilla and Matteucci, he visited and carefully examined the Carboniferous deposits

occurring in the well-known fumerole districts of the Tuscan Maremma (Annalen, 1844 (49), 264), and in 1846 he undertook his journey to Iceland, where he spent three and one-half months, and the outcome of which was the well-known series of investigations on the volcanic phenomena of that island (Annalen, 1847 (62), 1; 1848 (65), 70). No doubt it was the eruption of Hecla in 1845 which served as the incentive to this expedition, for he desired not only to examine the composition of the Icelandic rocks, which are entirely of volcanic origin, but especially the pseudovolcanic phenomena, which present themselves in greater force immediately after a period of activity than at other times.

The expedition to Iceland was an official one, promoted by the Danish Government. Bunsen was accompanied by Sartorius von Waltershausen and Bergman, both colleagues at Marburg, as well as by the French mineralogist Des Cloizeaux. They left Copenhagen on May 4, 1846, reaching Reykiavik after a short but stormy passage of eleven days. The party spent ten days at the foot of Hecla, where Bunsen collected the gases emitted by the fumeroles and investigated the changes which these gases effect on the volcanic rocks with which they come into contact. Eleven more days were given to the investigation of the phenomena of the geysers, and at the end of August Bunsen left the island, having in the short space of about three months collected a mass of material the working up of which, as he writes to Berzelius, "will tax all my energies for some length of time," a prediction which was subsequently fully realized.

Connected with the Icelandic expedition the following story is told: Bunsen had made all his arrangements for the expedition; had packed all the apparatus required for carrying on an experimental research in those regions, but he had been unable to obtain from the Kurfürst of Hesse-Cassel, of whose civil service he was a member, leave of absence from his professorship, although the application had been made repeatedly. In this difficulty he appealed for help to a cousin who happened to be domestic physician to this prince, whose eccentricity was well known. The difficulty was solved as follows: The physician informed his royal highness that a cousin of his, who was professor of chemistry in the Marburg University, had conceived the wild idea of voyaging to Iceland, and tha
would inevitably lose l
quently he hoped tha
request. The result o
waited for were in Bu

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tigations on the compo
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laid the foundation of

which he therein expressed mark an era in the history of geological theory. It is now acknowledged that the idea which he was the first to propound, namely, the necessity of examining the chemical composition of eruptive rocks taken as a whole rather than the determination of their various constituent minerals must be carried out if we wish to come to an understanding as to their mode of formation. For this purpose he made an extensive series of complete analyses of the Icelandic rocks. And from these results he drew the remarkable conclusion that in Iceland, and probably in most of the larger volcanic systems, there exist two extreme types of rocks. One of these, richest in silica, is termed the "normal trachyte;" the other, containing less silica and naturally more basic constituents, is the "normal pyroxene." All the Icelandic rocks can be classed as being either one or other of these normal silicates, or as admixtures of the two. In order to account for these well-established facts, Bunsen supposed that the two normal types were separated out from the mass of molten silicate in the interior of the earth at distinct points; and he founded this supposition on the fact of the influence of pressure on the melting point. (Pogg. Ann., 1850 (81), 562.)

This had been independently pointed out by James Thomson, in 1849, as being a corollary of the mechanical theory of heat, and had also been experimentally verified by William Thomson (Lord Kelvin) in the case of water. Bunsen developed this point further by proving that, exposed to a pressure of 156 atmospheres, the temperature of solidification of spermaceti was raised from 47.7°, under ordinary atmospheric pressure, to 50.9°. As volcanic rocks must have been subjected to varying pressures amounting to many thousands of atmospheres, it is clear that the effect of such variation on the point of solidification of the rocks must be very considerable, and that where the pressure is less the composition of the crystalline mass would be different from that of the rock formed where the pressure is greater. This remarkable theory of the existence of two distinct types of rocks separating out from the same fluid mass has recently been supplanted by other views, but the facts respecting the composition of the eruptive rocks upon which the idea was based will ever remain not only a monument to the patience and perseverance of their discoverer, but

were conducted by Bunsen in collaboration with Des Cloizeaux. He first shows that the cylindrical shaft, which is no less than 74 feet deep and 10 feet in diameter, had been built up by the deposition of the silica which the water holds in solution, so that, in Tyndall's words, "the geyser is the architect of its own tube." Bunsen determined the temperature of the water contained in the tube a few minutes before an eruption, and found that in no part of the tube did the water reach its boiling point. The situation at which the temperature of the water most nearly approached the boiling point under the superincumbent pressure was about 30 feet from the bottom, reaching there 121.8°, whereas the boiling temperature was 123.8°, making a difference of only 2°. The question occurs, Why, under these circumstances, does an eruption take place? This is satisfactorily accounted for by the fact that, owing to the existence at the base of the geyser tube of volcanic vents, through which steam under pressure is passing, the whole column of heated water is lifted, so that while originally at a point 30 feet from the bottom the temperature of the water was below the boiling point, when it became raised through a height of 6 feet by the pressure of the issuing steam its temperature was 1° above the boiling point, the same being true for every point in the cylinder, and thus the ebullition gradually increased until at last it became eruptive. An experimental illustration of Bunsen's geyser theory is described by Tyndall in his well-known work.

The distinct shade of blue possessed by waters of the geyser led Bunsen to examine the color of distilled water (Edin. New Phil. Journ., 1849 (47), 95). For this purpose he inclosed carefully purified distilled water in a horizontal tube 2 meters long, closed by plate-glass ends, the interior of which had been blackened, thus showing that the absorptive power of water is exerted less upon the blue than upon the other rays of the spectrum, and explaining the blue color of certain lakes and rivers and the color of sea water as observed in the Blue Grotto of Capri. The differences in depths of shades of blue possessed by waters in various places are doubtless due to the variation in size of the suspended particles varying in their reflective power.

Of a totally different character was the next piece of work to which I shall refer; it related to the separation of the metals of the platinum group.

In 1868 (Annalen, 1868 (146), 265) Bunsen worked for some time on methods of separating the several metals contained in the residues left after the process of extracting the platinum as practiced in the imperial mint at St. Petersburg. He fully describes the somewhat complicated processes by which he effected these separations: (1) The elimination of platinum and palladium; (2) the separation of ruthenium; (3) the deposition of iridium and rhodium; and (4) the

chief aim of the research, the preparation of pure rhodium and its compounds.

In the course of these experiments Bunsen met with a singular and unexplained accident, which fortunately had no serious consequences. With reference to this he writes to me as follows:

> "It is still difficult for me to write, as my hands are not quite healed, but I can not longer delay replying to your sympathetic letter, as I fear you may be uneasy about me. The cause of the explosion is to me still quite inexplicable. I had prepared about a pound of the mixed metals rhodium and iridium by zinc reduction, and had dried the powder at 100° in a water bath, when, on lightly touching the finely divided metal, which was not quite cold, with my finger, the whole suddenly exploded with the energy of rammed-in gunpowder. This is all the more puzzling, as I have often rubbed a powder of the same metals violently in a mortar in similar quantities without any explosion occurring. I have also heated similar preparations to a redness in vacuo without any gas, and certainly without a trace of hydrogen, being evolved. My left hand, with the first finger of which I touched the mass, saved my eyes, as my face and eyes were only superficially burned by the flames which penetrated through my fingers. My eyes are, with the exception of singed eyebrows and eyelashes, unhurt, and so the explosion will luckily leave behind no serious traces."

In the preceding communication on the platinum metals Bunsen first describes the well-known filter pump which now bears his name. But in a later publication (Annalen, 1868 (148), 269) he gives further particulars of its construction and use. These are so well known that it is only necessary to say that it is, in fact, a Sprengel pump in which a column of 28 inches of mercury is replaced by one of 32 feet of water. In this way a flow of water down a pipe of the above length produces a vacuum perfect up to the limit of tension of the aqueous vapor, and under the diminished pressures thus brought about all the processes of filtration and of the washing of precipitates can be carried out with much greater rapidity and perfection than is the case when working under the ordinary atmospheric pressure. Here, as in all his published work, Bunsen is precise and exact. To show the timesaving value of the process he precipitates two equal volumes of chromium sesquichloride solution of known strength by ammonia; the one portion he treats in the ordinary way, the other by the filter-pump method, whereby he demonstrates that, treated by the latter process, the precipitate is completely washed in one-thirteenth part of the time needed by the old plan, while only one-fiftieth of the volume of wash water is required. Such filter pumps, furnished with mercury pressure gauges, are now found in every well-fitted laboratory.

A somewhat simpler form of filter pump, first described by Piccard (Zeit. anal. Chem., 1865 (4), 45), is, however, now also very generally employed. This consists of a short glass tube attached to the water tap, with an inner jet for the water and an outer air tube, the rapid

flow of water carrying down with it a sufficient volume of air—on the principle of the steam injector—to create a diminution of pressure, which, although by no means so great as that effected by the filter pump as described by Bunsen, is still sufficient for many purposes.

As another example of the far-reaching character of his work, a few words must be said about his experiments on the products of the firing of gunpowder.

The nature of the reaction taking place in the firing of gunpowder has attracted the attention of chemists from early years. The accuracy of the simple equation, which at one time was believed to express this reaction, was long ago rendered doubtful by the observations of Gay-Lussac and Chevreul, but the first exact investigation of the composition, both of the gases and of the solid products of the explosion, we owe to Bunsen and Schischkoff (Pogg. Ann., 1857 (102), 321). The points of importance which they ascertained were, in the first place, that a large number of salts, whose presence had hitherto not been detected, were shown to be normal constituents of the smoke and solid residue; and, secondly, that many other gaseous products besides carbon dioxide and nitrogen are formed.

The powder was burnt under ordinary pressure, and the maximum temperature of combustion as well as the maximum pressure were determined. Since 1858 other investigators have taken up this subject, especially Abel and Noble, Berthelot, and Debus.[1] All these elaborate and more recent researches bear out the conclusion arrived at by Bunsen and Schischkoff, namely, that it is not possible to give any simple expression for the reaction, the products not only being very numerous, but varying considerably in their proportion according to the conditions, especially the pressure and therefore the tem-

to the extraordinary luminous intensity of incandescent erbia, interesting as being the starting point for the enormous industry of the incandescent mantle. He also determines the melting points and the rate of volatility of many salts by means of small beads of material placed at the end of a thin platinum wire in the flame, the rate of volatilization being ascertained by the microscopic measurement of the diminishing diameters, in given periods of time, of the molten globules. He next details a variety of original and most ingenious methods of detecting minute quantities of the metals and nonmetals by the help of reactions effected in the flame. So delicate are some of these methods, as, for instance, that of the detection of gold, that its presence can be with certainty ascertained in one centigram of a sample of a tellurium ore containing only a few tenths of a milligram of the metal.

Another most characteristic contribution to analytical chemistry is the investigation of a method of general applicability, published in 1853 (Annalen (86), 265), known as the iodometric method and consisting of the volumetric determination of free iodine by means of sulphurous acid, for which has since been substituted the more stable sodium thiosulphate. This method, as every chemist knows, is not only largely employed in commercial analyses, as, for example, for the estimation of the amount of manganese dioxide in manganese ores and Weldon mud, but it also gives valuable assistance in the determination of interesting theoretical questions, as, for instance, by Bunsen in the separation of cerium and lanthanum, and in the estimation of the atomic weight of the former metal.

Bunsen also devoted much time and labor to the perfection and systematization of the processes of mineral-water analysis.

In 1871 he published a detailed account (Zeit. anal. Chem. (10), 391) of the methods of analysis which he adopted and their results in an investigation, made on behalf of the authorities, which had occupied him for some years, on the chemical and physical properties of the mineral waters occurring in various parts of the Grand Duchy of Baden. These results he afterwards published in pamphlet form. They certainly constitute the most complete series of mineral-water analyses existing, and serve as a model in this domain of analytical chemistry. It is interesting to remember that Victor Meyer, who ... rried out a large part of the experi-

the main features of Bunsen's sci-
my personal recollections to give
an he was, and how he lived and

as far back as the year 1852. In
roduced to him by Professor von

Mohl, the father of the late Frau von Helmholtz, who was then professor of international law in the University of Heidelberg. Bunsen had just been called from Breslau to fill the chair of chemistry at Heidelberg in succession to Leopold Gmelin, best known to English chemists as the author of the great handbook translated by our late editor, Henry Watts, and published by the Cavendish Society. I shall never forget the first sight of the man who afterwards became one of my most intimate and valued friends, and to whom I owe more than I can tell. At that time Bunsen was at the height of his powers, physical and mental. He stood fully 6 feet high; his figure was well knit and powerful; his manner was one of suave dignity, while his expression was that of great kindliness and of rare intelligence. Nor did this first impression of his bearing and character ever change, much less lose force. On the contrary, the more intimate became my knowledge, the more had I cause to respect and admire him. His was a heart free from guile, guiding a temper equable and amiable. During my long and intimate friendship I never heard him set down aught in malice or express more than a mild and good-natured remonstrance—as when, for instance, one of the "Practicanten" had adopted some faulty method of analysis, the master would remark: "Mein Gott, wie konnten Sie so was thun!" His genial, yet quietly dignified, manner placed strangers at their ease, at once inspiring confidence and commanding respect. All saw in him a man worthy of esteem and safe to trust, while those who were favored by his more intimate friendship knew that for true modesty and greatness of heart he was excelled by none; they feel that for them he was the "chevalier sans peur et sans reproche," and that his companionship, whether scientific or social, was something to be proud of, the recollection of which remains as one of the most fruitful as well as one of the pleasantest of their lives.

Considerate and generous toward the opinions of others, he held firmly to his own, which at times he did not fail strongly to express. Simple and straightforward, he disliked assumption and hated duplicity; single-minded and wholly devoted to his science, he abhorred vanity and despised popularity hunting. Indeed, of so retiring a disposition was he that it was difficult to get him to take part in public

distant cousin, "the Chevalier" Bunsen. "Did you ever complete, sir, your great work on God in History?" asked the lady. "Alas, no," replied Bunsen, "my untimely death prevented me from accomplishing my design." One of his assistants, engaged in rearranging the collection of specimens, came to him with a bottle containing quinine, and, wishing to find out whether the Geheimerath remembered the formula for the alkaloid, asked him for it; Bunsen, who was, however, not to be caught by chaff, replied, "Wozu denn, Herr Doctor, sind die Handbücher?" Like many men who are engrossed in their special calling, Bunsen was often absent-minded, and many good stories were current about the mistakes which he thus unwittingly made. He had a well-known difficulty in remembering names. One day a visitor called who he knew quite well was either Strecker or Kekulé. During the conversation he was endeavoring without success to make up his mind which of these two gentlemen was his caller. First he thought it was Kekulé, then he convinced himself that he was talking to Strecker. At last, however, he decided that it was really Kekulé. So when his visitor rose to take leave, Bunsen, feeling confidence in his latest conclusion, could not refrain from remarking, "Do you know that for a moment I took you for Strecker!" "So I am," replied his visitor in amazement.

His, too, was a most affectionate nature, and one may regret that this side of his character was never freely called forth by family life. For Bunsen, like Dalton, tried to explain this failing by saying that he could never find time to get married. And this loneliness, especially in later life, oppressed him, and he often felt his isolated position keenly. When bidding him good-by after my summer visits to him, he would smile sadly and remark, "Jetz verlassen Sie mich wieder in meiner Einsamkeit." The following extract, from a letter to myself, referring to a notice of his life and labors, which appeared with a portrait, in 1881, in the columns of Nature, indicates more clearly than any words of mine can do, this side of Bunsen's character:

> "The kind things you say of me in Nature touch me the more, as I see in them the faithful expression of your old true friendship for me, which is one of the great joys of my old age. When one arrives, as I shall do in a few days, at one's seventieth birthday, one has only to live through a short span of bodily and mental decay. Standing as I do at that period of my life, I feel as keenly as ever how modest and contemptibly small is the amount which I have added to the building of science. In the years which I am rapidly approaching, one lives more in the recollection of past happy days than in the present; and to the most pleasure giving of them belong those which for many years we spent in true friendship together."

As another touching illustration of his affectionate disposition, I may mention that when congratulated one day by a friend on his having ··ed some high mark of distinction, he remarked, "Ah, the only

value such things had for me was that they pleased my mother, and she is now dead!"

It may here be well to mention that, in the year 1881, a congratulatory address, accompanying a bronze statuette of Berzelius, from his old pupils, was presented to Bunsen on the occasion of the celebration of the jubilee of his doctorate.

A letter written to me on November 3 in that year indicates the feelings of regard and affection which bound together the professor and his students.

"MY VERY DEAR FRIEND: Please accept my most hearty thanks for all the kindness you have shown toward me on this occasion, which has been so exciting for me. Of all the friendly interest that has been shown, what gave me the greatest and most heartfelt pleasure were the congratulations to which your signature bore witness, amongst those of so many old friends, and of the donors of the beautiful and artistic gift which I received from the hands of Baeyer on the fiftieth anniversary of my graduation.

"I and all friends will be glad to see you. I was absent from here on my anniversary day, hoping in that way to escape all official notice, but on my return I found so many tokens of kind interest that I scarcely see how it will be possible for me to answer each one separately * * * , and so I am beginning to feel very much exhausted after all I have been through; I long most heartily for your friendly visit, which will be the best of medicine for me."

This flight of the principal actor in the scene is very characteristic of the man, and à propos of this, Kopp writes to me in January, 1882:

"We had expected you to be present at the jubilee. Bunsen had secreted himself with a few intimate friends in Gugenheim on the Bergstrasse; he had noted the locality of his retirement on a card, which, in case of your arrival, my wife was privately to hand to you. Bunsen took the unavoidable in good part, and not wholly without pleasure. He is very fresh and well, apart from his nearly permanent bronchial catarrh; he grumbles much, and is therefore perfectly normal."

his sovereign. To be used on such occasions only, he kept an "order" coat, a "frack" or tail coat, upon the breast of which he had stitched as many of the stars and crosses as it would comfortably hold. During the jubilee the Grand Duke held a court in the castle, and presentations were made. Bunsen, who had already paid his devoirs to the Grand Duke's party, expressed his unwillingness again to go through the necessary formalities, but after some persuasion on my part he consented, hoping, as he said, to conceal himself behind the crowd of officials and dignitaries of all sorts who thronged the hall in which the royalties were assembled. So we walked together up to the castle in evening dress, as the custom is, Bunsen wearing his "orders." The streets through which the procession of magnates was to pass were filled to overflowing by a good-natured crowd, no military or even police being present to clear the way, so as the royal carriages came up the steep road leading to the castle, a block occurred, and, as luck would have it, that containing the Grand Duke, the Duchess, and the Prince of Prussia came to a standstill at the exact point where Bunsen and I stood endeavoring to make our way through the crowd. The Duke at once recognized the Geheimerath, and beckoned him to come to the carriage, and there and then they had a friendly chat, and I had the honor of being presented. As soon as the cortege moved on I had a good laugh at Bunsen, who, endeavoring to escape from all notice and attention, was entrapped in this amusing fashion.

Let me next endeavor to give you a picture of the master working in his laboratory.

When he first came to Heidelberg, in the summer of 1852, Bunsen found himself installed in Gmelin's old laboratory. This was situated in the buildings of an ancient monastery, and there we all worked. It was roomy enough; the old refectory was the main laboratory; the chapel was divided into two; one half became the lecture room and the other a storehouse and museum. Soon the number of students increased and further extensions were needed, so the cloisters were inclosed by windows and working benches placed below them. Beneath the stone floor at our feet slept the dead monks, and on their tombstones we threw our waste precipitates! There was no gas in Heidelberg in those days, nor any town's water supply. We worked with Berzelius's spirit lamps, made our combustions with charcoal, boiled down our wash waters from our silicate analyses in large glass globes over charcoal fires, and went for water to the pump in the yard. Nevertheless, with all these so-called drawbacks, we were able to work easily and accurately. To work with Bunsen was a real pleasure. Entirely devoted to his students, as they were to him, he spent all day in the laboratory, showing them with his own hands how best to carry out the various operations in which they were engaged. You would find him with one man showing the new method of washing precipi-

tates, so as to save time and labor, or with another working out a calibration table of a eudiometer, or with a third pointing out that the ordinary method of separating iron from aluminum is unsatisfactory and carrying out a more perfect process before his eyes. Often you would find him seated at the table blowpipe—the flame in those days was fed with oil—making some new piece of glass apparatus, for he was an expert glass blower, and enjoyed showing the men how to seal platinum wires into the eudiometers, or to blow bulb tubes for his iodometric analyses. Maxwell Simpson, who worked with Bunsen in the fifties, tells me that one day he saw Bunsen blow a complicated piece of glass apparatus for a pupil, who quickly broke it; Bunsen then made him a second, which at once met with a similar fate; without a murmur Bunsen again sat down to the blowpipe and for the third time presented the student (who we will trust looked ashamed of himself) with the perfect apparatus. Then he would spend half the morning in the gas-analysis room, going through all the detailed manipulation of the exact measurement of gaseous volumes, and showing a couple of men how to estimate the various constituents of a sample of coal gas, and pointing out the methods of calculating the results, and then leaving them to repeat the processes from beginning to end for themselves.

His manipulative ability was remarkable; his hands, though large and powerful, were supple and dexterous. He was amusingly proud of having a large thumb, by means of which he was able to close the open end of a long eudiometer filled with mercury and immerse it in the mercury bath without admitting the least bubble of air, a feat which those endowed with smaller digits were unable to accomplish. Then he had a very salamanderlike power of handling hot glass tubes, and often at the blowpipe have I smelt burnt Bunsen, and seen his fingers smoke! Then he would quickly reduce their temperature by pressing the lobe of his right ear between his heated thumb and forefinger, turning his head to one with a smile as the "agony abated," while it used to be a joke among the students that the master never needed a pincette to take off the lid from a hot porcelain crucible.

Accuracy of work was the first essential with him; most of us learned for the first time what this meant. Six weeks' work was spent on a single silicate analysis, but most of us contrived to keep two such analyses going at once, while an analysis of coal gas occupied a week or ten days. Not that he was averse to quick processes; indeed, many of his own investigations contain novel proposals for shortening chemical methods, but this was never done at the expense of accuracy.

After having l[illegible] his methods of quantitative work, of silicate analysis, for exa[illegible]r having [illegible]gh a course of gas analysis, those [illegible]ready [illegible] less trained elsewhere were set [illegible] inv[illegible]har Meyer, who

worked at the next bench to myself, being a medical student, was set to pump out and analyze the blood gases; Pauli and Carius worked on gas absorption, employing for this purpose Bunsen's recently invented absorptiometer; Russell was set to work out a new method of sulphur determination in organic bodies; Matthiessen was put on to the electrolytic preparation of calcium and strontium; Schischkoff analyzed the gaseous products of gunpowder fired under varying conditions; Landolt had to find out the composition of the gases in various portions of a flame, and I worked by myself in one of the monk's cells upstairs on the solubility in water of chlorine when mixed with hydrogen and carbonic acid, the object being to ascertain whether this gas obeys the law of Dalton and Henry.

These are only some of the investigations on a variety of subjects carried on in the old monastery by Bunsen's pupils under his supervision, and they indicate only a tithe of his activity, for at the same time he was engaged in investigations of his own. He always had two or three on hand at once.

When Bunsen accepted the chair of chemistry at Heidelberg the Baden Government agreed to build him a new laboratory. This was accordingly done, the plans having been worked out by him to the smallest detail, and in the summer of 1855 the new laboratory in the Plöck Strasse was opened. The rooms were by no means so lofty as those of our more modern laboratories, and as students from all parts of the world streamed in in large and increasing numbers, the new building soon became inconveniently crowded, and many applications for working benches had to be refused.

Some short time before the opening of the new laboratory the town of Heidelberg was for the first time lighted with gas, and Bunsen had to consider what kind of gas-burner he would use for laboratory purposes. Returning from my Easter vacation in London, I brought back with me an Argand burner with copper chimney and wire-gauze top, which was the form commonly used in English laboratories at that time for working with a smokeless flame. This arrangement did not please Bunsen in the very least. The flame was flickering; it was too large, and the gas was so much diluted with air that the flame temperature was greatly depressed. He would make a burner in which the mixture of gas and air would burn at the top of the tube without any gauze whatsoever, giving a steady, small, and hot, nonluminous flame under conditions such that it not only would burn without striking down when the gas supply was turned on full, but also when the supply was diminished until only a minute flame was left. This was a difficult, some thought it an impossible, problem to solve, but after many fruitless attempts and many tedious trials, he succeeded, and the Bunsen burner came to light. On the theory of the Bunsen burner I need not detain you, for it has already been brought before the society in his

usually clear and masterly manner by our president (this journal, 1877, i (31), 627). I may, however, here remark that so general, indeed so universal, has the use of this become that its name and value must be known to and appreciated by millions of the human race. Yet how few of these have any further ideas connected with the name of its author.

Another discovery which early brought him prominently before the public was that of the Bunsen, or as he preferred to call it, the carbon-zinc battery, a description of which has already been given. The manufacture of either the battery or the burner might, had the inventor wished, have been so guarded as to bring in a large fortune. But Bunsen had no monetary ambition, although he fully appreciated the importance of applied science; and this is a fine trait in his character. He not only disliked anything savoring of money-making out of pure science, but he could not understand how a man professing to follow science could allow his attention to be thus diverted from pure research. "There are two distinct classes of men," he used to say; "first, those who work at enlarging the boundaries of knowledge, and, secondly, those who apply that knowledge to useful ends." Bunsen chose the first—perhaps one may say the higher—part, and the notion of making money out of his discoveries, or of patenting any of them, never entered into his head. As illustrating this habit of mind, I remember that once we were talking about a former pupil of his, of whose scientific ability he entertained a high opinion. "Do you know," he remarked to me, "I can not make that man out. He has certainly much scientific talent, and yet he thinks of nothing but money-making, and I am told that he has already amassed a large fortune. Is it not a singular case?" To which I replied that I did not find it so very remarkable.

In the new laboratory research work was carried on with even greater activity than it had been in the old one. My own work on photochemical measurements was first carried out in a darkened chamber under the slates, where the summer temperature was usually above blood heat, and afterwards in Bunsen's private room downstairs. Men whose names have long ago been household words with us came to work under the master. Baeyer carried out his early work under Bunsen's care, though after a time he left to work with Kekulé, who had just set up a private laboratory in the neighborhood. Lothar Meyer, Carius, and Landolt were continuing their several researches Dexter worked on the atomic weight of antimony, Holtzmann on the cerium metals, while Pebal, Erlenmeyer, Meidinger, Lieben, Barth, Moritz Hermann, and Lotz each published interesting communications; and Bahr, from Stockholm; Frapolli, from Milan; Pavesi, from Padua, and Lourenço, from Goa, were also occupied in research. Most of this work Bunsen had initiated; all he assisted by cooperation

and advice.[1] Then, in addition, there were the beginners, to the number of 60 or 70, all of whom were looked after by the professor, and with some of whom he would spend hours showing them how to detect traces of metals by aid of the "flame reactions," or how to estimate the percentage of dioxide in pyrolusite by his iodometric method. So from Bunsen all who had eyes to see and ears to hear might learn the important lesson that to found or to carry on successfully a school of chemistry the professor must work with and alongside of the pupil, and that for him to delegate that duty to an assistant, however able, is a grave error.

How, it may be asked, could a man who thus devoted himself to supervising the work of others in the laboratory—and who, besides, had a lecture to deliver every day, and much university business to transact—how could he possibly find time to carry out experimental work of his own? For it is to be noted that Bunsen never kept an assistant to work at his researches, and unless cooperating with someone else, did all the new experimental work with his own hands.

It is true that in certain instances he incorporated the results of analyses, made by a student whom he could trust, into his own memoirs; notably this was the case with the silicate analyses which he used in his chemico-geological papers, and with many of the examples given in illustration of some of his new analytical methods. Then, spending the whole day in the laboratory, he was often able to find a spare hour to devote to his own work of devising and testing some new form of apparatus, of separating some of the rare earth metals, or of determining the crystalline form of a series of salts.

Again the editing of the research, and the calculations, often complicated, which that involved, were carried on in the early morning hours. When, for four summers after the year 1857 I spent my vacations working at Heidelberg, I lived in his house, and although I rose betimes, I always found him at his desk, having begun work often before dawn.

Then, although he frequently traveled during the vacations at Easter and in the autumn, often, I am glad to remember, with myself as companion, he generally returned after a short absence to continue an unfinished, or to commence some new, research, and during these quiet days much work was done by both of us.

[1] During the twenty years following 1856 the following were among those who worked with Bunsen: Graebe, Ladenburg, Bütschli, Wichelhaus, Laspeyres, Richard Meyer, Victor Meyer, Crum Brown, Thorpe, H. Rosenbusch, Horstmann, Emmerling, A. Salkowski, Bunte, Guido Goldschmiedt, Gibson, Smithells, Michael, Zorn, Bernthsen, Königs, Treadwell, Herzig, Fabinyi, Wanklyn, Phipson Beale, Cartmell, Long, Schischkoff, Andrejeff, Beilstein, Filipuzzi, Schneider, Dollfus-Ausset, Kündig, Goppelsroeder, Mayboom, Nessler, Winckler, Rose, Lucius, Friedländer, L. Mond, Sprengel, Messel, and, lastly, Curtius, who at present occupies the chair of chemistry at Heidelberg.

I will now say a few words about Bunsen as a lecturer.

Bunsen lectured on general chemistry every morning in the week from 8 to 9 in the summer, and from 9 to 10 in the winter semester. The lectures were interesting and instructive, not from any striving after oratorical effect, or by any display of "firework" experiments, but from the originality of both matter and illustration. His exposition was clear, and his delivery easy, and every point upon which he touched was treated in an original fashion; no book, of course, was used or referred to; indeed, he avoided much consultation of handbooks, the only two which I have seen him occasionally turn to for the purpose of looking up some facts about which he had doubts were Gmelin and Roscoe and Schorlemmer. When occasionally one of the practicanten consulted him about a passage in some manual which appeared defective, he would laughingly remark that most of what is written in books is wrong.

The illustrative lecture experiments, which he invariably performed himself, were generally made on a small scale, were often new, always strictly relevant to the matter in hand, and never introduced for mere sensational effect. He paid much attention to these experiments, and after the table had been set in order for the particular lecture by the assistant, he would regularly spend half an hour, sometimes an hour, in convincing himself that all was in readiness and in rehearsing any experiment about the success of which he was not perfectly certain.

He used few notes, but it was his habit to write up any numerical data in small figures on the blackboard, and to refresh his memory with these when needed. When I attended the lectures in the early fifties, Bunsen used the notation and nomenclature of Berzelius, writing water $\dot{H}$, and alumina $\ddot{A}l_2$. Later on, he still employed the dualistic notation, writing $KOSO_3$, $HOSO_3$, for K_2SO_4 and H_2SO_4; indeed, I believe that he never adopted our modern formulæ or used Cannizzaro's atomic weights, although his determination of the atomic heat of indium and his work on cæsium and rubidium were amongst the most important contributions toward the settlement of those weights.

Bunsen did not enlarge in his lectures on theoretical questions; indeed, to discuss points of theory was not his habit, and not much to his liking.
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individuality and characteristics stand out in prominent relief compared with which such things as theories and formulæ were almost lost sight of. At a very early stage of the course the greater part of two lectures was devoted to an analysis of mercuric oxide carried out before the class in the most precise and painstaking manner. The whole was a masterly exposition of analytical method involving a detailed but most lucid discussion of the various sources of error and of the two ways by which accuracy may be arrived at, namely, either by reducing these errors to a minimum or by estimating them and making the necessary corrections."

Concerning this side of Bunsen's character, and on the influence exerted by his work on chemical theory, I can not do better than quote the judgment of Cannizzaro, who, in his eloquent éloge, uses the following words:

"Bunsen did not take any active part in the theoretical discussions which took place during that period of his scientific career, but he was not indifferent to the fundamental arguments of chemical science; that is, the atomic weights of the elements and the formulæ of their compounds. While controversy raged he was silently employed in collecting experimental data and teaching how these can be best obtained, in order to settle all pending questions. This was his true mission. And this he fulfilled admirably."

To this passage Cannizzaro appends a note, which is so characteristic both of the writer and of his subject that I venture to quote it:

"In 1860, while I was on my way to attend the Chemical Congress at Carlsruhe, which was convened in the September of that year by Weltzien, Wurtz, and Kekulé, I stayed for several days at Heidelberg, where I had an opportunity of discussing with Bunsen the questions which were to be raised at the congress, namely, the choice of a system of atomic weights of the elements, and of the notation of their compounds. I found him well informed as to my published views on these subjects, which he had discussed with his intimate friend Kopp. He was satisfied with the attempt to effect an agreement between the conclusions drawn from atomic heat, isomorphism, and from the application of Avogadro's theory, but he did not enter seriously into the discussion, and in conversation on the subject he immediately reverted to an enumeration of the new experiments which ought to be made in order to settle doubtful points."

Apropos of Bunsen's lectures, I may here relate a story which is characteristic of the man.

Although the motto of "Lehr- und Lern-Freiheit" is that of every German university, yet it is obligatory on all candidates for public appointments to bring up certificates, signed by the professor, of
…lance on specified lectures. Bunsen, considering this a matter of
…usually signed "Mit ausgezeichnetem Fleiss," without further
…. On one occasion, however, looking at the applicant, he
…d, "Aber Herr Dingskirch ich habe Sie in der Vorlesung gar
…" "Ja, Herr Geheimerath," replied the student, "ich

sitze aber immer hinter dem Pfeiler." "Ach da sitzen so viele," was the only remark vouchsafed by the Geheimerath, who at once filled in the schedule, "Mit ausgezeichnetem Fleiss."

In conclusion, I may remark that Bunsen's constitution was a vigorous one, and it carried him fairly well through a long life; still, continuous exposure to the fumes and vitiated air of the laboratory induced bronchial troubles, from which in later life he suffered considerably. Beyond one sharp attack of peritonitis when traveling with Pagenstecher in the Balearic Islands, I do not think he ever had a serious illness. His habits were frugal, the only extravagance in which he indulged being his cigars. Of these he consumed a fairly large number, always having one or a part of one in his mouth; but as he generally allowed it to go out many times before he finished smoking it, the time it lasted was much above that of the average smoker.

Although taking no active part in German politics, Bunsen was a stanch Liberal; and no one rejoiced more than he on the consummation of the unification of the German people under the headship of the Emperor William. He was, however, no admirer of Bismarck's régime. On Mitscherlich's death Bunsen received a very pressing invitation to become Mitscherlich's successor in Berlin. On this subject, he writes to me:

"Very liberal offers with regard to Mitscherlich's professorship have been made to me, but I have declined them, as I did not wish to belong to the regiment of Herr von Bismarck, or to start again from the beginning with chemistry, the position of which had there fallen so low. In addition to that, they have here complied with the wishes which I had before expressed, and have offered Kopp a professorship here, besides raising the fund of my institute by a thousand gulden."

In 1889 Bunsen retired from active university life, resigning his professorship, and therefore his official residence, and retiring to a pretty little villa in Bunsenstrasse, which he had purchased, where he spent the remainder of his days in quiet repose. His chief relaxation and enjoyment throughout his life in Heidelberg was to wander with Kirchhoff or Helmholtz or some other of his intimate friends through the chestnut woods which cover the hills at the foot of which the town lies. As the infirmities of age increased and his walking powers diminished, he was obliged to take to driving through the woods along the charming roads which intersect the hills in all directions. Writing became a difficulty, and in his latter days the news of him came to me through our mutual friends Quincke and Königsberger. One of the last letters I received from him is dated June 4, 1890:

"* * * I have been suffering for weeks from the after effects of influenza, and I am still so weak that I have to spend my days on the sofa, and have scarcely strength to walk the few yards to dinner at

the Grand Hotel. When I think that next March I enter on my eightieth year, I must resign myself to the fact that such a state of things is inevitable. My hearing, too, becomes more and more difficult, and my eyes are worse, so I have to deny myself all social intercourse, and only see now and then one of my old friends who comes to look me up. But in spite of all this, I can still feel the humor of life. This is, unfortunately, not the case with Kopp, who has just resigned his chair. He suffers constantly, but with his chronic hypochondriacal temperament he was unable to fulfil his professional duties, and feels very unhappy. I hope that in time he will resign himself to the inevitable. * * *"

Few men knew Bunsen so well or admired him so much as Leo Königsberger, the distinguished professor of mathematics at Heidelberg. The following appreciative remarks contained in a letter to myself on Bunsen's mental constitution seem to me so true that I make no apology for here quoting them:

"Bunsen did not possess a mathematical brain in the sense so splendidly illustrated in the cases of Maxwell and Kelvin. He had, however, a logical mind, enjoying the rational analysis of recognized truths, and was thus able, thanks to the wonderful intuitive power of a great scientific man, and thanks also to his æsthetic character, to grasp and to understand rather than to explain phenomena. These, therefore, were rendered evident to him, not so much by an exact intellectual process as by the evidence of the senses and by the gratification which their perception afforded. Quite otherwise was it with Kirchhoff, as he entered frequently and with zest even into unfruitful mathematical or philosophical speculations. It was always interesting to listen to these two remarkable men dispute about some mathematical, scientific, or philosophical subject. Still more interesting was it, however, to watch, when he was present, the incomparable Helmholtz looking silently on, from his calm Olympian heights, with an appreciative but meaning smile as the discussion proceeded."

But although Bunsen was not a mathematician as compared with the men mentioned above, he not only possessed great mathematical ability, but, what is more important, the power to apply mathematical treatment to chemical problems. He constantly pressed upon all his pupils the necessity for a chemist of a thorough training in mathematics and physics; indeed, I have heard him exclaim, "Ein Chemiker der kein Physiker ist, ist gar nichts."

Bunsen at the time of his death had been for many years our senior foreign member, having been elected on February 1, 1842, during the first session of our society. It was not until 1858 that he became a Foreign Fellow of the Royal Society. In 1860, the Copley medal was awarded to him, and in 1877 he and Kirchhoff were presented with the Davy medal, being the first occasion of its award, in recognition of their researches and discoveries in spectrum analysis.

With respect to the award of the Davy medal, Bunsen writes to me, on November 10, 1877, as follows: "My best thanks for your

friendly letter with the news of the very unexpected distinction which has been conferred upon me. I received it almost simultaneously with the official announcement from Williamson, and I am indeed quite confused by so much kindness from my English friends."

Another English honor conferred upon him was that of the award, in 1898, of the Albert medal of the Society of Arts, given for "distinguished merit in promoting art, manufacture, or commerce," in recognition of his numerous and most valuable applications of chemistry and physics to the arts and to manufactures.

Almost up to the last Bunsen continued to take a vivid interest in the progress of scientific discovery, and, though suffering from pain and weakness, ever preserved the equanimity which was one of his lifelong characteristics. Three days before his death, so Quincke writes to me, he lay in a peaceful slumber, his countenance exhibiting the fine intellectual expression of his best and brightest days. Thus passed away, full of days, and full of honors, a man equally beloved for his great qualities of heart as he is honored for those of his fertile brain, the memory of whom will always remain green among all who were fortunate enough to number him among their friends.

[Before commencing the lecture, Sir Henry Roscoe read the following telegram from Dr. Philipp Bunsen, of Marburg, the nephew and executor of the late professor: "On the occasion of the memorial lecture the Bunsen family joins sincerely with the illustrious society and sends respectful thanks and compliments."]

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